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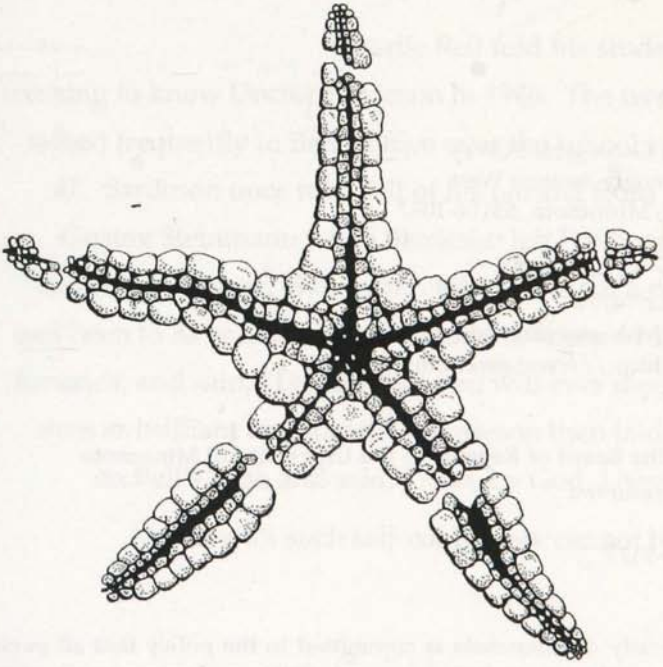
FREDERICK WILLIAM SARDESON, GEOLOGIST 1866–1958

MALCOLM P. WEISS



150 YEARS
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Title page: The Ordovician starfish species *Protoplaester narrawayi* as described by F. W. Sardeson in 1928. Redrawn by Mark A. Jirsa, Minnesota Geological Survey, from the original sketch.

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For Mary

Charlie Bell told his students of his coming to know Doctor Sardeson in 1946. The two met and talked frequently in Bell's office over the school year 1946–47. Sardeson once told Bell of his parting from Professor Gustav Steinmann when Sardeson left Freiburg with his new Ph.D. degree. Steinmann told him what a pleasure it had been to have Sardeson as a student, wished him well in America, and said, "I doubt that you will ever meet another man as brilliant as yourself." Sardeson then laid his hand on Bell's desk and said, "And, by God, I never have!"

A man with such self-confidence cannot be all bad!

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FOREWORD

The following is here because of what Charlie Bell said about Sardeson. Sardeson's reply is also interesting, and so typical of the man. The chapters that follow are intended to accomplish Bell's wish.

January 29, 1949

Dr. F. W. Sardeson
3942 Marine View Drive
Seattle 6, Washington

Dear Doctor Sardeson:

During the past year I have, as time permitted, read many of your papers and referred frequently to specimens in your collection. My sincere admiration for your paleontologic and stratigraphic work prompts me to tell you so, and to express my regret that you are no longer here where I can ask your advice. I learned much from our conversations, and am sorry that your visits came to an end.

This quarter in my course in Stratigraphy, in order that my students might have a little practice in writing a short paper, I assigned as topics an evaluation of the major contributions of some 40 so-called "famous" paleontologists and stratigraphers. In doing so I arbitrarily selected those no longer living, with one exception - you. My reasons were manifold, but one of them was that the reading of many of your papers would stimulate a very promising student to do graduate work in the Minnesota Ordovician.

However, I suppose my main reason was the hope that, on behalf of the student, I would have the nerve to make of you a very personal request that I have wanted to ask for the past two years. You have outlived most if not all of your contemporaries, Doctor Sardeson, and the question I raise is who will there be to write, when the time comes, a memorial that does justice to your geological contributions? Consequently I now suggest that you write a memorial as you would like it to appear, send it to me, and I will do all in my power to see that it is published as you write it. My principal fear is that some biased person will be called on to do the job, and will take the opportunity to do some backbiting. Perhaps you don't care, and possibly feel that I am being presumptious [*sic*], but I do care because I believe you to be the outstanding paleontologist and stratigrapher of your period. Will you do it?

Very respectfully yours,

W. C. Bell

Sardeson replied on 3 February in a long letter that contained some biographical material, some paleontological suggestions, and the following by way of reply to Bell.

"You can do me the most good by finishing some revision that I was working on. In particular . . . *[the early bryozoans as corals, something that Bell would not have touched]*."

"When urged lately to write, and have published, my autobiography, I refused, because I do not care to whitewash bad characters, nor to tell the truth about them."

PREFACE

Frederick W. Sardeson was a highly capable, innovative, productive, and fascinating figure in geology, yet he never enjoyed admiration from most of his contemporaries or lasting distinction in his chosen field. Cut adrift from his teaching post in middle age, he had to live by nip and tuck thereafter. Nevertheless, his legacy of published geology is of interest still to students of the Lower Paleozoic rocks and fossils of the Upper Mississippi Valley, and to students of Ordovician fossils from several phyla.

This report attempts to explain something of Sardeson's qualities and behavior, to give balance to the record of his professional accomplishments, and to throw some light on the practice of geology in the half century prior to World War II.

How is it that one undertakes to study and report on such a person in detail? As a graduate student in geology at the University of Minnesota in 1949, I was assigned by W. Charles Bell to prepare a brief account of Sardeson and his contributions to the science of geology. Bell turned me on to Sardeson to help me prepare for my dissertation work on the litho- and biostratigraphy of the Middle Ordovician rocks of Fillmore County in southeastern Minnesota. Charlie Bell had told his students stories from his short acquaintance with Sardeson (1946-47), between Bell's arrival in Minneapolis and Sardeson's departure for Seattle, and I was able to recall those accounts. In addition, in 1949, I interviewed several persons who had known Sardeson. Among those were Frank F. Grout, recently retired from the Department of Geology and, as I now know, a major antagonist of Sardeson early in this century; plant ecologist William S. Cooper, who skirmished with Sardeson over the Anoka sandplain; and Frederick K. Butters, also of the Botany Department. Some biographical information came from the Minnesota Alumni office, and the President's office gave some details of his dismissal from the University. For the early orientation to Sardeson's work I also consulted some of his published works and the standard reference volumes.

Between 1949 and 1952, I used Sardeson's works on Ordovician rocks and fossils extensively, both in the field and in the office. So also did the U. S. Geological Survey personnel (e.g., A. F. Agnew and A. V. Heyl, Jr.) who were studying the geology of the Lead District centered on southwestern Wisconsin, at about the same time. They found some of his papers very useful and gained great respect for the man's work (especially in contrast to the work of some younger and better known geologists). My own studies showed that Sardeson was an interesting and creative person, whose work on the rocks and fossils of the Ordovician beds had laid a useful foundation for my own stratigraphic paleontologic work. As a graduate student I developed the urge to learn more about this remarkable man who had done so much, but had earned so little approbation for his efforts.

But young professors of geology, as I was in 1952, cannot divert and work with the history of the science; the Dean would be greatly offended and send one off without tenure. At last, now that I am old, gray, and deanless, I can return to the story. Regrettably, many of the actors in the pages of this story are dead, including Charlie, who died too soon at age 68, in 1979. Their absence makes it easier to write freely about them but diminishes the story, because most firsthand details and impressions are gone forever. If any young geologists read this account, I advise them to listen to the old ones, take notes, get a tape recorder, and do some oral history! They can put the results aside until, like me, they too are retired and no longer need to fend off a Dean or a President.

Tape recorders just didn't exist when I studied Sardeson nearly 50 years ago. Had his widow not thrown out virtually every piece of paper that he wrote or received, there would be a trove of material with which to work. Approaching an early retirement, I began searching for Sardeson's one remaining relative, his daughter Marion Sardeson Buyken, and in 1984 I found her. She has since been immensely helpful in my work and appreciative of my giving her a new understanding of her father. She had admired and loved him, but she did not know that he had done geologic work that others found useful; she knew only of his downfall and shame when he was run out of the University of Minnesota. She was delighted to learn, at last, that her father did good things in science, and it was my pleasure to share in her enjoyment of a part of her father's life that she had not known.

With his store of personal papers and mementos seriously depleted by Mrs. Sardeson's urge to clear the home of "junk," it might have been almost impossible to "get at" him through primary sources. Fortunately, he was an inveterate letter writer, nearly until his death, and he lived in a time when many people not only wrote letters but also kept those they received. Sardeson's numerous publications, together with his correspondence, make it possible to see the man, if not entirely to know him. Especially as he grew older, Sardeson had the quaint habit of writing often to friends (especially R. S. Bassler) on Sardeson's birthday, February 22nd, and giving a report on his health. Present-day users of e-mail will leave much less of a record for future historians to mine!

Many archives contained documents, and Marion gave me a number of Sardeson's personal items. The Smithsonian Institution Archives has correspondence of his in several collections, notably the Ray S. Bassler papers. The National Archives has many letters to and from Sardeson in various files of the U. S. Geological Survey's records. Papers of the Charles W. Jerome family (Sardeson's brother-in-law) and the Minnesota State archives are at the Minnesota Historical Society. Records of the University of Minnesota and Sardeson's numerous letters to university officials are in its archives. The papers of Charles Schuchert at Yale University contain

many items of Sardesonia. The abundant papers of Thomas C. Chamberlin, at the Regenstein Library of the University of Chicago, contain much early material regarding Sardeson. A number of pertinent letters, copied from among the papers of Frank Leverett, have come indirectly from the University of Michigan. Sardeson's correspondence with W. Charles Bell was left to me on Bell's death. Some letters are in the Kenneth E. Caster papers at the University of Cincinnati. A few items lie in the archives of the University of Wisconsin, and fewer still in the Department of Geology at the University of Iowa (its university archives received some records of the Iowa Geological Survey only in 1998). The Field Records Section of the U. S. Geological Survey Library, in Denver, holds some of Sardeson's field notebooks and the drafts of his geological folios. The collection on the history of petroleum geology, in the American Heritage Center at the University of Wyoming, contains many useful ancillary records.

ACKNOWLEDGMENTS

This story depends heavily upon documents from many sources, but it could not have been assembled without the help and advice of friends, former colleagues, and others who have made use of Sardeson's geologic work. My debts are numerous and large. The work has allowed me to renew professional acquaintance with a number of students of the Earth, particularly that small part in the Upper Mississippi Valley. Such renewal has been pure pleasure to one retired from nearly all active geologic work.

For his enduring encouragement and thoughtful criticism of multiple drafts of the whole, and for his confidence that Sardeson was worthy of thorough treatment, I owe much to my longtime dear friend, Allen F. Agnew. Improved clarity, continuity, and accessibility for nonspecialists were provided by Mary O. Furner, Professor of American History, University of California-Santa Barbara, to whom I am much indebted. Nancy S. Nelson, William F. Rice, and Robert E. Sloan have read all parts of the early drafts, suggested improvements, and supplied additional information. Several persons have given valuable help and guidance on certain chapters; these include Robert H. Dott, Jr., William M. Furnish, Alan S. Horowitz, Allison R. Palmer, and Herbert E. Wright, Jr. Nancy Balaban did a sensitive job of copy editing. G.B. Morey has an enduring interest in the history of Minnesota geology, and his support of this project has been most welcome.

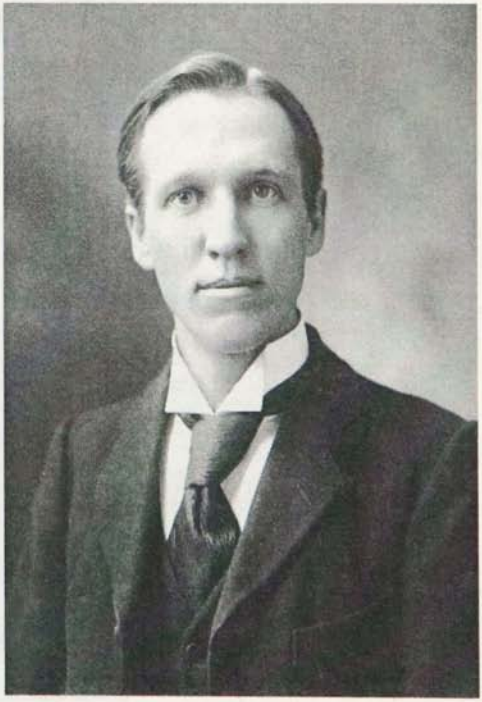
Many have shared their expertise on particular topics or sections. Robert L. Anstey, William I. Ausich, Rex E. Crick, Roger J. Cuffey, J. Thomas Dutro, Jr., Nigel C. Hughes, John Pojeta, Jr., and Peter J. Wagner advised on aspects of Sardeson's paleontology. Advice on stratigraphy was kindly provided by G.B. Morey, John H. Mossler, and Anthony C. Runkel. Henry W. Chaney, Roland Guyotte, Eric P. Hedblom, Gregory A. Ludvigson, G.B. Morey, Clifford M. Nelson, Eleanora I. Robbins, Thomas D. Rossing, and the late Curt Teichert have supplied critical pieces of geology, history, or genealogy.

Archivists at repositories have been unfailingly helpful with the discovery and copying of source materials. Penelope Krosch and Lois Hendrickson, of the University of Minnesota Archives, discovered many critical facts and sources and answered countless queries graciously. The late Marion Sardeson Buyken was helpful with this work in many ways that only a close relative could be. Electronic editing and preparation of copy, under difficult personal conditions, were made possible by careful and considerate instruction from Tracey J. Felger.

SARDESON IN PHOTOGRAPHS, 1890–1954

1890
Undergraduate
University of Minnesota
24 years of age

11 March 1898
Instructor of Paleontology
University of Minnesota
32 years of age



SARDESON IN PHOTOGRAPHS, 1890–1954

16 June 1903
Wedding Day
37 years of age

22 February 1916
Birthday Portrait
50 years of age

SARDESON IN PHOTOGRAPHS, 1890–1954

Date uncertain
Probably in his 50's.

22 February 1954
At home in Seattle
88 years of age

1

DEVELOPMENT AND HOPE

This is the story of, and an appreciation of, a remarkable man whose professional career and personal life were composed of two quite different phases. The first phase, 1892–1914, was one of accomplishment, success within and respect from the geologic community, great personal satisfaction, nearly 20 years of college teaching, and an enviable record of stratigraphic and paleontologic publications. The second phase, 1914–1940, was also one of accomplishment, with a large amount of published work, but in those later years Sardeson was ignored or regarded with tolerant amusement by many in the profession. He lived a somewhat bitter life apart, and did no teaching.

Frederick W. Sardeson, following his M.Sc. degree from the Department of Geology and Mineralogy at the University of Minnesota, was employed as student and teacher by that department from 1892 through 1914, except for his year in Germany. During that interval he accomplished a great deal of stratigraphic and paleontologic research, mostly on the Lower Paleozoic rocks of the Upper Mississippi Valley. In 1913 he was dismissed from the University, ostensibly because he "couldn't get along with his colleagues," and given severance salary through the year 1913–1914.

From 1913 to about 1919 he completed and published his parts of cooperative projects underway between the U.S. Geological Survey (USGS) and the Minnesota Geological Survey (MGS). From 1917 to about 1930 he worked for the Minnesota State Securities Commission, though his reports to it were neither published nor preserved in the state archives. From 1914–1940 he did some forensic geology and a great deal of independent geologic work in the Upper Mississippi Valley. That independent work was mainly on paleontology and stratigraphy; nearly all of his 91 papers from that period were published in *The Pan-American Geologist*, a maverick journal that was not taken seriously by many geologists in those years. By no means all of that journal deserved to be rejected, although much of it did. The journal was considered by many to report less than respectable science. Thus Sardeson's abundant later work suffered from the indifference accorded *The Pan-American Geologist* itself.

Sardeson's association with *The Pan-American Geologist* and his professional isolation have deprived his work (paleontology, stratigraphy, and glacial geology) of the balanced assessment that it clearly deserves. We need to know how and why his professional career developed as it did—in two phases. What was the innovative work of the first phase, and did the products of the second phase deserve to be largely ignored? This study attempts to fill these needs, but also to place Sardeson's unusual

career in the context of the geological profession of his day, and to shed some light on scientists' lives in America prior to the mid-20th Century. What does Sardeson's exile from the University of Minnesota tell us about academic politics and personnel policies before the establishment of safeguards such as tenure? What does it say about the potency of reigning orthodoxies or paradigms in the science, in relation to heterodox ideas? To what extent was the support structure of a university appointment indispensable for effective scientific work then? This account describes Dr. Sardeson's professional life and his contributions to the sciences of geology and paleontology to the degree possible this late in time. It also places him in the larger context of geology as it used to be practiced.

YOUTH AND COLLEGE

Frederick W. Sardeson was born 22 February 1866 in southern Wisconsin, at Owego Mills, Wiota Township, Lafayette County, the middle one of five children. His father Joseph Sardeson (1839–1914) was a miller from Dike,[1] a windmill and living quarters near Bourne in southern Lincolnshire, England. His mother, Petra Rossing Sardeson (1839–1929), came from a family of farmers living on the Pecatonica bottomlands in Wiota Township, who had immigrated from Lands Parish, Norway in 1857.[1,2] Joseph operated the Owego Mills, which was built in 1843, the first mill in the drainage of the East Branch of the Pecatonica River. The mill had two "runs" of stone and two houses.[3,4] Joseph ran it for his older brother James, who had married the widow of the previous owner and soon thereafter moved to the nearby town of Argyle, Wisconsin.[1,4,5] The mill site, located less than 3 miles south of Argyle, on Whiteside Creek at Wisconsin Highway 78, is near the East Branch of the Pecatonica River and close to outcrops of the St. Peter Sandstone and Platteville Formation; fossils from the Platteville Formation entertained young Frederick. These and associated Ordovician strata in the Midwest later occupied much of his professional life.

But Frederick's youth was largely spent in town, for James sold the Owego Mills in 1870 and Joseph moved his family to Argyle, a village of about 350.[5,6] There Joseph bought and ran a combination sawmill and gristmill in partnership with older brother James, and a drugstore in partnership with Dr. C. A. Hansen.[1] In 1886 Joseph moved his family to Minneapolis, Minnesota, apparently to improve the educational opportunities of his children; school teachers had recommended that Fred, as the family called him, go to college.[6] Frederick had enrolled in the Augsburg Seminary in Minneapolis in 1883,[3,6] where older brother Amos was already a student; Amos graduated soon after. Why the family chose Minneapolis rather than Madison or Milwaukee is not known, unless the mother's wish that the boys have a Lutheran education there prevailed.[6]

Frederick and younger sister Eva subsequently graduated from the University of Minnesota as members of Phi Beta Kappa.

As a boy Frederick enjoyed the sparsely settled southern Wisconsin country—fishing, hunting, swimming in the millpond and the Pecatonica River, and working at the mills.[6,7] He made a map of his vicinity, showing the best places to pick blackberries, nuts and other edible plants.[7] He had a "distinctly naturalist instinct,"[3] and he must have learned as a child to garden, for he was an inveterate gardener all his life, nearly until the day he died. He became skillful with tools and craftsmanship also, for he later made many toys and furniture for his daughter.[7] Few records or memories of his years in Argyle survive, but he probably helped at the mills and drugstore. He did acquire "a strong contempt for saloons and liars."[6] To judge from his later academic achievements, he must also have taken his schoolwork seriously.

In 1883, when Frederick was 17, he was sent to the Augsburg College and Theological Seminary in Minneapolis,[6] where Amos had gone. Both hoped to become lawyers, as Amos later did upon graduation from Augsburg.[1,3] Frederick took a classical sequence of work typical for the day, including Latin, English, Greek, German, and History.[3,6,8] He had grown up speaking and reading Norwegian and some Swedish, and he could read French and Italian.[7] When the rest of the family moved to Minneapolis in May of 1886, Fred switched to the University of Minnesota.[3] Although he should have been a freshman, during the 1886–87 school year he took "sub-freshman" classes to get Mathematics instead of Greek.[6] Taking German there as well, he then had knowledge or acquaintance of eight languages, but no geology.[9] At Minnesota he read Lyell's "Principles of Geology" on his own, enjoyed N. H. Winchell's natural history museum, and collected agates and fossils.[6,10]

In the fall of 1887 Frederick was enticed into geology by Christopher W. Hall, Professor and Head of the Department of Geology and Mineralogy (the "Department") at the University of Minnesota (the "University").[8] Surely Hall saw great promise in a bright young man with extensive undergraduate preparation; he designated him "Freshman Geologist" [6] and urged him to become a geologist instead of a lawyer. Sardeson began to catch up on geology and other science courses, and to help Hall informally. Even before he took classes in geology, he was collecting, sorting, and identifying fossils for Hall, E. O. Ulrich of the Minnesota Geological and Natural History Survey (the Winchell Survey), and C. D. Walcott of the USGS.[6] He joined the Cadet Corps and was a member for three years, promoted to corporal 1 October 1889 and to second lieutenant 1 October 1890.[3,11,12] In June 1889 he and Earle J. Babcock (much later dean and president of the University of North Dakota) were hired by Hall to collect fossils for Walcott in southeastern Minnesota;[3] they worked at this for six weeks.[8] Sardeson did similar work for Hall in 1890 and 1891 and for Walcott

again in 1891.[13] He also worked on fossils at the University.[3] He collected as far away as Cincinnati in 1891 or 1892, for he donated specimens from there to N. H. Winchell (Winchell and Schuchert, 1895). Sardeson took Hall's classes in 1890–91, mostly to learn how to teach geology,[3] and studied mineralogy and petrography on his own.[3] He graduated, B. Lit., from the University on 4 June 1891, having been elected to Phi Beta Kappa. That summer he was employed by the USGS for "a few weeks" to collect Trenton (Middle Ordovician) fossils in southern Wisconsin, where he had grown up.[14] He had already been enlisted by Hall for graduate work in geology.[3]

GRADUATE SCHOOL

Sardeson began graduate work at the University in the fall of 1891. Under the direction of Hall, he worked on the stratigraphy and paleontology of the Middle Ordovician rocks of the state. Publication of a long paper by the two of them (Hall and Sardeson, 1892b) shows that Sardeson had already been working at the professional level. One of the first master's students in the University, he completed requirements rather different from what we know today. A thesis was to be defended, but the thesis format, production of a small book as is familiar today, had not yet been instituted.[15] The earliest thesis volume on record is from 1896.[16] Instead, Sardeson completed four related projects that were accepted as fulfillment of the research requirement for the master's degree, awarded 2 June 1892. The project reports were "presented on May 13, 1892 . . . for the degree of Master of Science" and published as "Paleontological Papers" (Sardeson, 1892a, b, c, d).* At the December 1892 meeting of the Geological Society of America, in Ottawa, Canada, Sardeson was elected a Fellow of the Society (Fairchild, 1893).

Already achieving distinction as a young student, Sardeson continued his graduate studies. From 1892 to 1894, he held a "scholarship" in the Department of $350 for each year (10 months),[17] acting in a capacity that would today be called a part-time graduate assistantship.[3] Sardeson claimed this was the first such award to any graduate student in the University.[18] During 1892–94 Sardeson stayed in the Department to work toward a doctorate while holding the "scholarship," encouraged in this course by Hall. A "Committee on Graduate Studies to Degrees" oversaw graduate students and their programs in those years before the establishment of a Graduate School. The Committee consisted of six professors, including Conway MacMillan, the Botany teacher in the Department, in 1893–94, and the Committee kept handwritten minutes.[19] At a meeting of all six on 24 November 1893 the Committee admitted a number of students, including "Sardeson, Fred Wm. M.S. 92 for PhD."[19]

*Hereafter, Sardeson's publications will be cited only by the year and letter, as given in the bibliography.

At that time courses in Botany and Zoology were taught in the Department of Geology and Mineralogy; they were not separate departments. In fact, Hall had earlier taught all those subjects himself (Winchell, 1912). The list of graduate students in the University catalog for 1893–94[20] included "Sardeson, Frederick William, B. L., 91; M.S., 92. Paleontology, Geology, Zoology, Botany." Sardeson's examining committee in Minneapolis in 1894 consisted of Hall (geology), MacMillan (botany), and Henry F. Nachtrieb (zoology).[3] None of the three examiners ever held a doctoral degree. Three members of the Committee on Graduate Studies to Degrees, not including MacMillan, met 18 March 1894 and recorded:

> Com. to examine Frederick W. Sardeson, consisting of C. W. Hall, Nachtrieb & Macmillan [*sic*], reported that Mr. S. failed to show himself prepared for the preliminary examination.[20]

This rather cryptic statement is difficult to interpret, but it does not say that he failed the examination. Regrettably, no other documents are known that enlarge upon the story. As Sardeson explained the case, he

> came up for examination for the Ph.D. in 1894 . . . . One of the Faculty committee of examination, Prof. H. F. Nachtrieb [,] raised the question of "apostolic succession." None of the three examiners had a Ph.D. and he claimed they had no right to grant it to me: He won. I went to Germany next and by special, unanimous vote of the Faculty at Freiburg i. B., was allowed an examination [doctoral examination] at end of a second semester. But, those men [the professors at Freiburg] questioned closely before granting that permission.[8]

Whatever the case of the examination in Minneapolis in March, Sardeson's disappointment must have been enormous; nonetheless, he continued to work with and support Hall, on the record, as Hall supported him, until Hall's death in 1911.[21] Even years later, Sardeson also spoke favorably of MacMillan.[22] In 1944 he wrote that Nachtrieb had been "severe, but not necessarily unfair" with him.[23] Shortly after the March meeting of the examining committee, Nachtrieb urged Sardeson to attend the University of Chicago for his doctorate.[3] Probably in preparation for that, Hall (then also Dean of Engineering, Metallurgy and Mechanic Arts) wrote a letter of recommendation for Sardeson.[24]

In the spring of 1894 Thomas C. Chamberlin, spurred by the intervention of Chamberlin's friends at Minnesota,[25] invited Sardeson to apply to the University of Chicago. Although Sardeson had already made inquiry of Freiburg the previous January,[26] he did apply to Chicago[27] and was awarded a "Fellowship" of $320 for eight months of departmental

assistance and study.[28] After investigating the staff and program at the University of Chicago, he decided to go to Germany for completion of his doctoral work,[3] in part because President Northrup advised him not to go to Chicago.[29] Turning down the Chicago offer may have resulted in tension, even distrust, between Chamberlin and Sardeson. Although *The Journal Of Geology*, which Chamberlin edited, subsequently published some of Sardeson's work, Sardeson later offended Chamberlin, and the relationship ceased (see chapter on paleontology). At about that same time, Hall and Sardeson had a squabble with the Chamberlins—father T. C. and son Rollin T.—over a USGS folio (see chapter on glacial geology).

In considering study in Germany and then going there, Sardeson was very much in fashion for his time (Veysey, 1965). Graduate study in Germany was widely regarded as superior to that in North America, especially for research in the sciences. He was just ahead of the peak enrollment of Americans in German universities, which occurred in 1895/96 (Veysey, 1965). The historian of American higher education Lawrence Veysey also points out that some regional universities that granted the doctorate after only two semesters of study, as was true of Freiburg, were not well regarded by faculties of the better research universities in America. In the case of Freiburg, however, the requirements may well not have been insufficient, for Professors Steinmann and Weismann, who endorsed Sardeson, were internationally distinguished. Steinmann, in fact, later moved to the university in Bonn.

Accepted in January 1894,[26] Sardeson went to the Albert-Ludwigs University in Freiburg i. Br. (Breisgau; also i. B. [Baden], for Breisgau is a region of Baden). He left the United States on borrowed money[25] to work with Professor Doctor Gustav Steinmann.[26] Under a passport dated 25 June 1894,[30] he sailed in July.[31] In August, before his matriculation, Steinmann took him into the field to map loess deposits, which he did to the great satisfaction of Steinmann.[32] Later he had much success in a similar field venture with Professors Boehm and Steinmann together.[32] Having passed the special examination that would permit him to take the doctoral defense in his second semester, Sardeson, before classes began, attended the Sixth International Geological Congress at Zurich, where he read a paper for Warren Upham of Minnesota.[32]

Sardeson's formal course work at Freiburg extended from the late fall of 1894 to the late summer of 1895.[33] He took a variety of lecture and seminar series, eight in the "winter" term and five in the "summer" term. He studied invertebrate paleontology and petrology under Boehm; earth history, phylogeny of the invertebrates, alpine geology and glacial geology with Steinmann; and zoology with the great Professor Weismann. During the first term Sardeson carried out a local field project that may have been a test of his suitability for further work. He restudied the Dogger beds of the Upper Rhine Valley, and his report was published soon after (1895b).

His dissertation topic, the relations of the Tabulate corals to the Alcyonaria (1896c), was assigned to him by Steinmann.[34]

Sardeson enjoyed the work at Freiburg and completed it in a highly respectable manner. He described the pleasant and productive way the staff and students worked, in a series of rooms for study, the library, offices and laboratories, plus a common room for discussions; apparently everyone worked at similar times, if not actually in concert.[35] The certification of his final payments for lectures came 10 July 1895. The total cost of his matriculation, class fees and graduation fee was 560 marks, or about $140 at the rates then current.

He paid his graduation fee 11 July 1895[36] and on 5 August received a certificate[37] of completion of his dissertation and his doctoral examination (*multa cum laude*), the second highest of the four grades of completion awarded at Freiburg[38] (pretty good for not having been a native speaker!). This is the basis, and a valid one, for citing 1895 as the year of the doctoral degree, as he did in his professional listings and notices, and as in his memorial (Howell, 1960). His doctoral diploma (*examine multa cum laude superato*),[39] however, is dated 18 March 1896, because publication of the dissertation (1896c) within one year of the examination was a condition of granting the diploma.[38]

Sardeson was enormously proud of his German doctorate and often wrote that he was glad he had gone to Freiburg instead of Chicago; among other things it was cheaper. He learned at Freiburg that his German degree could be withdrawn at any time for unprofessional conduct.[40] As he believed in later life that many holders of American doctorates were shady characters, he regretted that American practice did not provide for withdrawing degrees awarded in the United States.

THE FACULTY YEARS

Sardeson's life and profession between 1895 and 1913 had clear focus and unity because of his constant association with the Department of Geology and Mineralogy at the University. During this period, he also accomplished most of his original work on the Lower Paleozoic formations and fossils of the upper Mississippi Valley. He moved up the academic ladder only to assistant professor, but he also did a great deal of consulting for government agencies and businesses. In this period Sardeson built a solid reputation for innovative work in stratigraphy, paleontology, and glacial geology.

Appointments

En route home from Europe Sardeson stopped in Lincolnshire, England, to visit cousins, the children of his father's older siblings who had remained in England. All eight of the male cousins whom he met on that visit were later killed in World War I.[7] By early fall he was back in

Minneapolis.[41] No longer a student, Sardeson took up a half-time appointment in the Department as Scholar in Paleontology at $250 per year (10 months). During the remainder of 1895 he was busy with work on the St. Peter and Oneota faunas that he had collected before leaving for Germany,[42] and his conclusions were soon published (1896a, 1896b). Between 1895 and 1905 he was advanced in rank, first to Instructor and then to Assistant Professor.[43] The progression of his academic appointments is shown in Table 1.1.

As the chronological summary clearly shows, Sardeson was evaluated and retained by the University numerous times over the course of a decade. Upon being promoted to Assistant Professor in 1905, Sardeson saw himself as "number two, in a department of five teachers."[43] The five were not all geologists until about 1908 or 1909, however, and Department Head Hall was, naturally, the "number one." Of course the academic ladder of the early 20th century was not the same as today's. A "professor" at the top of a broad pyramid of several instructors and assistant professors was a common model.

Sardeson was loyal to the University of Minnesota. About 1900, Sardeson was tempted briefly by the University of Washington.[7] Alden J. Blethen, a Regent of that University and publisher of the Seattle Times, was a journalist and editor who had worked in Minneapolis during 1884–1896.[45] The administration of the University of Washington was in moderate chaos, and the Geology Department was seriously understaffed in 1897–1899, although it grew very rapidly over the next several years (Barksdale, 1973). Apparently Blethen, an 1891 classmate of Sardeson, dangled a job there, but no action was concluded. Surely Sardeson would have been reluctant to trade the Ordovician beds with their abundant fossils for the volcanic rocks of western Washington. And in 1900 he felt that he was in an enviable position, one that would grow in interest and glory with the years.

These hopes were disappointed. After a number of years in which, by Sardeson's account, the University's plan for him (Hall's concept, surely) had been "to accumulate as much as practicable of partly finished work," in order to have problems available for graduate students,[46] the University betrayed him. The Vincent administration ignored the policy, and when Sardeson was later dismissed he lost access to the records of the collections and some of the collections themselves—he was "robbed." [46]

Philosophy of Geology and Education

Sardeson had very well-defined views on science, geology, and teaching—as on many other aspects of life. The consistency of the opinions over many years suggests that his professional values were formed during his years in college and graduate school, and thereafter firmly held. Surely he already had definite views regarding the high standards required for

TABLE 1.1. SUMMARY OF REGENTS' ACTIONS AFFECTING SARDESON.

[From a typed extract "Regents' actions re Sardeson," filed in the office of the Coordinator of the Academic Personnel System, University of Minnesota]

DATE	ACTION
4 Nov. 1895	Executive Committee (EC) of the Board of Regents (BoR) voted him "scholar in paleontology" at $250 per year.
1 June 1896	(EC) voted him "scholar in paleontology in relation to Geology & Biology" at $25 per month.
9 April 1897	"Petition of Mr. Sardeson for a salary of $1200 was laid on table" [by BoR].
13 April 1897	"Professor Hall being absent for a year on half salary provision is made [by BoR] as follows: [C. P.] Berkey $1000, W. [U.] S. Grant $500, F. W. Sardeson $500 if the President should find that Mr. Sardeson was needed for this work."
1 June 1898	". . . the Committee on Geological and Natural History Survey [of the BoR] made a report which was adopted as follows: Mr. Sardeson be appointed Instructor in Paleontology at a salary of $500."
6 April 1899	[BoR] "salary of F. W. Sardeson increased from $500 to $750." for the work of the [Wyoming] expedition covering July and August."
5 April 1900	"A communication was received [by the BoR] from Professor Hall relating to the work done by Dr. Sardeson and recommending his retention on the faculty."
5 June 1900	"The [BoR] Committee on Geological and Natural History Survey made a report including that 'application to continue service of Instructor Sardeson for another year be declined.' The EC accepted the report . . . except so much thereof as pertaining to Instructor Sardeson [,] was adopted."
23 April 1903	BoR voted to increase Mr. Sardeson's salary from $850 to $1000. [No changes between 1900 and 1903 are included in this extract of the record.]
4 May 1905	BoR increased his salary from $1000 to $1100.
? July 1905	Promoted to Assistant Professor. [44]
3 May 1906	BoR increased his salary from $1100 to $1200.
2 May 1907	BoR increased his salary from $1200 to $1400.
12 June 1907	[sic; probably a misprint for 1908] BoR increased his salary from $1400 to $1500.
21 Aug. 1908	"Wages for men doing field work in Geology authorized by BoR [to] be as follows: Professor Sardeson for actual time per day $5.00."
7 June 1910	"Report of the [BoR] Salary Committee on salary increases was as follows for Mr. Sardeson: 1908: $1500; 1909: $1500; 1910: $1500; 1911: $1700"

decent scientific work while an instructor at the University. He was also strongly partisan—people who did not share his views were suspect at best, and often condemned in vigorous terms. Even so, his conceptions of geology and science are interesting and sound; indeed they are quite modern in tone. His own words are the plainest way to tell his story.

> In looking back over 60 years as geologist, I see first of all how the pleasure of exercising competent perception and comprehension in research led me into Science instead of into Law as intended.[47]
>
> Science demands honesty, ability to perceive and experience on which to base comprehension . . . . If Earth was mathematic, i.e., a perfect spheroid, there would be no Geology . . . . Geology is the study of Earth's imperfections and their successive consequences.[48]
>
> Geology is highly educational to any number of persons, and of professional use to few . . . . Educationally, Geology fits in with History and Philosophy . . . .[22] [Geology can lead] . . . to willingnes [sic] and ability to think logically, both widely and deeply, and to increased ability to comprehend . . . . The end and aim of education is comprehension.[43]

Speaking of conditions in the middle of this century he wrote, "young geologists are not required to be educated, to think."[49] Implicit herein was his underlying belief that the program in Geology at the University had been commercialized. Sardeson considered geology a branch of humanistic studies first and foremost; only secondarily, later in the student's career, should the emphasis shift to professional practice. Chiding the President of the Minnesota Board of Regents during the years (late 1920s) when they were establishing The General College (for two-year degrees), he wrote

> The "sacrificing of quality for quantity" is freely admitted throught [sic] by the best and most valuable of the professors. Mixing screenings with the wheat, affects the product in the same way. Other state Universities all do it. Endowed universities are getting the advantage of the Market for product both on quality and "rep" and will increasingly do so until there is a change of policy in the state schools.[50]

The problem he inveighed against is, of course, still with us, in a more pervasive form. Not content just with defining reality for University regents, Sardeson frequently expressed to correspondents strong views about the practice of science as he saw it. "The intent of the Sciences is

(or should be) to find facts and the truth," he ardently insisted, "and therefore dishonesty in Science is treason."[47]

Teaching Methods and Program

Sardeson was very proud of his teaching; he enjoyed it and believed he was effective. Education, in contrast with training, was to be the special province of colleges and universities, in his view, and he tried to emphasize the former. Although he did not define the two precisely, his language makes the distinction rather clear. Education is designed to produce understanding in the independent thinker, whereas rote learning is designed to inculcate unthinking behavior, as exemplified in the selected quotations reproduced here.

> The basis for education is natural intelligence and the aim is comprehension. Schools must select the best intelligence and promote the fastest practicable progress in quantitative and qualitative learning in all grades of teaching. Those pupils who cease in intellectual capacity for learning by showing no expansion, at any grade, should then be given training only thereafter.[51]
>
> The aim of training is for efficiency, but training is habit-forming and as such it hinders or prevents education, because trained minds prefer to think along the lines as trained rather than make the effort to comprehend, even if [further] comprehension in a case is [still] possible . . . to that mind.[52]

When he started teaching in 1896, he believed that geology was "taught backwards," so he reversed it to "educational first, [with] training secondary and service to Geology [the Department and public] third, if at all."[43]

Sardeson's fullest description of the organization and teaching of a geology class seems quite modern.[53] Yet it is easy to imagine that it may have seemed unorthodox—even dangerous—100 years ago.

> As a teacher in Geology, I made [a] reputation for myself, as of the best. I did it this way, -
>
> 1st. When either of the others was absent, I could take over any class and do his way as well as he did, in the Department. That attracted students to me;
>
> 2nd. In my own classes, I used the rule that students may change registration within the first ten days (= two weeks). I gave three lectures and then asked whether anyone found it "hard." If so I advised his changing registration. That avoided "flunks."
>
> 3rd. I would say, If you don't know, then guess! Four times out of five they guessed right (Subconscious). That gave them confidence in me and in themselves.

4th. For one (or two or three) lectures, I am giving a great lot of details which must be remembered, to reach comprehension of an idea (or principle). "Then you may safely remember that idea, only." (Forget details). That required them to think and comprehend.

5th. In elective courses, I took inventory for each student of his previous work in related sciences, by semester, (A. Smith, Z4, B3, C6, M7, Ph.0) [Zoology, 4 semesters; Botany, 3; Chemistry, 6; Mathematics, 7; Physics, none], and in oral quiz, I tempered the questions to the qualifications,—also in reading quiz papers. That allowed latitude to my work.

6th. Student's notebook was his affair, - not mine.

Of course, if some advisor or committee had been able to send me a bunch of dumbbells, I would have been sunk! I attracted native ability, as Geology and Paleontology requires, for advanced courses.[53]

Sardeson's challenging courses ranged widely over his discipline. He offered Historical Geology and six courses in Paleontology, more subjects than offered by any of the other four geology instructors.[54] Three of the six were year-long courses. Certainly not all of these were offered every year, especially not by someone on a half-time appointment. The following gives some idea of his teaching load.[55]

I formerly taught Paleontology, 1896 to 1913 in a University. I had the best of students to do with, — advanced students in Botany, Zoology and Geology, mostly graduate class, —who were interested in Organic Evolution or in Historical Geology. By lectures with demonstrations I gave them the fossils' point of view.

In one term of introductory, I taught all to evaluate for fossilization, for environmental effects, such as prosperity, pauperism, accident, etc., on individuals, for variability in heredity and how to determine species. Also how species originate (from few individuals out of a large and variable species, under new conditions) which is seldom seen in the fossils, in fact. Principles of Taxonomy were included.

Then (a) one year of lectures and illustrations on traced descent of fossil animals [comparison of typical fossils of the several geologic periods]. Or (b) one year on identification of faun[a]s (Historical), by lectures and laboratory; - and floras. Also (c) individual students, training in techniques.

Throughout Sardeson's tenure in the Department, Hall, who was also Dean of the College of Mines, Metallurgy and Mechanic Arts, kept

Sardeson out of the classes for Mines students, whom Sardeson considered the dregs of the engineering group.[6] Sardeson taught Geology to the Civil Engineers and Paleontology to "Academic and Graduate students."[6] He established Paleontology as a significant part of the University's curriculum in science with success, as he put it, "so great that President Vincent reported me . . . as one of the best teachers in the University."[56] "But all that was suddenly abolished as I left the University."[55]

Sardeson described his demanding pedagogical methods, allegedly learned from Hall, and urged that training of geologists be limited to a few, chosen for aptitude in science and geology (1924d). He scorned departments that tried to train all comers and fit them for commercial work, for he thought that many students lacked the necessary educational foundation for advanced geology training, and that acceptance of masses made the student body and their department mediocre.

Seen through a colleague's eyes,[57] Sardeson's method was to "expose students to geology at their own option;" he didn't care whether they learned it or not. Further, displaying gender stereotypes characteristic of the male-dominated universities of his era, Sardeson contended that women liked to take paleontology because of the pretty "bugs." Sardeson compared his own earlier experiences with conditions 40–50 years later, to the advantage of the earlier times:

> It may be that teachers of sciences, even in Arts colleges and Graduate departments, are under educated trainers now, more than formerly. Research students themselves tend to become like hermits and troglodytes unless prevented from doing so! It is the Faculty's duty to prevent them from doing so.
>
> As a graduate student, 1891–4, I felt that drag, in research, although Geology and Botany departments were unusually advantageous against it then. I saw other graduate students affected by it and as a remedy, I organized a Fortnightly Scientific club with discussional programs for evening meetings. F. B. Sumner aided me greatly in it. As an educational influence, it was very successful and well attended.[58]

Sardeson always thought that the quality of geologic and paleontologic education at Minnesota had suffered after he left the Department. It serves no purpose to try to characterize the teachers there in the first half of the century, but it is true that the Department at Minnesota was very strong in economic geology and petrology, beginning with Emmons in 1911 and lasting through World War II. Sardeson believed that "training" had overtaken "education" in universities generally. He wrote as follows to the President of the Minnesota Regents:

> the prestige that derives from a reputation of having graduated from the University may be of as great or greater value to the person than are what he learned and the mental improvement from the studies. It is accordingly not an easy matter to turn away the student applicants who are not competent for a college education . . . . Failure to turn away such students, who have reached the limit of their educability, naturally results in damage to the prestige of the institution, however. There is then a temptation to the teachers to set up training courses, if not mere entertainments, which of course avoids external interferences and also delays on the other hand the awareness of the damaged prestige.[59]

Those who have taught through the years since World War II understand the validity of this observation and are keenly aware that the trend Sardeson deplored has intensified since he made this remark in 1946. His training/education dichotomy somewhat parallels the distinction often made today between the "grantsmen" and the teacher–researchers.

The program that Sardeson taught may have been excellent for paleontology and paleobiology, but it was also expensive in terms of the allocation of faculty time and the curriculum it required. So long as his patron Hall lived, he had a free hand with his courses. Hall died in May of 1911, and that fall the Department had new leadership, in the person of Professor William Harvey Emmons. If, as is suggested in the following quotation, Sardeson refused to moderate his course list and syllabi, he may have set himself on the path that resulted in his dismissal. "I became 'too valuable (commercially) to be allowed to teach Paleontology.' I refused to quit and the University discharged me; the Survey [Minnesota Geological Survey] dropped me. I was robbed. And there was nothing, I could do."[34]

Bighorn Basin Expedition

Yet while he labored in the academic vineyard, Sardeson had the opportunity for satisfying work. On 6 July 1899 the Executive Committee of the Board of Regents received a recommendation from Hall for an expedition to gather fossils in Wyoming. Expenses were "to be paid by friends in Minneapolis and St. Paul [Mr. Peavy, member of a milling family, and others]; the expedition to be in charge of Dr. Sardeson . . . . Rev. Dr. S. G. Small of St. Paul was made a representative of the University, accepting invitation of the N. P. R. R. [Northern Pacific, which paid all travel and shipping costs] for the excursion and $100 was allowed Dr. Sardeson as a salary for the work on the expedition covering July and August" (Table 1.1).

Sardeson led the expedition to Wyoming to collect vertebrate fossil material. It is apparent from the fact that he never published any scientific reports on the fauna collected that the main purpose of the expedition was

to collect samples for teaching and museum display. Sardeson was not a vertebrate paleontologist, despite some graduate work of that sort. Hall assisted Sardeson in securing support for the enterprise. Sardeson led the field party and was assisted by Messrs. Moore and Stewart, probably graduate students. The reason for the collection of bones, according to a report of the expedition probably recounted by Sardeson himself, was related to teaching: "They open the mind of the student to the facts and thus widen his horizon of thought and extend his sphere of intelligence. They are an invaluable attribute to any well equipped university."[60] Sardeson was keenly interested (1901c) in the details of preservation of fossil bone and the matter of preburial scavenging. He assembled at least one skeleton from his collections, a precursor of the horse; the disassembled bones are still in the Department. Oddly, Sardeson rarely mentioned the project in later years, although he did once refer to it as "the Peavy Expedition."[61]

The yield of this early expedition was rich. Sardeson reported to George Girty of the USGS that he had collected 2,000 lbs. of Upper Jurassic saurian bones and 1,000 lbs of Eocene mammal bones, free of matrix.[62] The fossil bones are still at the University and carry numbers, but no locality register has survived.[63] Professor Robert E. Sloan estimated many years later that the array of fossils included saurians, from the Jurassic Morrison of the Freezeout Hills north of Medicine Bow, as well as material of Wasatchian age from the Eocene Willwood Formation, from about 20 separate localities in the Bighorn Basin.[63]

Departmental Service and "Outside" Work

In addition to his teaching and research, Sardeson engaged in two other kinds of effort while at the University. "Departmental service" then was much as it is for academics now: public lectures to lay audiences, answering inquiries, and identifying specimens. The "outside" or "commercial" work that Sardeson was expected to do to supplement his half-time salary was what would today be called "consulting" work, for expenses and—sometimes—for pay. A review of salaries in the natural sciences during Sardeson's tenure there[64] shows that he was in the middle of the range. From about 1898 on, having his doctoral degree and publishing regularly as he did, it is reasonable that a median salary for Sardeson may have indeed been "half-pay," according to his view and that of President Northrup. Records do not permit a closer analysis of this matter. His teaching load appears to have been about "half-time" early on, but greater after about 1900.

Departmental Service. The few extant sources suggest that Sardeson was in demand for public lectures on geology, paleontology, and evolution. The latter was a particularly popular topic around the turn of the century. For many persons in those days, the doctrine of evolution as Darwin

outlined it did not carry the anti-religion "threat" that religious conservatives see in it today. People were often interested in learning the latest views, rather than attacking a supposed enemy, and many believers saw the hand of God behind the working of natural selection. Sardeson was himself a religious man and a regular attendant at the Congregational Church.[7] He tried to demonstrate that natural science (including organic evolution) and religion could be complementary rather than antagonistic (1902b), and he certainly did not subscribe to the "military metaphor" by which some expressed the relation of evolution to religion (Moore, 1979).

In his service role, Sardeson gave public lectures occasionally and organized geological walking trips, surely a popular Edwardian-era activity.[65] He tended to put notices of some classes and these service activities into *Ariel* and the *Alumni Weekly*. In 1907 he offered to lecture on "Evolution in Geology" in Owatonna, to a study group including his sister, for his expenses "at least."[66] At about the same time he gave that same lecture in the Twin Cities and scheduled another in Glenwood, both while preparing a paper for forthcoming meetings of the American Association for the Advancement of Science in Chicago.[66]

Sardeson donated some of the product of his field work to his alma mater. In 1904 he made a gift of fossils to the University, which was accepted with thanks by the Board of Regents.[67] The number and sorts of fossils were not specified in the minutes, but it surely was an important gift, as he was one of the very few serious fossil collectors in the state. Part of the Department's collection, these specimens were also lost in 1913–1914 [see Chapter 2].

Also in 1904, Sardeson was busy preparing a "book," probably on paleontology, for he reported preparing drawings and photographic figures with the help of his wife and having "finished to the 12th chapter."[68] He once wrote that he had compiled a textbook of paleontology for his class, but he decided not to publish it (1925d). Regrettably, nothing of it survives.

"Outside Work." Professors in the early 20th century performed many tasks without regular compensation. The Minnesota Geological and Natural History Survey (the Winchell Survey) had terminated 1 October 1900, coincident with the retirement of N. H. Winchell. The Minnesota Geological Survey, also an element of the University, was not established until 1911. In the interval, work on the botany and zoology of the state, which had been subordinated under Winchell, was emphasized in the departments of Botany and Zoology (Schwartz, 1964; Morey, 1988). Also during this period, cooperative work with the USGS was developed under C. R. Van Hise, chiefly on the Iron Range (Schwartz, 1964). During the time that the state was without a geological investigative and service organization, the Department undertook to provide such service to the public. "Persons coming to Geology for expert assistance were served gratis, for sake of the University and for our own enlightenment," Sardeson

recalled of this era. "Off-campus services were expected to be paid for, to the individual, at least as to field expenses."[3] The performance of ad hoc consulting on projects, apparently mostly mineral investigations, was to supplement a professor's teaching salary. Hall, at least, also did private work while on the teaching staff (Winchell, 1912). There was then no institutional support for research. This part-time teaching, plus outside consulting, was a subterfuge practiced by the administration to save money; today one must get grants to carry part of the salaries. Professor R.H. Dott, Jr. asks "What is new?"

To make matters worse, Sardeson's teaching load was increased by Emmons without any corresponding increase in pay (1912–1913), as Sardeson complained to President Vincent somewhat later.[69] "I worked for 15 years [instructor and assistant professor] on *half* pay, the agreement being that I would use the other time becoming a 'master geologist'" [by personal research and consulting work], Sardeson recalled.[29] [I was] "to use my facilities and earn a living commercially in Geology, which I did do!"[34] This arrangement ran routinely for a ten-month term each year.[70] Faculty members could consult during the summers as well. Sardeson carried out investigations for cities and towns: for example, he worked for a company in Red Wing that needed suitable clay for tile pipe and for the New Ulm Commercial Union in search of limestone. He also did some field work for the University under special arrangements.[69] He consulted for the Chicago Great Western Railroad for the summers of 1896 and 1897 [work important to glacial geology, as we shall see] in northeastern Iowa.[70,71] While doing so, he earned expenses and free passage on the railroad.[32,72] He studied "every well" owned in Minnesota by the Chicago Northwestern Railroad.[70] Sardeson would consult on any aspect of geology, and he had a good reputation; "lawyers recognized me as a very able witness in court," he could accurately boast.[3]

Actually, a "very good reputation in outside commercial geology . . . was dangerous. 'Business' wanted control of me, (in a corrupt way)," Sardeson understood.[34] Commercial interests, including some owned by members of the University's Board of Regents, expected him to "fudge" reports on lands and mineral properties, for payment. Sardeson studiously resisted such pressures. "I evaded Regent Strickler, who wanted a fraudulent report for a cement plant at New Ulm, by giving him nothing but the truth," he crowed on one occasion.[72] Sardeson was here more than a little hard on Dr. O. C. Strickler, a booster for New Ulm hoping for development. A few thin beds of Cretaceous limestone do lie in the New Ulm area, but they are hardly what is required to support a cement industry (Sloan, 1964).

In the early years of the century, when petroleum exploration was growing, Sardeson was told that he could earn great sums by making false reports for new oil companies, but he declined to indulge in such a practice.[3]

"If a person says that he is a scientist and then issues a knowingly falsified report he virtually commits perjury," he insisted. "I was asked to commit perjury in legal sense too . . . . Why disgrace my profession, - as I saw other 'geologists' doing."[34]

Through all his years in the Department Sardeson collected fossils persistently, at Hall's urging; Hall wanted him to collect and make exchanges while the "gathering and exchanging was good."[73] Between 1890 and 1896, all specimens were stored in the Department; from 1897 on [presumably until Hall's death in 1911], Hall requested that Sardeson keep newly collected and exchanged specimens at his home, against the day when a museum of natural history would be built by the University.[73] Hall expected that Sardeson would be reimbursed [presumably for field expenses] when "the museum" was built.

A museum was eagerly desired by all the natural scientists. It was expected also to house collections made by the Winchell Survey, but in this everyone was disappointed. A Dr. Elliott had encouraged the natural scientists to expect a museum that he would endow in his will.[3,74] The money came ultimately (1905) from his widow, with a request from the heirs "that this fund be used to erect a hospital" (Gilfillan, 1908). The Regents built Elliott Hospital, a superior academic choice, especially in light of the subsequent relationship of the University's Medical School to the Mayo Clinic. Sardeson, all the same, botanist MacMillan, and probably others never forgave the Regents, and MacMillan left the University in 1906 to go into private business.[74]

Relations within the Department

While Sardeson taught in Minnesota, he belonged to a department of moderate size for the day, and one that was highly stratified. In his 15 years as a regular faculty member, he had 13 geological colleagues, but only four or five at a time (Table 1.2). Hall, Winchell and Emmons were senior to Sardeson, although Winchell did no teaching during Sardeson's tenure in the Department. Of the other 10, only Sardeson advanced beyond the rank of instructor in that interval. Thus Sardeson was caught in a tricky position between elders and subordinates, most of whom served only briefly. Of those, Sardeson's fast friend Berkey had been a fellow student and was the first to complete a doctorate in geology at Minnesota. His colleagues during his academic years are listed in Table 1.2.

Sardeson had few close relationships with his colleagues. As we have seen, he was close to Hall. Berkey remained a close friend of Sardeson all his life, and Bowles was cordial to Sardeson in later years, long after Bowles had left Minneapolis. Sardeson had, of course, known Winchell from the late eighties, but had little to do with him early on. Only two others in Table 1.2 seem to have interacted with Sardeson: Emmons and Grout. Emmons engineered Sardeson's dismissal from the Department,

TABLE 1.2. EARTH-SCIENCE FACULTY OF THE DEPARTMENT OF GEOLOGY AND MINERALOGY, 1896–1913.

[Data from Schwartz (1972). Only the intervals coinciding with Sardeson's tenure are given here. The biologists are not included in the Table.]

Newton Horace Winchell—Professor of Geology and Director of the Minnesota Geological and Natural History Survey: 1896–1900; had taught in earlier years, but not in this interval (retired 1900).

Christopher Webber Hall—Head of Department; in charge of the teaching program: 1896–1910 (died 1911).

Frederick William Sardeson—Scholar, Instructor, Assistant Professor: 1896–1913 (sabbatical 1913–1914; not reappointed).

Charles Peter Berkey—Instructor: 1896–1903 (resigned).

Warren Upham—Instructor: 1896–1897 (resigned).

Arthur L. Parsons—Instructor: 1903–1906 (resigned).

Frank Fitch Grout—Instructor [Professor later]: 1907–1913 (retired 1948).

Edward M. Lehnerts—Geography Instructor [Assistant Professor later]: 1907–1913 (resigned 1919).

Andrew Walfred Johnston—Instructor [Assistant Professor later]: 1909–1913 (resigned 1920).

Oliver Bowles—Instructor: 1910–1913 (resigned).

Edgar K. Soper—Instructor: 1910–1913 (resigned).

William Harvey Emmons—Professor, Head of Department and Director of the Minnesota Geological Survey: 1911–1913 (retired 1944).

Ulysses S. Grant—Part-time assistant (and Winchell's son-in-law): 1903–1907 (resigned).

as we shall soon see. Grout supported Emmons; although references to Grout are scarce in Sardeson's many letters, he hated Grout with a passion.[7]

Sardeson's cordial relations with Winchell show that Sardeson could get along with colleagues, although Winchell was respected by and senior to Sardeson. "Prof. Winchell was hard to get along with and I was afraid of him," Sardeson recalled. "However, after I returned from Germany, he was very nice to me and in winter of 1895–6, my desk was in his Survey library. He asked my advice sometimes."[75] Although Winchell was a distinguished senior geologist and director of an outstanding state survey, he may have been a little awed by a live Doctor of Philosophy, the first in the Department. (The Department awarded its first Ph. D. degree, to C. P. Berkey in 1897.)

Sardeson respected Winchell for the massive administrative load he carried for 28 years, in addition to the teaching and geologic field work that Winchell himself accomplished. Winchell, like a number of other state geologists of the late 19th Century, notably John S. Newberry of Ohio, was severely criticized for spending state money on paleontology. As a dedicated paleontologist who understood the value of an accurate geologic time scale, Sardeson was sympathetic to Winchell in this regard.[34] He always believed that Winchell had been ill-served by the authors of the paleontology volumes of the final reports of The Minnesota Geological and Natural History Survey, particularly by Edward O. Ulrich.[34] He considered that Ulrich and perhaps others had named specimens rather than species. He also characterized Winchell as one of "our two best" in the Department [the other was botanist Conway MacMillan].[74]

Sardeson's Expectations for the Future

In the last years of Hall's leadership of the Department, Sardeson fancied that he might succeed Hall as Head.[76] Hall was ill in 1910,[42] and died 10 May 1911 (Winchell, 1912). Sardeson wrote his sister, "Between the private interests of some regents, some deans and of my colleagues the situation has been fairly stultifying at times. Yet I do not feel discouraged."[76] As it happened, an unusual conjunction of personnel changes that year was to dash Sardeson's hopes and alter his career forever. His patron died; his friend and supporter, President Northrup, resigned effective 1 April 1911 (Gray, 1958); and two new regents took office during the 1910–11 school year.[64]

Not having a crystal ball, Sardeson was hopeful about the new president, along with most of the faculty, because for some years the Deans (and not the President) had control of the curriculum and faculty salary scale.[76]

> There is general eagerness for the coming of President-elect Vincent, on April first. He will evidently have the solid support of the faculty members, especially that of young men . . . . The (new) President will resume control and the deans and regents must take their proper places, is the expectation.[76]

Sardeson's optimistic view of several unspecified opportunities he foresaw for himself ("as soon as Congress adjourns I may have some news for you."[76]) suggest that he himself shared the general enthusiasm about the coming of a new President. He also hoped that a new President might make him Head of the Department, but Sardeson was to be disappointed in this and other ways very soon.

NOTES

[1]Records of Sardeson and Rossing Families, told to Charles W. Jerome, husband of Joseph Sardeson's daughter Eva, a "few years" prior to Joseph's death, but not typed by Charles until 1920, 6 p. Eva was Frederick's younger sister [CWJ]. The boy was baptized Frederik William on 31 May 1866 at the Wiota Lutheran Congregation (doubtless his mother's church), but he never used this spelling and it occurs nowhere else in the records, [Pastor E. R. Peterson to the author, 27 November 1984].

[2]Rossing Family genealogical notes and tables—a gift of Thomas D. Rossing, Prof. of Physics, Northern Illinois University, De Kalb, Illinois. Sardeson's mother was a Rossing.

[3]Frederick W. Sardeson (FWS) to W. Charles Bell (WCB), Prof. of Paleontology, then at the University of Texas-Austin, 7 October 1956, [WCB]. Sardeson remarked in several letters over the years that he was apprehensive about his treatment at Chicago, but nowhere states the substance of his unease.

[4]Catherine Barry, *"Memoirs of the old gristmills in the Pecatonica River Valley: Owego, Argyle, Blanchardville, Moscow and Puddle Dock,"* Argyle, Wisconsin, 1951, 10 p.

[5]Story by Mrs. Orville Sardeson in the Argyle Atlas, in two successive weeks of September, 1956. Orville was a second cousin of Frederick and the grandson of his Uncle James.

[6]FWS to W. C. Bell, Prof. of Paleontology, then at the University of Minnesota, 3 February 1949, [WCB]. Sardeson's daughter Marion told me that he hated Augsburg, but that his mother wanted him to go there, [interview with author, 13 March 1987].

[7]Author's interviews and correspondence with F. W. Sardeson's daughter, Marion Petra Sardeson Buyken, 1984 to date; entries in *Who's Who, Who Was Who*, and obituaries in newspapers [FWS]. Also the Sardeson memorial in the *Proceedings of the Geological Society of America for 1959*, p. 143–146, [the date of his death given as 1959 in the title line is not correct; he died in 1958].

[8]FWS to Kenneth E. Caster (KEC), Prof. of Paleontology, the University of Cincinnati, 15 July 1940, [KEC]. Sardeson wrote that he read Lyell's *Principles* in 1886: FWS to R. S. Bassler (RSB), Paleontologist, U.S. National Museum, 21 December 1946, [RSB].

[9]FWS postscript No. 3 to RSB, 10 September 1944, [RSB].

[10]FWS to RSB, 21 December 1946, [RSB].

[11]Univ. of Minnesota Cadet appointment diplomas, 1889 and 1890, [FWS].

[12]FWS to RSB, 13 December 1957, [RSB].

[13]FWS to RSB, 27 October 1928, [RSB].

[14]Christopher W. Hall (CWH), Prof. and Head of Geology and Mineralogy, University of Minnesota, to FWS, 17 July 1891, [FWS]. See also USGS *13th Annual Report, 1891–1892*, p. 137.

[15]Lois G. Hendrickson (LGH), Assoc. Archivist, University of Minnesota, personal communication, March 1994.

[16]Penelope Krosch (PK), Archivist, University of Minnesota, personal communication, June 1983.

[17]John F. Downey (JFD), Dean of Science, Literature and the Arts, to President George E. Vincent (GEV), 26 April 1913, [PP].

[18]PK, personal communication, 1984. An "efficiency expert" employed by the University in the mid-1920s recommended discarding and burning a major fraction of its records. More recently, the Graduate School destroyed all files of graduate students prior to 1949 (see Note 15). See also Gray (1958).

[19]Minute Book of the University of Minnesota "Committee on Graduate Studies to Degrees," 1893–94, meeting of 24 November 1893, p. 3, [UOM].

[20]*Ibid.*, meeting of 18 March 1894, p. 5, [UOM].

[21]Sardeson was a protegé of Hall, both as student and colleague, and doubtless owed his job at Minnesota to Hall. The latter seemed always to put Sardeson forward; for example, the 1899 expedition to the Bighorn Basin was engineered by Hall. There may have been a hidden agenda, however, at least on Sardeson's part. Four letters in early 1896 between H. F. Nachtrieb and D. C. Worcester of Ann Arbor make it clear that Sardeson harbored resentment against Nachtrieb and hoped the latter might leave the university (he never did, but is instead considered the founding father of the Department of Zoology). Hall was characterized contemptuously by Nachtrieb and Worcester, apparently because he had played an autocratic role in the Minnesota Academy of Natural Sciences. Worcester further alleged to Nachtrieb that Sardeson had revealed to a third party that Hall was a "rascal." Dean C. Worcester (DCW) to Nachtrieb (HFN), 28 January 1896; HFN to DCW, 31 January; DCW to HFN, 2 February; HFN to DCW, 12 February. Worcester's letters are in the Nachtrieb Papers (N 114, Folder 5); Nachtrieb's letters are in his Letterpress volume I, p. 291 and 293, [UOM].

[22]FWS to RSB, 8 September 1950, [RSB].

[23]FWS to Fred B. Snyder (FBS), President of the Board of Regents of The University of Minnesota, 15 April 1944, [FBS].

[24]CWH letter "To Whom it May Concern," 29 March 1894, [FWS].

[25]FWS to RSB, 10 August 1944, [RSB].

[26]Prof. Gustav Steinmann to FWS, 21 January 1894, [FWS].

[27]Thomas C. Chamberlin (TCC), Prof. and Head of Geology, University of Chicago and Editor of *The Journal of Geology*, to FWS, 2 April 1894, [FWS].

[28]Pres. William R. Harper's form letter to FWS, 22 May 1894, [FWS].

[29]FWS to George O. Smith (GOS), Director of the USGS, 9 October 1925, [MGS-5].

[30]United States passport, issued 25 June 1894, [FWS].

[31]FWS's shipboard letter to his parents, 23 July 1894, [FWS].

[32]FWS to Herbert E. Wright, Jr. (HEW), Prof. of Geology, University of Minnesota, 26 January 1953, [HEW].

[33]Collegienbuch für Frederick W. Sardeson [FWS]. This shows that he paid his matriculation fee on 27 October 1894.

[34]FWS to RSB 5 May 1953, [RSB].

[35]Ariel (U. of Minn. newsletter), v. 18, no. 21, p. 3, 1894, [UOM].

[36]Receipt for graduation fee, University of Freiburg, 11 July 1895, [FWS].

[37]Certificate of completion of dissertation and doctoral examination (*multa cum laude*) at Freiburg, dated 5 August 1895, [FWS].

[38]Regulations for obtaining a doctorate, Faculty of Philosophy, University of Freiburg, 1892, [FWS].

[39]Diploma for the Ph.D. degree from Albert-Ludwigs University of Freiburg, dated 18 March 1896, [FWS].

[40]FWS to his sister Eva Jerome, 28 November 1947, [CWJ].

[41]*The American Geologist*, v. 16, no. 5, p. 327, November, 1895.

[42]Ariel, v. 19, no. 24, p. 1, 4 April 1896.

[43]FWS to WCB, then at the University of Texas, 7 October 1956, [WCB].

[44]The University of Minnesota *Alumni Weekly*, v. 4, no. 33, p. 7, July, 1905 reported "Assistant professors promoted to rank of professor with increase in salary include . . . Frederick W. Sardeson." [UOM]. The report was in error, however, for he was promoted ***from*** Instructor ***to*** Assistant Professor, the rank he used in his listing in *Who's Who*.

[45]Index of members of the Board of Regents of The University of Washington. The Secretary of the Board, Seattle. University of Minnesota Alumni Directory, v. 12, no. 7 of the Alumni Weekly, p. 21.

[46]FWS to RSB, 22 February 1936, [RSB]. Fossils collected by Sardeson (and their records) stored at the Department were dispersed somehow in 1913–1914, as described in Chapter 2. He wrote further, "About all that I have done thus far since coming to again as author, is to salvage a lot of theses of which I was deprived on leaving the University."

[47]FWS to RSB, 26 April 1950, [RSB].

[48]FWS to WCB, then at Minnesota, 18 March 1950, [WCB].

[49]FWS to RSB, 7 November 1950, [RSB].

[50]FWS to FBS, 5 April 1929, [FBS].

[51]FWS to FBS, 27 October 1942, [FBS].

[52]FWS to RSB, 8 October 1939, [RSB].

[53]FWS to WCB, then at Minnesota, 28 February 1949, [WCB].

[54]University of Minnesota *Bulletin*, v. 13, no. 9, p. 95–96, 1910/11, [UOM].

[55]FWS to RSB, 2 January 1954, [RSB].

[56]FWS to RSB, 18 August 1928, [RSB].

[57]Interview of Frank F. Grout, Prof. of Geology, University of Minnesota, by the author, 1949.

[58]FWS to FBS, 20 June 1950, [PP].

[59]FWS to FBS, 15 September 1946, [FBS].

[60]W. B. S. [unknown, but possibly William B. Scott], 1900, Geological expedition. *Ariel*, v. 23, no. 19, p. 231–232. This is fullest known account of the expedition, [UOM].

[61]FWS to FBS, 12 June 1928, [PP].

[62]FWS to George H. Girty, USGS Paleontologist, 11 December 1899, [SIA, RU 7329, G. H. Girty Papers].

[63]Robert E. Sloan, Prof. of Geology and Geophysics, University of Minnesota, personal communications, 3 January 1987, 11 June 1995, and 10 February 1997.

A series of 79 black-and-white photographs taken on the trip were given to me by Sardeson's daughter. They show outcrops, fossil quarries, and excavation and packing of bones; a set has been furnished to the museum of the Department of Geology and Geophysics at the University.

[64]LGH, University of Minnesota Archives, personal communication, 22 April 1994.

[65]The Minnesota Alumni Weekly, v. 12, no. 22, p. 6, 1913.

[66]FWS to his sister Eva, 20 December 1907, [CWJ].

[67]Extract of minutes of 1 June 1904 meeting of the Minnesota Board of Regents, [UOM].

[68]FWS to Eva, 26 June 1904, [CWJ].

[69]FWS to GEV, 25 March 1916, [PP].

[70]FWS to FBS, 12 August 1928, [FBS & PP]. Sardeson's testimony [1917] in State of North Dakota, Complainant v. State of Minnesota, Transcript of Record November 17 Original, Vol. II, p. 851–910. Supreme Court of The United States, October Term, 1919, (Washington, Judd & Detweiler). Also his testimony [1924] before the Senate Committee that investigated the Teapot Dome scandal: Congressional Record, 68th Congress, First Session, hearings before Senate Committee on Public Lands and Surveys, S 223-0, v. 3, p. 3010–3035.

[71]FWS to HEW, 5 December 1952, [HEW].

[72]FWS to FBS, 20 December 1938, [PP].

[73]FWS to RSB, 21 July, 1947, [RSB].

[74]FWS to FBS, 16 January 1924, [FBS], and FWS to RSB, 8 September 1950, [RSB].

[75]FWS to WCB, 20 August 1950, [WCB].

[76]FWS to Eva, 26 February 1911, [CWJ].

2

DISAPPOINTMENT AND DOWNFALL

We left Sardeson poised for further academic advancement and still good research and geologic work, possibly leading ultimately to a distinguished professional career and respect from his peers. In the event, he was driven from academe and had mostly ad hoc employment thereafter. He accomplished a great deal of geologic work, most of it of good quality, but was constrained to publish it in a journal that was ignored or scorned by most of his peers. The latter part of his career was a bitter time.

Any academic dismissal is a complex business, and a number of factors were involved in this one, among them Sardeson's prickly personality and the negative reaction of his colleagues to it at a time when personnel policies did not bar personal considerations. Even after the promulgation of the first rules on such actions by the American Association of University Professors, personal matters could—can still—be disguised as professional ones. But in this case, as we shall see, the working out of Sardeson's destiny was complicated by and implicated in a set of intellectual, professional, and institutional developments that—in consideration of his difficult personality and the fear or animus that it aroused in other minds—spelled disaster.

A STORM GATHERING?

A hint that some time in the future Sardeson might not fit into the evolving University master plan came as early as 12 April 1899, when President Cyrus Northrup, a friend of Sardeson, wrote to tell him that his salary had been raised to $750; he wrote because Sardeson had not called to see him![1] Northrup also explained that the Board had instructed him to tell Sardeson that it was "not their intention to make yours a permanent position here, and to advise you to look out for work elsewhere if it can be found." He invited Sardeson to call on him and discuss options, as the Board did "not propose to extend the work [of the Department] along the lines of your work [that is, evolution and paleontology]."

The Minnesota Geological and Natural History Survey (the Winchell Survey) was closed out the following year [1900], its final reports having been completed. Winchell suffered much complaint from the Legislature about money wasted on paleontology, Sardeson's specialty. That complaint was not directed at Sardeson per se, for he had little to do with the Winchell Survey. Probably the Legislature had simply had enough of fossils and was ready for some geological work that could be turned into money.

Sardeson replied to Northrup the same day, thanking the "Regents for their favorable consideration of my interests." He referred, evidently,

to the salary raise. "It is plain to me that no promise of future advancement has been given me," he acknowledged.[1] Yet Sardeson was prepared to defend the validity of continued support for paleontology, especially in light of the intimate dependence of the idea of organic evolution, then much discussed, on paleontology, and the importance for an educated person of knowing both what organic evolution "does not mean and what it does mean."[1]

Looking at both sides in this way provided an enlightened view of the concept, unusual for its time. Whether Sardeson's views on evolution were at issue or not, President Northrup sent him a "verbal message" to consider his position permanent and told him to "go down to Saint Paul, call on Governor John Lind, [at the time a Regent *ex officio*], and find out what is the matter."[1] (Imagine an instructor going on the trolley to see the Governor of the State to clarify a problem in a university department!) In advance of the meeting, Sardeson suspected that some public pressure was being brought on the Regents because of the "sensationalism" with which some teachers had invested the idea of organic evolution, something Sardeson had carefully avoided. Although some ministers objected to the evolution idea, those who had been in his classes were satisfied with his treatment of the subject. In the Governor's office, Sardeson recalled, "Lind spent a long time visiting with me . . . [,] said nothing about Evolution . . . [, but was] very much interested in the economic aspect of my work."[1] Here is an early manifestation of the Regents' preoccupation with economic geology, which ultimately brought in a department head who engineered Sardeson's dismissal. Governor Lind was only Regent *ex officio* at that time, but later as ex-Governor he was President of the Board, 1908–1914, the period in which Sardeson was dismissed.

Reassured at the time by his conversation with Governor Lind, Sardeson continued his intense pursuit of paleontologic knowledge and its dissemination to students—for the intellectual benefit of non-majors and for the education of majors in geology. He offered an extensive and intensive array of instruction in paleontology, more courses in fact than any other faculty member. As he was only on half-time, of course, his class load in any given semester would not have been as heavy as the variety of his courses suggests.

A NEW DEPARTMENT HEAD

Between his chat with Governor Lind and 1911, Sardeson was promoted once and his salary was raised several times. Then, in 1911, a conjunction of important changes in administration allowed the forces that favored a shift in emphasis to gain control of the department. Dr. George Edgar Vincent succeeded Northrup as President of the University of Minnesota on 1 April 1911 (Gray, 1958) and was inaugurated in mid-October of that year. Professor C. W. Hall, Head of the Department of Geology

and Mineralogy and Dean of the College of Mining, Metallurgy and Mechanic Arts, died on the evening of 10 May (Winchell, 1912; Note 2), after a long illness. Vincent, a social scientist from the University of Chicago, had taken up his duties on 1 April, "in time to attend Hall's funeral," as Sardeson put it.[3] Finding a new head for Geology and Mineralogy was one of the first personnel problems Vincent faced.

Evidently because Hall had been ill in 1910,[4] and because the Regents wanted to alter the Department in the direction of economic geology, they had attempted that year to persuade S. F. Emmons to leave the U. S. Geological Survey (USGS) and come to Minnesota.[3] Indeed, hiring an economic geologist had been on the minds of some people for over a year. Regent H. B. Hovland, a mining engineer from Duluth, revealed as early as 1909 that the Regents wanted to remove Hall and get a new Head.[5] F. F. Grout, a member of the Department, was urging the reestablishment of a state survey to work on iron resources at about the same time.[6] For that work an economic geologist was the obvious choice.

Now, after Hall's death and Vincent's arrival, a new stage in the recruitment arrived. The selection of a replacement for Hall was carried out at the Presidential and Regency level, and their deliberations were kept secret from the members of the Department.[4] In Minneapolis only a few weeks, Vincent sought advice regarding a new Head for Geology; he wrote, naturally, to his friends at the University of Chicago, or to one at least.[7] Remember that in those days "affirmative action" was still 70 years into the future! The "old boy" network was operating in Minnesota. Professor Rollin D. Salisbury, the number-two man behind T. C. Chamberlin at Chicago, recommended 17 men and one woman, ranked in order (the woman last, although he said she was highly capable).[7] As the University of Chicago was at that time the principal producer of geology doctorates in the Midwest, many of the 17 had received or were about to receive Chicago doctorates (most of the others were from Yale). Salisbury especially recommended to President Vincent any one of the first six on his list, all professors: G. F. Kay of Iowa, Joseph Barrell and H. E. Gregory of Yale, E. B. Branson of Missouri, Eliot Blackwelder of Wisconsin, and R. D. George of Colorado. He further recommended the kind of structure that the Department at Minnesota should adopt; it should emphasize General Geology, Mineralogy/Petrology, Economic Geology, and Geography. The "head" should be very strong "in an educational sense" and an "all around type," he counseled. Salisbury also recommended very strongly that the Head should not be the State Geologist, but that the State Geologist should be an economic geologist, because of Minnesota's special geologic resources.[7]

Instead of S. F. Emmons, the Regents' earlier choice, and instead of any of Salisbury's top six nominees, President Vincent brought in W. H. Emmons (no relation), who had been listed seventh on Salisbury's

list. W. H. Emmons had a Ph.D. degree from the University of Chicago, had worked for several years for the USGS, and most recently had spent four part-time years in the Geology Department at Chicago. He must have been strongly recommended for this appointment by Chamberlin and others at Chicago. Emmons was brought in as Professor and Head of Geology at $3,500 *per annum*, and as Director of the reestablished Minnesota Geological Survey at an additional $1,500 per annum (Schwartz, 1972). (This was when Vincent himself was paid only $10,000 *per annum* [1913] plus a house.[8]) Sardeson later claimed that former Governor and Regent John Lind was angry at the "deception," i. e., that Vincent had hired the "wrong" Emmons![9] W. H. Emmons predicted to the faculty that he would be at Minnesota for two years and then return to the USGS,[3] where he expected to work under Waldemar Lindgren, like himself an ore-deposits specialist, who became Chief Geologist in 1911. Emmons' hopes did not work out, however, for Lindgren became Professor and Head of the Department of Geology at the Massachusetts Institute of Technology late in 1912.

There is a reason to suspect that the Regents indeed wanted an Emmons (read ore-deposits specialist) for defense against a possible lawsuit. Upon arriving at Minnesota, Emmons spent much of his time preparing the geological aspects of a controversy that had developed in 1910 into a lawsuit in Montana.[3] Several Minnesota Regents were principals in the ownership of the Butte & Superior Copper Company, which was alleged to be stealing ore from beneath property of the Clark-Montana Realty Company, *et al.*, which latter company brought a suit to recover damages.[3] Emmons spent three months in Chicago during the winter of 1911–12,[10] probably to work with R. D. Salisbury on a defense and to have access to a better library. It is easy to believe that the prospect of a lawsuit prompted Emmons' selection by the Regents—and his most unusual salary! Sardeson obviously believed so, for he wrote of Emmons, "He came to the U. of Minn. for an emergency in Montana."[9] Emmons was on leave granted by President Vincent, 10 October through 5 December of 1915, according to Mrs. Emmons,[11] and surrendered $500 per month of his salary, meaning he was paid that by the owners, including some Regents, while on the case.[12] Interestingly, one of Emmons' fellow expert witnesses was Dr. Salisbury of Chicago, the man who had recommended his employment by Minnesota.[13]

This suit involved the "law of apex," an early legal mining practice well known to geologists. Butte & Superior owned a mine on a claim adjacent to one owned by Clark-Montana. The older claim (Clark-Montana's Elm Orlu Mine) had exposed a thick mineralized vein that extended beneath Butte & Superior's Blackrock claim and mine, and the law of apex entitled Clark-Montana to extralateral rights to that ore under that part of the Blackrock claim. The two claims also overlapped in part.

Each side had five geologists or mining engineers (some academic and some commercial) to attest to the difficult geologic conditions, especially underground. The basic issue was, however, the priority of the patented claims. For the plaintiff (Clark-Montana) N. H. Winchell's son Horace V. Winchell was one expert; for the defendant (Butte & Superior) W. H. Emmons and R. D. Salisbury were among the geologic experts appearing. The trial was held in a Montana District Court, in January and February 1916.[13]

The suit was decided against Butte & Superior in May of 1916,[13] and Emmons "found himself marooned in Minnesota."[9] Butte & Superior lost again on appeal to the Ninth Circuit in the Spring of 1918.[14] As Sardeson recollected much later, H. V. Winchell explained to colleagues in the Department that "Prof. [W. H.] Emmons' theories were based on assumed simple conditions such as might never occur so, in Geology";[9] further, Emmons had "succeeded in protecting the Regents . . . , but committed perjury to do it."[3] The record of the suit suggests no such thing as perjury, however, and the fact that the issues were legally technical and historical rather than geological casts doubt on Sardeson's allegations about Winchell's comments. Butte & Superior Copper Company had a lot of trouble in Montana—for on 25 August 1916 they also lost a suit brought against them by Minerals Separation Co. for infringement on its patented oil-flotation process for mineral recovery!

Whether "marooned in Minnesota" or there by choice, Emmons undertook to lead the Department and the Survey, and he did so until his retirement in 1944. In that interval the Department became a distinguished one for the first time, famous for its work on economic geology, Precambrian geology, mineralogy, and petrology. Similarly, the Survey was distinguished for its reports on the bedrock and Pleistocene geology of the state, always done on a small budget and often in cooperation with the USGS. At the same time, Emmons was a well-loved and inspiring teacher, and the author or coauthor of several textbooks. He wrote a leading text on economic geology and led a group that wrote a physical geology text. He even wrote an early textbook on petroleum geology, although he had once offered to "drink all the crude oil ever discovered in Minnesota." The first historian to write a modern, comprehensive history of the university, James Gray (1958), regarded Emmons as the embodiment of the conviction that "the University must serve the state."

Sardeson emphatically disagreed. He compared Emmons as an educator unfavorably to Hall,[3] an assessment that doubtless expressed mostly his animus, but also was a mistaken application of Sardeson's education/training dichotomy. A number of distinguished scientists have revered Emmons as a teacher, for they found him exciting and stimulating to a high degree, and they wanted eagerly to "do geology" as he did (Levorsen, 1950; Pettijohn, 1984). Emmons certainly paid no attention to

paleontology, which Sardeson would have found unsupportable; George A. Thiel told about Emmons teaching historical geology, which Emmons did many times.[15] Emmons liked to turn through the pages of (not read) the textbook on the lectern while he talked; when he got to the section on fossils he would flip over all the pages of fossil and habitat pictures and say something like, "In the Ordovician, life abounded."[15] Despite this lapse, Samuel S. Goldich, who was associated with Emmons in the Department for a decade, said Emmons' classes were absolutely fascinating; although Emmons was frequently out of town on consulting jobs, nobody cared, because classes were so much fun and so interesting when he was there.[16]

TRANSITION IN THE DEPARTMENT

The hiring of a new University President and Department Head and the coming of the new Minnesota Geological Survey in the same year brought many changes from the Hall administration. Sardeson must have been distressed not to have become Head (though no extant letter says so). He was troubled by what he saw as a turn toward commercialization and training in economic geology.

> When I was starting in Geology all schools, excepting Harvard, perhaps, had an educational teacher as *head* in Geology, with one or two trained economic or mining Profs. as associates. These also had been well educated before specializing. But after 1900, the trained commercial minded teachers were put ahead and training largely rather than education dominate[d].[3]

With this mindset it is small wonder that he was offended by Emmons' constant urging that the staff should strain every nerve to "boost" the Department and the reputations of its staff members.[17] It may not have been apparent to Sardeson at the time, but the movement, which he despised, to provide practical, professionally or vocationally oriented training was underway nearly nationwide, especially in the public universities, which used service to various segments of the public in their economic endeavors to justify their growing cultural authority. If, as seems likely, he set himself against this new mission resolutely, he laid a basis for the administrative complaints that assisted in his dismissal. But Sardeson wished to be free of requests for commercial consultation, in part to avoid corruption. "Pillsbury and Hill interests and friends demanded false statements from me," he would complain. [18]

As the department became less congenial for Sardeson, he began looking for new opportunities and objects for his scientific curiosity. The establishment of a new state geological survey had been much discussed in 1910, but apparently without much hope under the former, Northrup

administration.[19] Sardeson did not count on a survey for additional income, however. The career optimism that he expressed to his sister early in 1911 must have had to do with his USGS cooperative work on glacial deposits in Minnesota, which became his next major project following the work at the University.[19] Eventually, too, work with realtors and studies of soil conditions and their suitability for various uses seemed a way out of a department which was increasingly dissatisfying to him.

In those latter regards, Sardeson was becoming "commercial" himself, perhaps in self defense! But he valued "scientific" work, such as that for the USGS, for its intellectual content and geologic significance. Sardeson had worked with Frank Leverett for a month during the fall of 1909, and again during the summer of 1910, in the Duluth area.[20] They cooperated on investigations of Pleistocene deposits and the leveling (surveying) of raised beaches, the sort of work that later developed into the map of the surficial deposits of the State. In 1909 the Acting Director of the USGS applied to the Civil Service Commission for field assistants, including Sardeson during 1–11 September.[21] Whether that specific work was performed is not known, but Sardeson was appointed Geologist No. 67 of the USGS, on a *per diem* basis, at an initial rate of pay of $7 per day.[20,22,23] (Although he held this position until 1924, he had no assignments and no income from it after 8 December 1915.[22])

At this point, clearly personal—and by later standards clearly improper—considerations came prominently into play as Vincent and Emmons commenced reshaping the Department. In early August, 1911, Vincent had a conference with Frank Leverett (of the USGS) and Sardeson in which he reported that the Regents had decided to use new money authorized by the Legislature to start a new geological survey. The Geology Department was to be dominant over the Survey, and Dr. Emmons would be elected by the Regents as Director of the Minnesota Geological Survey and Head of the Geology Department.[11] Evidently it was with this appointment that the Department name was simplified by dropping "and Mineralogy." President Vincent had, after all, put four eggs into one basket: inspiring teacher, economic geologist, Department Head, and "State Geologist," when he hired William Harvey Emmons. The title "State Geologist" had been abandoned in the 19th century, and Emmons was therefore to be called Director of the Survey.

Although Sardeson was launched on the work with Leverett before Emmons arrived, Emmons may have viewed that work as a fall-back position for Sardeson when he began to consider dismissing him from the Geology Department shortly after his arrival. Sardeson's notebook[11] indicates that on 19 September 1911 Emmons assured him of encouragement and support of the cooperative work with the USGS, namely surveys of quadrangles and mapping of soils and glacial deposits. Emmons could credibly offer such assurances as he had close ties with both the USGS

and the University of Chicago, where T. C. Chamberlin was still a potent force in Pleistocene research (though Chamberlin had not been chief of that USGS branch since 1904). It seems likely that Emmons and Vincent thought that moving Sardeson out of the Department would be easier because he had cooperative work with the federal survey to fall back on.

Another event suggested the same conclusion. Emmons told Sardeson in September 1911 that Professor Grout's "not favoring me as member of the State University field survey" meant he was to be excluded from that work.[11] In later years at least, and perhaps through his entire tenure, Emmons delegated to Grout the planning of the field and office work and the apportionment of the budget for the Minnesota Geological Survey.[24] Grout had already developed a dislike of Sardeson, and Emmons' accession to power made it possible to put Sardeson down. Grout's dislike may have resulted from simple jealousy or from a clash of egos. Although Grout was a calm, kindly man, much loved by his students, he was of strong determination.

The state-and-federal cooperative project on glacial deposits came to fruition in time for the 1912 field season and provided for the mapping of six 15-minute quadrangles in the Red River Valley of western Minnesota.[25] Sardeson himself estimated the cost of the work in salary and expenses at $600 each from the U. S. and Minnesota surveys.[25] Sardeson was expected to coordinate his field work with Frank Leverett,[26] a distinguished student of Pleistocene geology who was continuing the state-wide work. Sardeson and Leverett, already used to each other, had a fruitful relationship, and they ultimately mapped the Pleistocene deposits of the entire State of Minnesota. The requirement that Sardeson cooperate with Leverett while mapping in the Red River Valley[26] meant that the plan for them to map the glacial deposits of the state had already been set in motion. Although the two had worked together briefly in 1909,[20] Sardeson sometimes wrote that he began work with Leverett on glacial deposits in 1910.[27,28] Leverett, himself an employee of the USGS, came to the work in Minnesota after having made such a map of part of the State of Michigan. Sardeson's work in the Red River Valley was published much later (1919), but covered only four quadrangles, not the six originally projected.

DISMISSAL FROM THE UNIVERSITY

Early in April 1913, John F. Downey, Dean of the College of Science, Literature and the Arts, asked Emmons if there were any persons on his staff who "should be discontinued." Shortly thereafter Emmons replied that "Dr. F. W. Sardeson, Assistant Professor in the department, finds it difficult to maintain friendly relations with the other members. He has, undoubtedly, good ability along certain lines, but his disposition has been spoiled and I doubt whether it will ever be different from what it is now. I therefore recommend that his services be discontinued."[29] (A second

person, a young geographer, was also marked for dismissal, but his case is not considered further here.) Only a day later, Emmons informed Downey, "[T]he Department of Geology has recommended that Prof. F. W. Sardeson receive an honorarium of a year's pay" and further that "the courses in Paleontology would be discontinued for one year [1913–1914]." Looking ahead for paleontology, Emmons suggested that a new paleontologist might be hired in the fall of 1914 with Sardeson's former salary, plus the increment from the dismissed geographer.[30]

Dean Downey wrote to President Vincent on 26 April, enclosed copies of the letters to him from Emmons, concurred in the recommendation of the Department that Sardeson be given leave with full pay for the 1913–14 school year, and himself recommended that Sardeson's connection with the University terminate at the end of that school year (Appendix 1). The Dean added that this recommendation had been approved by "the Advisory Committee" (Appendix 1).

Nothing happened in response to Downey's letter until Vincent replied in mid-June, suggesting that the two of them should confer with Sardeson before Emmons left for field work, in preparation for the July meeting of the Board of Regents.[31] Another unexplained delay ensued, for Downey did not reply until 28 June.[32] Although his reply was respectful, the tone of his letter clearly indicated that he thought Sardeson's case had been allowed to drift and that Sardeson might rightly claim that, had he had earlier notice, he might have found another post! Downey had deliberately sent his recommendation to Vincent on 26 April, expecting that action would be taken at the April meeting of the Regents [the 29th].[32] During this June exchange, Sardeson was out in the field, and Emmons was eager to leave as well. Downey thought a meeting that late with Sardeson would be counterproductive. "It would be utterly impossible to convince [him] that he is not the ablest man and greatest geologist in the department," Downey objected.[32] Having scored Vincent by implication for delay in the matter, Downey recommended that the recommendation for dismissal go forward to the Regents for approval.

As a consequence of Downey's letter, but following his own sense of duty, Vincent called a conference of Dean Downey and Department Head Emmons with Professor Sardeson on Saturday morning 5 July 1913 (Appendix 2). At that conference, of which there is no record, it was decided, by Vincent apparently, that a departmental meeting should be held; this was done on Sunday evening July 6th, at a session that lasted more than three hours (Appendix 2). At the time the Department had seven faculty members, including Sardeson. E. K. Soper was absent from the meeting. Those present who objected to Sardeson were (in decreasing order of length of service in the Department) F. F. Grout, E. M. Lehnerts, A. W. Johnston, Oliver Bowles, and W. H. Emmons. Downey and Vincent were present, and the latter prepared the summary of the proceeding (Appendix 2). Sardeson was also present at the meeting.

Sardeson was flayed pretty well at the Sunday evening meeting, although it is crystal clear that the quality of his science, his professionalism, and his teaching were never in question—only his personal qualities were attacked. A sampling from the record (Appendix 2) shows how bitterly he had affected his co-workers: "extraordinary egotism," "an air of patronage which is galling," "a bitter and sarcastic tongue," "belittles" others and speaks of them "contemptuously," and "sowed constantly the seeds of suspicion and discord." Vincent interpreted Sardeson's explanatory responses, writing that Sardeson's early experiences in the Department had been "unfortunate," but no details were recorded. Apparently Sardeson felt "he must fight his way," "the world was against him," and he had constantly to be on "guard to protect himself." When Sardeson interposed that things had been going rather more smoothly lately, several colleagues said they had been avoiding him for months.

Somewhat more disturbing to the Dean and the President was the report that Sardeson had discussed the Department with faculty in other departments, and that such airing of dirty linen had long contributed to "demoralization" within the Department. It was further alleged that geologists across the country knew well Sardeson's reputation for "sarcasm, bitterness and exaggerated egotism." This last must have come from Emmons himself, who, with his wide acquaintance in Washington and Chicago, would have been the only one with sufficient scope to make such an assertion.

In rebuttal, Sardeson stated that he felt the charges against him were "greatly exaggerated," but he recognized that he "had failed to secure the confidence and goodwill of his colleagues." One member (Lehnerts, the geographer), who was himself at risk of dismissal and who apparently had concurred in the original departmental recommendation against Sardeson, demurred at the Sunday evening meeting, for he thought it possible that Sardeson "could control himself." Grout, Johnston, Bowles, and Emmons voted against Sardeson, whereupon Sardeson said that he would not resign voluntarily—he would have to be dismissed. Vincent offered Sardeson the opportunity to appear before the Board of Regents (a meeting was scheduled for July 9th), but Sardeson declined, asking the President to convey to the Regents that he wished the opportunity to show that he could learn to get along in the Department. He also asked that Vincent interview three faculty members who were his close friends. These were Professors Henry Erikson and Anthony and John Zeleny (Appendix 2), all of the Physics Department—in fact, the families "neighbored" and their children played together.[33] After that Sardeson left the room and Vincent urged the geologists to consider once more whether there were any reasonable hope that Sardeson might be able to "readjust" himself. This occasioned no change in their attitudes (Appendix 2).

On Monday, July 7th, Vincent interviewed the two Zeleny brothers and Erikson (Appendix 2). Two of the three (not named) very much regretted that matters had gone so far, but recognized the characteristics which made Sardeson "so serious a problem," and concurred in the conclusion that for him to leave the University was the only solution. One of the two told Vincent that he and others had repeatedly "warned" Sardeson that he "could not safely indulge his habits of detraction and sarcasm." The third suggested that it might be better to let all the other geologists go, and to keep Sardeson because of his abilities, long tenure at the University, and his knowledge of the geology of Minnesota! He would not concur in the dismissal, but did not offer any other solution (Appendix 2). This third man probably was Erikson, who himself had a reputation of being cynical, autocratic, and arrogant.[33]

On Tuesday, now the 8th of July and the day before the Board meeting, Vincent consulted with former Regent H. B. Hovland of Duluth, a mining engineer and long-time supporter of the practice of geology in the state and at the University (Appendix 2). Hovland recognized the dimensions of the loss the University would undergo if they separated Sardeson, but concluded that it was the only way to further the project of promoting economic geology to the forefront in the Department and to support Emmons as Head. Vincent also interviewed an unnamed member of the Advisory Committee to Dean Downey and learned that the committee had not merely rubber-stamped the Dean's recommendation back in the spring, but had supported him out of strong conviction and apparently unanimously (Appendix 2).

An Executive Committee composed of five members of the Board met with President Vincent in his office at 10:30 on 9 July, and adopted Vincent's recommendation that Sardeson be fired.[34] It made Vincent's condensed memorandum to that effect (Appendix 3) a part of its minutes. Before the day was over, Vincent wrote to Sardeson, who was doing glacial field work near Brainerd, to inform him that his employment by the University was ended (Appendix 4).[35]

The Immediate Aftermath

The Department had sent its most experienced member packing. On the basis of professional qualifications alone, the Board's action could not have been justified. Although it is not possible to compare publications of different lengths and on different subjects precisely and accurately, if "full" reports or papers are taken as the measure, Sardeson in 1913 had about twice as many solid scientific contributions to his credit as the other six taken together. Both Bowles and Grout were just launching their careers; Emmons had published fewer than half as many geologic contributions as Sardeson and was the only other faculty member with a doctoral degree at that time. Sardeson, by his longer tenure, also had more practical

experience in the state, through the "outside, commercial" work that he had been expected to do under Hall.

Sardeson was 47 years old when this career disaster occurred. Oddly, he did nothing whatsoever to defend his reputation. Even presuming that his former colleagues were entirely accurate, why did he not respond? Why didn't he get a lawyer and fight back? The American Association of University Professors might take up such a case as his today, but it was not organized until 1915. Surely he was disappointed, hurt, and probably angry, but his acceptance of a summary dismissal without a complaint at the time seems surprising. "It was easy for Pres. Vincent to discharge me because I did not resist," he recalled in 1949.[36] Perhaps he sensed some validity in the charges against him and hesitated to expose himself to further scorn and abuse. His daughter Marion, only 9 years old that summer, learned about the dismissal only after she entered high school (the University High School, which she attended for only one year before switching to city schools).[33] Her parents never mentioned the subject to her. Presumably, her young friends, who included the daughters of Emmons, Erikson, and Vincent, must have known of her father's dismissal as well, but no disapprobation was directed at her or her family by those young people.[33] Later, however, when she was a young lady, she encountered some coolness when her family connection was discovered. Eighty years later, Marion, who loved and respected her father greatly, believed that Sardeson was so badly scarred by the rejection that he just buried the hurt deep within him and looked elsewhere for approval and success.[33]

If we recall that Sardeson had developed the relationship with the USGS in 1910–11, in order to help map the Pleistocene deposits of Minnesota, we might believe that he thought in 1913 that he did not need the University, that he might continue indefinitely with the national organization. In fact, however, Frank Leverett, Sardeson's partner in that work, reported that Sardeson sought to "quit all relations with Emmons" by dropping the USGS work—which was cooperative with the MGS. Charles A. Davis and Leverett were with Sardeson at that time and advised him to continue with the work which he "had so well in hand."[37] Davis was very sympathetic toward Sardeson, for he himself had been shouldered out of the Forestry program at the University of Michigan—with the acquiescence of that administration—by the man he had brought in as his assistant (Lane, 1917).

Sardeson's Apologia

We have only Sardeson's letters, written over the 45 years following 1913, to show Sardeson's view of why he was dismissed. He believed that Emmons, with Vincent's support, wanted to transform the Department from an educational unit to a training school for economic geologists (including

oil). In order to do that, Sardeson believed, they had to do some "promoting," which he always took in its darkest context. The Hall administration was a hangover from the 19th century. Most professors then had only bachelor's or a master's degrees, the curricula were concentrated on established classical forms and courses, and research as a mission of the university was found only in private institutions and at the University of Wisconsin, which T. C. Chamberlin had transformed into a sound institution for research in addition to teaching. Surely both Emmons and Vincent, recently arrived from the University of Chicago, and Emmons with a Ph.D. from there, hoped to turn the University of Minnesota toward a new path, one that they believed would be the path of the 20th century. Surely also, the proximity of the University of Wisconsin had an influence on their attitude and aspirations. So, the attempt to put new policies into action and to thump their chests a bit about the "new, improved" Department were not surprising. Who is to know, at this distance, what their motives were?

Sardeson, certainly, thought the worst. Several times he reported that Vincent told him that they intended to do some "promoting," and "that my personal inflexibility, 'honesty' he called it, would be in the way."[38, 39] The fired professor many times blamed Chamberlin and the University of Chicago generally for instigating Vincent's actions.[40, 41] He quoted Mrs. Emmons as having said that her husband "could not help doing" the dismissal.[42] Does this mean that Emmons regarded himself as a "captive" of the Regents? For $5,000 *per annum* before World War I, perhaps so! Whatever the case, it must have been their association with Chicago, not necessarily specific requests from Chamberlin and others at Chicago, that colored the visions of Vincent and Emmons.

Sardeson believed also that the charge of his being difficult to get along with originated partly with persons at the USGS.[43] Until 1904, Chamberlin had been the chief of its Pleistocene Geology section, even though he had a full-time job in Chicago. He continued thereafter to be a central figure in Pleistocene studies because of that fact, and also because among geologists in the Midwest he had the longest experience in the study of Pleistocene deposits. Frank Leverett, a USGS employee and Sardeson's companion in the work on the Pleistocene of Minnesota, never complained about Sardeson on the record—to the contrary, in fact. They worked well and companionably together. As we shall see in the chapters on paleontology and glacial deposits, Sardeson did have a couple of run-ins with Chamberlin. Whether Chamberlin would therefore have set out to destroy Sardeson, readers will have to decide for themselves. The present writer doubts it.

SARDESON'S DISMISSAL IN HISTORICAL CONTEXT

Such wrenching events as just described are not common, but there are parallels between Sardeson's experience and that of Professor Benjamin

Waterhouse, of Harvard College, who was dismissed in 1812 after 24 years of varied and effective service. His sin was the perceived unsatisfactory curation of the mineral and natural history collection, but he was also reputedly an "irascible and contentious person" and had not the skill to mollify the administration (Frondel, 1988). Summary dismissals were often attributed to some other cause than personality, but the victim's personality often played a part, as it had in the case of Waterhouse.

Until about World War II, university and college presidents and governing boards had substantially total and peremptory control over their faculties. Due process and involvement of professors in governance of the institutions (at a low level, admittedly) have since worked to avoid the sort of trial that Sardeson and many others have endured. Academic freedom began to filter into American institutions about 100 years ago (Metzger, 1955), because thousands of Americans had gone to Germany for advanced study. The transformation was slow, and it still is not complete, as some small, usually church-related, colleges operate yet under the traditional autocratic policies.

Some historical examples are outlined briefly here to show how truly unusual Sardeson's dismissal was. They come from Hofstadter and Metzger (1955), Metzger (1955), and Furner (1975), where these and other cases are described and analyzed. Furner (1975) describes the cases of E. W. Bemis, R. T. Ely, H. H. Powers, and E. A. Ross in detail. The cases reported fall into a few types of objectionable behavior: teaching of evolution, political radicalism, too liberal views of society, and immoral conduct. In 1884 James Woodrow (scientist) was dismissed by the Presbyterian Theological Seminary in South Carolina for teaching evolution. Alexander Winchell (geologist) was fired from Vanderbilt University in 1875 for alleged anti-Christian teaching of human evolution, though Alberstadt (1995) suspected a campaign by a colleague to discredit and oust Winchell. Closely allied to such cases are those in which some anti-religion views or criticism of the Bible were the proximal cause. Egbert Smyth (historian) was let go from Andover Academy in 1886 because he taught against religion. H. H. Powers (political scientist) was fired from Stanford University in 1898 for heresy, by order of Mrs. Leland Stanford, sole owner of a powerful institution named for her dead son.

The most common excuses have been criticism of the social order, radical politics, and opposition to corporate greed, but liberal religious views and wartime "disloyalty" are also invoked. H. E. Stockbridge, President of North Dakota Agricultural College, was dismissed in 1893 for social and political reasons. E. W. Bemis (economist), was fired from the University of Chicago in 1895 for pro-labor activism and again for similar reasons from Kansas State Agricultural College in 1899. E. A. Ross (economist) was run out of Stanford University in 1900, at Mrs. Stanford's behest, for loud and vigorous espousal of the wrong political views. Dr.

W. A. Jones was dismissed from the Medical Faculty at Minnesota in 1913 because he was "anti-University."[44]

A few fought back and were retained. In those cases, publicity and the support of colleagues were the keys to keeping their jobs (Hofstadter and Metzger, 1955; Furner, 1975). W. G. Sumner (sociologist) was ordered by the president of Yale University, in 1879, to drop a certain textbook because it taught evolution. Sumner fought back with well-reasoned letters, publicity, and the support of his colleagues. R. T. Ely (economist) offended a business magnate on the Board of the University of Wisconsin and was tried by the Board in 1894. He was not dismissed, because of support from the president, effective publicity, and staunch support from the faculty (Metzger, 1955; Furner, 1975).

A popular reason for dismissal during World War I was known or suspected pro-German sympathy. Gruber (1975) described the "trials" of 20 persons fired for "lack of enthusiasm for the American cause." She shows that in some other instances colleagues rallied in support of an accused teacher; if the accused also responded appropriately there was no dismissal. Lack of support from colleagues and lack of contrition were a deadly combination, however. Two examples, treated by both Hofstadter and Metzger (1955) and Gruber (1975), are summarized here.

J. McK. Cattell (psychologist) was eventually expelled from Columbia University in 1917 on a charge of disloyalty. The administration had sought for some years to rid itself of a loud, pugnacious, offensive teacher who was good at his trade and respected by his colleagues, although even they wearied of his trouble-making in the end (Hofstadter and Metzger, 1955). John Dewey resigned in protest.

W. A. Schaper (Head of Political Science) was haled before the Board of Regents in 1917, charged, found guilty, and fired from the University of Minnesota in half an afternoon (Hofstadter and Metzger, 1955). Schaper had been fingered by an informer. He told the regents he had no sympathy with the war, but followed the law and urged students to do so as well. Although he was regarded as a good teacher, he was not popular with the faculty; that, combined with "his lack of commitment to the prevailing interpretation of the war," cost him his job (Gruber, 1975). Graduate Dean at the time, historian Guy Stanton Ford had Schaper's dismissal reversed and a modest monetary compensation paid in 1938, when Ford had become President of the University. Even so, Gruber (1975) believed that Ford never considered Schaper wholly innocent.

Oddly, Sardeson approved highly of Schaper's 1917 dismissal, as recalled in several letters and remembered by his daughter.[33] He sneered as well at the fact that an oil company that included some of the University's Geology professors hired Schaper about that time as treasurer of the company.[45] Evidently they had tried to help Schaper in a small way; the oil company, incidentally, did not last long.

These few cases—Cattell, Sardeson, Schaper and Waterhouse—serve to show that lack of support from colleagues and an unguarded mouth were a fateful combination in the days before tenure and even after, one that could not be overcome by outstanding teaching or scholarship. Sardeson, in addition, may have been affected by Vincent's need to retain Emmons, as strongly desired by some of the Regents of the University, for Emmons apparently threatened to resign if Sardeson were kept.[46]

INDEPENDENCE

From August 1913 through July of 1914, of course, Sardeson was on salary and had no required duties. He continued with the federal work already underway on Pleistocene deposits, and he ventured into real estate and related matters, mostly on a consulting basis. It was not until 1917, when he began working for the State Securities Commission, that he again had somewhat regular employment. His work during the transition years 1913 to 1917 is dealt with further on.

Sardeson's Long View.

For Sardeson, the dismissal carried consequences, especially changes in attitude, that lingered for years. His view of the matter oscillated from regret over his loss to belief that he was better off doing other things than working for the University. Scorn for the scoundrels who had done him wrong was a constant theme in his letters over many years. Left without money enough to travel, he was fortunate to have accumulated fossil samples that allowed him to remain productive. His loss also had consequences for the University.

Lost fossils. Emmons did not replace Sardeson until the fall of 1914, when he hired Clinton R. Stauffer. Sardeson was disappointed that Stauffer did not carry on the paleontologic work he had started, but Stauffer explained that the collections for teaching and research left in the Department by Sardeson were not there when he arrived in 1914.[47] The records of the collections were also missing. Specimens collected or received prior to 1897, and still kept in the Department, disappeared mysteriously during 1913–1914. Those of Sardeson's 1904 gift to the University also disappeared. "As I was away 1913–14, the fossils of the Geology Department were taken illegally," Sardeson explained [early in 1914, apparently[41]], " but not those of mine, at home."[48] Sardeson later surmised that Emmons had dispersed the invertebrate specimens: "Schuchert got the brachiopods, Walcott got the Cambrian fossils and presumably Ulrich got the other fossils."[41]

The fossils represented an important scientific resource, now lost to Sardeson. In 1921 he wrote that he regretted "very keenly" being thrown out of his life work—paleontology.[49] He was always distressed by the fact that he had to leave thousands of fossils and their collection records

with the department, and he several times put valuations of $25,000,[47] or even $30,000, on the collections (including, perhaps mostly, those in his home). He also regretted the work that he was thus unable to complete. "I never cared so much for my fossils, as such, but much more for the educational value that derived from the study of them."[50]

No evidence has been located proving Sardeson's assertion that Emmons actually donated those collections and records to any of several likely recipients of such a gift. The University of Chicago (whose fossils are now in the Field Museum), the U. S. National Museum (now the National Museum of Natural History), and the Peabody Museum of Yale University have no records of major acquisitions from Minnesota in that interval.[51] However, none of these museums has up-to-date records of historic acquisitions. It is also possible that Schuchert received specimens for his personal collection and later bequeathed them to the Peabody Museum. The case is insoluble as it stands—Sardeson's suspicions may have been well founded, but they cannot be proved at this time. The collections that Hall had asked him to keep at home (after 1897), however, were still there in Sardeson's possession and care. They are the ones that came ultimately to the University.

While complaining of his lost collections, Sardeson overlooked the fact that many of those specimens in his home had been collected for the Department at Hall's specific request. On the other hand, he was neither paid for that work nor reimbursed for his expenses, so he seems justified in having kept them. Of course the collection in his home was increased greatly by his private efforts from 1913 to World War II. All worked out satisfactorily for Sardeson in the end, for he sold them to the University for $10,000 in 1947, when he left Minneapolis to live in Seattle.[52]

Inability to Publish on Minnesota. Another result of the dismissal that galled Sardeson for years was the fact that editors of the standard journals apparently would not accept his reports for publication unless they were approved by the Director of the Minnesota Geological Survey, his nemesis Emmons.[43] Sardeson referred to this constraint many times, and at least once in print (1924d). He complained specifically that articles he offered were returned if they did not have Emmons' cachet, "By permission of the Director" [of the Minnesota Geological Survey].[43] As late as World War I, only four national geological journals were published in the U.S.A.: *Bulletin of the Geological Society of America*, the *American Journal of Science* (owned by Yale, where Charles Schuchert taught), *The Journal of Geology* (owned by the University of Chicago and edited by T. C. Chamberlin), and *Economic Geology* (in which, for obvious reasons, Sardeson had no interest). With a state survey in place, editors, as a courtesy, apparently permitted each survey director to exercise veto power over work coming from their state.

Emmons may well have written to editors requesting such a screen. Sardeson and Chamberlin had already had a falling out, and Sardeson believed that Charles Schuchert, a powerhouse at Yale, had joined with E. O. Ulrich in the theft of species from his master's thesis in 1892 (Weiss, 1997). If Emmons wanted to shut Sardeson off from the standard journals, Emmons really had only to appeal to the editor of the *Bulletin of the Geological Society of America*. No records of a rejected report exist, but articles published by Emmons' colleagues (such as Allison, Grout, and Stauffer) in the 1920s all acknowledged the Director's permission. So, with malice or not, Sardeson felt that he was restricted from publishing geologic reports (other than his work for the USGS) until 1922, when Dr. Charles R. Keyes rejuvenated the former *American Geologist* as the *Pan-American Geologist*.[53] As a professional outlet, however, that journal was not entirely satisfactory. Although Sardeson and Keyes were friendly, Sardeson was not permitted to use headings, received no proofs, and was limited to the simplest of line drawings in his publications. Keyes also sometimes relabeled Sardeson's maps or rearranged his plates.[53]

Sardeson was even excluded from credit in publications based on his own work. He had a running battle with Emmons over authorship of the USGS–MGS cooperative work with Frank Leverett on Pleistocene deposits. As part of the arrangement between the USGS and the MGS, the state paid a share of the field expenses and took responsibility for publishing the results. This gave Emmons, Director of the MGS, editorial control, and he left Sardeson's name off the first map (Leverett, 1914) and the first bulletin (Leverett, 1915)! *

Thus estranged and isolated, Sardeson could only deny the hurt. Concerning his teaching post *per se*, he conceded to the President of the Regents that the University "had the right to run any kind of a Geology Department that it chooses to run and I would not willing [*sic*] be a member of what you have there now."[39] When Kenneth Caster sympathized with him for the treatment he had received years earlier, Sardeson replied, "I am conceited enough to think that the University suffered an irreparable loss in Geology when Emmons took the stand that he would not remain if I did and thus compelled . . . Vincent to dismiss me."[54] He claimed he was embarrassed by the title "Professor";[55] but as he was active in state affairs for years afterward, and beloved of newsmen for his quotes, he was often called "Professor."

Reinstatement? Despite his bitterness, Sardeson repeatedly attempted to get his job back by appealing to presidents who followed Vincent and also to his friend, F. B. Snyder, President of the Board of Regents. Snyder was also an early graduate of the University, not in Sardeson's class, for whom he had genuine affection and respect. Snyder

*This affair is described in the chapters on Pleistocene geology and the Minnesota Survey.

was on the Board for decades; although not on it at the time Sardeson was dismissed, he was appointed not long afterward, knew the Sardeson story, and had access to the Board's detailed records. University presidents new to the office tended to refer Sardeson's appeals to Snyder, who had early become President of the Board; Snyder discouraged every appeal.

The appeals included the complaint about the way in which Vincent had treated him. Several explained that the teaching of geology in the University was just then in a parlous state and that he was the man who could and should be rehired to rescue the situation (from Emmons, actually). In addition to heckling Snyder,[56,57] Sardeson appealed directly at least once to succeeding Presidents Marion L. Burton,[58,59] Lotus D. Coffman,[40,60] and James L. Morrill.[61] Always proud and defiant, he asked President Burton only to return him to his place again, in the interest of the University, claiming (inaccurately) that he did not need the money.[59]

1914–1917. In recent years, the designation "public" has appeared in certain disciplines, to distinguish those working in government jobs or for private (for profit or non-profit) agencies from academics. Sardeson was a "public geologist" long before the designation came into use.

During the prewar years he continued and completed his work with Leverett on the Pleistocene deposits and maps, to be described in a separate chapter. He also went briefly into the real-estate business, but World War I and the death of his business partner left him with only the Pleistocene mapping for the USGS and, shortly thereafter, his work for the Securities Commission.[62] The real-estate business and the mapping of Pleistocene deposits seem to have been related, for developers were interested in knowing the properties of the soils in advance of construction and development. Realizing the opportunity, Sardeson formed a remunerative connection with the "Realtors Association" of Minneapolis that led to two paths: 1) mapping of soils and Pleistocene deposits and 2) studying drainage controversies, court cases, and lobbying on behalf of developers. (This phase of his career is treated in a separate chapter.)

But permanent government work was elusive. Sardeson was on a field conference in 1913 with E. O. Ulrich, Samuel Weidman, and W. O. Hotchkiss, the Wisconsin state geologist. On that trip, he apparently appealed to E. O. Ulrich for a possible post in the USGS, which must have caused him great humiliation. However his inquiry was made, Ulrich replied that he had taken the matter up with Chief Geologist David White.[63] Ulrich told Sardeson that White was prepared to recommend his transfer to the permanent roll and a place in the paleontological section. Not long afterward, Ulrich reported that fiscal constraints prevented an appointment.[64] Sardeson thanked Ulrich rather effusively for his efforts; how it must have galled him to have sought help from Ulrich![65] There is reason to doubt that Ulrich was sincere, for he had helped cheat Sardeson of some species names in 1892 (Weiss, 1997), scorned him in several letters

to Charles Schuchert in the 1890s, and was offended by Sardeson's lack of enthusiasm for Ulrich's newly proposed systems for the Paleozoic Era, the Ozarkian and the Canadian (Ulrich, 1911).

NOTES

[1]Frederick W. Sardeson (FWS) to Fred B. Snyder (FBS), 1 April 1926, [FBS]. In support of his representations to Snyder, Sardeson enclosed with his own letter typescripts of a pair of letters dated 12 April 1899—one from Northrup to Sardeson and his reply to Northrup. The original letters no longer exist, but Sardeson had himself typed the true copies from his files.

[2]*Minneapolis Journal*, 11 May 1911, p. 1, col. 2.

[3]FWS to Prof. W. Charles Bell (WCB), then at the University of Texas-Austin, 13 January 1956, [WCB].

[4]FWS to WCB, 7 October 1956, [WCB].

[5]Henry B. Hovland, mining engineer and University Regent, to Charles K. Leith (CKL), Prof. and Chairman of Geology, University of Wisconsin, 22 December 1909, [CKL]. Hovland sought Leith's suggestions for a new Department Head, saying, "No [,] Hall will not be let out he will be appointed head of the Museum and his salary will be raised. We want the new man to build up the geological department with particular attention to supplying a high grade of mining geology to our school of mines." Hovland wanted Leith to take the post, but well knew that he would not be interested. He asked Leith to suggest "to me some good material. We do not expect to pay small salary . . . ." Leith replied on the 23rd, saying that he would be glad to help and left the matter to be discussed when they met sometime. If they did, there is no record of same.

[6]Frank F. Grout, member of the Department, to CKL, 9 September 1910, [CKL]. "The general disturbance here is so long drawn out that I am making my best efforts—small as they are—to agitate. I want a state survey under way at once for the good of all, and especially as an attraction for good men." Grout also asked Leith for ideas of how to expand the work on the Iron Range and what a state survey should do for the good of the people. Leith replied that he would take the matter up with Grout on a forthcoming trip to St. Paul; again there is no record of their having met over these matters.

[7]Rollin D. Salisbury, Prof. of Geology, University of Chicago, to George E. Vincent (GEV), 13 and 17 April 1911, [PP].

[8]GEV to FBS, with notes appended by FBS, 31 December 1913, [FBS].

[9]FWS to FBS, 15 November 1948, [FBS].

[10]*Minneapolis Journal*, 11 August 1911, p. 1, col. 2 and p. 10, col. 1.

[11]Sardeson's pocket notebook of dated memos and news clippings, [FWS].

[12]The Minnesota *Alumni Weekly*, v. 15, no. 8, p. 9, summary of Regents' minutes, 8 November 1915.

[13]Clark-Montana Realty Company *et al.* v. Butte & Superior Copper Company, District Court of Montana, No. 19, 233 Federal Reporter, p. 547–579, 1916.

[14]Butte & Superior Copper Company v. Clark-Montana Realty Company *et al.*, Ninth Circuit, No. 2939, 248 Federal Reporter, p. 609–617, 1918.

[15]Prof. George A. Thiel, Chairman of Geology, University of Minnesota, personal communication, 1951.

[16]Samuel S. Goldich, Prof. of Geology, Northern Illinois University, personal communication, 1975.

[17]FWS to FBS, ? January 1947: filed between Oct. '46 and Jan. '47 letters, [FBS].

[18]FWS to his sister, Eva Jerome, 28 November 1947, [CWJ].

[19]FWS to Eva, 26 February 1911, [CWJ].

[20]Sardeson's testimony [1917] in State of Minnesota, Complainant v. State of Wisconsin, Transcript of Record No. 18 Original, Vol. II, p. 910–938. Supreme Court of The United States, October Term, 1918, (Washington, D. C., Judd & Detweiler).

[21]H. E. Rizer, Acting Director of USGS, letter to U. S. Civil Service Commission, 21 October 1909, [NA].

[22]Sardeson's testimony [1917] in State of North Dakota, Complainant v. State of Minnesota, Transcript of Record No. 17 Original, Vol. II, p. 851–910. Supreme Court of The United States, October Term, 1919, (Washington, D. C., Judd & Detweiler).

[23]Sardeson's testimony [1924] before the Senate Committee that investigated Teapot Dome: Congressional Record, 68th Congress, First Session, hearings before Senate Committee on Public Lands and Surveys, S 223-0, v. 3, p. 3016–3035.

[24]Samuel S. Goldich, Prof. Emeritus of Geology, Northern Illinois University, personal communication, 1986.

[25]Waldemar Lindgren, Chief Geologist, USGS, to William H. Emmons (WHE), Director of Minnesota Geological Survey and Head of the Department of Geology, 27 June 1912, [MGS-5].

[26]Arthur Keith (Geologist in charge, Sect. of Areal and Structural Geology, USGS) letter to WHE, 27 June 1912, [MGS-5].

[27]FWS to Herbert E. Wright, Jr. (HEW), Prof. of Geology, University of Minnesota, 26 January 1953, [HEW].

[28]FWS to William S. Cooper, Prof. of Botany, University of Minnesota, 21 December 1935, [WSC].

[29]WHE to Dean John F. Downey (JFD), 21 April 1913, [PP, O-782]. This letter responded to a letter from Downey that is absent from the files.

[30]WHE to JFD, 22 April 1913, [PP, O-782].

[31]GEV to JFD, 19 June 1913, [PP, O-782].

[32]JFD to GEV, 28 June 1911, [PP, O-782].

[33]Author's interviews and correspondence with Sardeson's daughter, Marion Petra Sardeson Buyken (MSB), 1984 to date; entries in *Who's Who, Who Was Who*, and obituaries in newspapers, [FWS]. Also the Sardeson memorial in the *Proceedings of the Geological Society of America for 1959*, p. 143–146. [the date of his death given as 1959 in the title line is not correct; he died in 1958].

[34]Minutes of the Board of Regents, v. 3, 1911–1914, p. 349–350, [UOM, M661].

[35]FWS to David White (DW), Chief Geologist, USGS, 23 June 1913, [NA].

A rather bitter sidelight to this is the fact that Emmons had told Sardeson in mid-June that he had reserved $800 [state funds] for Sardeson's work on glacial deposits in July, August and part of September—in northern Minnesota—

to commence 1 July. This while Emmons was confident that Sardeson would be fired while in the field!

[36]FWS to WCB, then at the University of Minnesota, 3 February 1949, [WCB].

[37]Frank Leverett (FL), Pleistocene Geologist, USGS, to DW, 2 June 1917, [NA].

[38]FWS to FBS, 18 July 1938, [FBS].

[39]FWS to FBS, 2 August 1938, [FBS].

[40]FWS to Pres. Lotus D. Coffman (LDC), 24 August 1924, [PP].

[41]FWS to Ray S. Bassler (RSB), Paleontologist, U.S. National Museum, 4 March 1956, [RSB].

[42]FWS to WCB, 21 February 1956, [WCB].

[43]FWS to RSB, 15 September 1933, [RSB].

[44]Dr. William A. Jones to GEV, 28 January 1913, [FBS].

[45]FWS to FL, 31 December 1920, [HEW, JHZ].

[46]FWS to RSB, 20 July 1940; a copy was enclosed with letter of same date to Kenneth E. Caster (KEC) at the University of Cincinnati. Sardeson said that Emmons told the University to choose between himself and Sardeson, [KEC].

[47]FWS to RSB, 21 July 1947, [RSB].

[48]FWS to RSB, ? May 1950, [RSB].

[49]FWS to RSB, 16 December 1921, [RSB].

[50]FWS to RSB, 5 May 1953, [RSB].

[51]FWS to RSB 10 August 1944, [RSB]. Sardeson made such a charge several times in letters, but in this one he said, "Schuchert wrote me tauntingly that he had got my fossils, out of the 'Sardeson collection' remaining at the University." I can find no such letter in Schuchert's voluminous papers at Yale for the 1913–1914 period, and Schuchert typically kept drafts or carbon copies his letters. Furthermore, the two were mutually cordial and helpful by the 1920s.

[52]WCB, personal communication, 1949.

[53]FWS to FL, 6 May 1936, [JHZ]. The last issue of the *American Geologist* was December of 1905 (v. 36, no. 6), and the Winchell Family sold the name and the series to the Economic Geology Publishing Company. The old name and volume numbers were carried on the title page of *Economic Geology* for some years thereafter. Charles R. Keyes issued the first *Pan-American Geologist* in February of 1922 and labeled it v. 37, no 1. He had bought the stock of back issues of the old Winchell journal and offered them for sale. However, the continuity from the *American Geologist* to the *Pan-American Geologist* was illusory; the former had no legal connection to the latter, but only to *Economic Geology*.

[54]FWS to KEC, 20 July 1940, [KEC].

[55]FWS to FBS, 11 March 1941, [FBS].

[56]FWS to FBS, 4 August 1928, [FBS].

[57]FWS to FBS, 12 August 1928, [FBS & PP].

[58]FWS to President Marion L. Burton (MLB), 4 July 1919, [PP].

[59]FWS to MLB, 29 August 1919, [PP].

[60]FWS to LDC, 27 September 1924, [PP 1744].

[61]FWS to President James L. Morrill, 24 January 1952. In this instance Sardeson wrote merely to explain the record and did not request reappointment, [PP].

[62]FWS to RSB, 18 September 1951, [RSB].

[63]Edward O. Ulrich (EOU), Paleontologist, USGS, to FWS, 6 November 1913. "I have talked with our Chief Geologist regarding your appointment in the Paleontological division of the Survey. He finally assured me that he would recommend your transfer to the permanent roll but that he could not do so before the next fiscal year, there being no money immediately available for a new salary. But I see a chance for a vacancy in one of the two statutory positions for paleontologists about Jan. 1. This pays $2,000.00 per year." Ulrich said further that he could make no promise, but needed to know if Sardeson would accept an offer: "If I knew that you would take it I would lay lines in advance and thus insure a claim on a place . . . ." Evidently Sardeson did respond favorably, although his letter does not survive, [NA, EOU's letter files].

[64]EOU to FWS, 4 December 1913. The $2,000 vacancy occurred, but they decided to raise salaries in-house, and promoted Dr. Kirk; ". . . maybe next year," but next year never came, [NA].

[65]FWS to EOU, 9 December 1913. "I am very grateful for your effort in my favor." Sardeson closed the letter with more thanks, [NA].

3

PALEONTOLOGY

Before continuing the story of Sardeson's life outside academe, his many contributions to professional geology should be assessed. His steady flow of scientific reports and papers continued throughout his student, faculty, and post-academic years—until 1940, in fact, when he was 74 years old. His professional work and reports fall mostly into three categories: paleontology, stratigraphy, and Pleistocene geology. This order reflects the priority of his interests and, to a large degree, his training.

Sardeson's education, broad across the field of geology, was strongest in paleontology and stratigraphy. An important part of his master's work was in paleontology and his doctoral dissertation was on Paleozoic tabulate corals. For nearly 50 years he published articles on paleontological subjects. Sardeson was a distinguished paleontologist, but for different reasons than in the cases of many other good men. He worked across several phyla rather than concentrating on one, never made intercontinental comparisons, and named rather few taxons as compared to some energetic workers, but each of those qualities worked against his becoming famous. Sardeson's enduring useful contributions were in his advanced views of what constitutes a species, his close attention to both synecology and autecology, and his attempts to establish lineages in several groups, particularly the bryozoans. He wrote about bivalves, brachiopods, bryozoans, cephalopods, corals, echinoderms (crinoids and starfish), and gastropods. Among these subjects, early Paleozoic bryozoans and their possible relation to the tabulate corals bulk large in his more than 50 papers and in his correspondence.

Criticism of scientific work, in order to get closer to the truth, was important to Sardeson. A critic of his own earlier work as well as the findings of others, he mentioned the need for a critical attitude frequently. In his view, scientific criticism disappeared about the turn of the 20th Century, and he mourned the loss. Yet Sardeson was very sensitive to criticism leveled at him by others and objected strenuously to it, in print and in private correspondence. This dichotomy is very human, surely, but he displayed it to a marked degree. His writings suggest a suspicious and defensive attitude toward the outside world, a kind of chronic fear that someone was out to do him professional harm. Not surprisingly, this characteristic was particularly conspicuous in the years after his dismissal from the University of Minnesota.

Concerning paleontology specifically, he had some basis for his paranoia, for some of the species names that he assigned to brachiopods in his master's thesis (1892d) were suppressed by trickery before their publication (Weiss, 1997). This and other slights and perceived abuses combined to make him guarded all his life. A good example of his response to imagined slights or threats occurred in 1906–07. In connection with a paleontological paper that Sardeson

submitted to *The Journal of Geology*. Stuart Weller, a Chicago paleontologist, wrote requesting the loan of a specimen. Thinking apparently that Weller either was seeking a basis for checking his work or really seeking the gift of a fossil, Sardeson somewhat later told an acquaintance, probably a German, that that university required deposit of specimens from papers published in its journal. That man inquired of T. C. Chamberlin whether that were indeed the policy. It was not, of course, and Chamberlin was rightly incensed by the suggestion. By instinct or by chance he wrote to Sardeson to ask if it had been he who spread the libel about his journal. Sardeson replied with his understanding of Weller's somewhat ambiguous letter and furnished a copy of it to Chamberlin. Chamberlin replied with a graceful and effective putdown,[1] and Sardeson had only one further paper (1908b) published in *The Journal of Geology*.

Despite such quirks, Sardeson was an outstanding paleontologist who contributed a great deal to science. He was also a particularly sharp-eyed collector of fossils. In the years following his work at the University and the Minnesota Securities Commission, he had abundant time to indulge his bent for collecting and made some spectacular and important finds, including numerous primitive starfish and brachiopods in living position. His contributions to paleontology are described in several parts below: collecting, identifying and naming species, ecology, and taxonomic work in phyletic order.

COLLECTING AND COLLECTIONS

Collecting specimens—of anything—was a hallmark of 19th Century science. Among biological materials it was the lifeblood and basis of all derivative fields of biology. Taxonomy was both popular and vital to the science early on, and it depended on collections. The fossil record is peculiar in the fact that it is so fragmentary; the original creatures cannot be observed nor can their soft tissues be studied. Thus the more abundant the series of fossil specimens the more sound the inferences that can be made about the original populations.

Sardeson collected fossils from childhood until he was nearly 80; apparently he started serious collecting of fossils and minerals in his second year in college, in Minneapolis.[2] His collecting for several professional specialists while a university student was touched on earlier. Here we are interested in his enduring work for the University and as an independent paleontologist.

Collecting

Over his 60-some years as a "rock hound," Sardeson ranged widely over the Upper Midwest, by rail and rented transport, for he never owned a horse and rig or an automobile. Rarely, he borrowed his brother's horse and carriage.[3] He visited many localities in the Twin Cities Metropolitan Area, collecting from the St. Peter Sandstone, the Platteville Limestone, the Decorah Shale, and the basal beds of the Galena Formation. In the decades since his explorations, most of his collecting sites have been destroyed by land

development, and several quarries have been closed down and filled because of the press of nearby residential districts. His visits to some sites, such as the Johnson Street quarry in Northeast Minneapolis and the St. Paul brickyard, numbered in the hundreds. After leaving the University he continued, for decades, to collect from exposures in the Twin Cities, climbing down the walls of the Mississippi gorge near his home, which was at 414 Harvard Street, close to the University, or using the street railway to get about.

The acquisition of a family car eased the transport, if not the finances. When daughter Marion was 25 years old (1929), Sardeson bought for her birthday a blue Chevrolet 2-door sedan, on the proviso that she take her mother whenever and wherever the latter wanted to go. This started a new wave of collecting, often away from the Twin Cities, during family picnic trips. The ladies would drop Sardeson off at an outcrop or quarry, continue to some park to prepare dinner, and then go back to get him when it was time to eat. Marion, a businesswoman at the time and subsequently, continued this program of family outings-with-collecting, using her own successive cars, until World War II.[3]

Still, during the 1930s, Sardeson was virtually without any income, and fossil sales were few then also. He lamented often that as an independent he could get no institutional funds for field work. He applied to the Geological Society of America, of which he was a Fellow, for $700 of Penrose Fund money for the 1934 field season, but he was refused.[4]

There was no magic in Sardeson's collecting methods, only his legendary sharp eyes, persistence, and skill at finding what others missed. He was accurate and precise about relating the specimens collected to the enclosing rocks; he collected "stratigraphically," for he believed that one could learn the succession of life forms only in that way. He would have scorned a hatful of fossils, collected from across a rock section several feet thick, as merely pretty stones. He compared Schuchert unfavorably to Ulrich in this regard, because the latter paid some attention to the particular strata from which his fossils came.[5]

Sardeson published his views on collecting early. He looked for changes in species up the rock section, but he found very little evidence of that in the Middle Ordovician of Minnesota. Even so, "Paleontologists do not hesitate to describe new species from a single specimen, or from many specimens from a single locality and horizon," he complained (1897d). Sardeson urged that collections be numerous and that they be accurately located stratigraphically as well as geographically.

Collections

Collecting was a minor lifeline for Sardeson in his post-academic career, for his collections permitted him to continue doing geologic and paleontologic work. His fossils helped support his family and also constituted a major fraction of the important collection now at the University of Minnesota.

Sardeson turned to collecting early, out of a naturalist's love for the fossil record. He had a personal collection of fossil specimens from his early

undergraduate days at Augsburg College. While an undergraduate at the University of Minnesota he was enlisted by Professor Hall to work with the teaching and research collections of the Department of Geology and Mineralogy, and given responsibility for preparation, curation, and exchanges.[6] Hall was very keen on exchanges as a way of building up the teaching collections. We saw earlier, also, that collecting and exchanging for the Department were important parts of Sardeson's duties after he became a teaching member of the Department in 1896. It was during that stage that Hall requested that Sardeson keep newly collected material at home, rather than in Pillsbury Hall, against the day when the University should have a museum of natural history. Perhaps Hall also hoped to keep Sardeson's samples separate from Winchell's collections, for Hall disliked Winchell (Weiss, 1997). It was this home collection, stored in Sardeson's large attic laboratory,[3] that remained to him after his dismissal from the University and which grew over the years into the great collection sold, ultimately, to the University. The disappearance of the fossils kept at the Department was described in Chapter 2.

As Sardeson's collection grew in variety and number it provided some income, attracted the interest of several institutions that hoped he might give it to them, and permitted Sardeson to publish papers based on, in some cases, a few thousands of specimens. He told his daughter that the sale of a rare fossil paid for her college education at Hamline University, in St. Paul, during the early 1920s.[3] No documentary evidence of such a sale is known, but college expenses in those days were very small in dollars (Marion lived at home and commuted by streetcar). He may have told her that merely so she would not think her schooling a burden on the family. He did, however, sell many fossils during the 1920s and early 1930s.

Sardeson's employment by public agencies prevented him from doing much commercial work. For most of the period 1917–1934 his work at the Minnesota Securities Commission presented a possible conflict of interest. Until 1924, when he resigned from the USGS, his federal appointment was also an impediment to private employment (although he had had no pay from the USGS since December of 1915). Work for the Securities Commission was for expenses and per diem only while investigating a property.

Sardeson relied on sales of fossils for travel funds. Several times he mentioned needing to sell enough specimens to get to a geological convention. Starting in 1931 he tried to sell enough starfish to get to the 1933 International Geological Congress, in Washington, D. C.,[7] but failed. Between 1929 and 1932 he made more than a dozen sales to Bassler and Charles Schuchert (e.g., Weiss, 1997). Other sales occurred as well, but most records were of sales to Bassler at the National Museum and to Schuchert, personally, for the Peabody Museum of Yale. Those sales brought in from $5 to $50 each. Sardeson also traded for fossils, as when he sent bryozoans to R. R. Hibbard, of Buffalo, for "good crinoids."[8] A crash in the price of crude oil followed the discovery of the East Texas field in 1930. That and the depression put a quietus on stock promotions

for oil, so Sardeson had little work with the Securities Commission and cherished the fossil sales. He told Bassler he wished the price of fossils were higher, and that he was living on what he used to spend on trips to meetings.[9] Daughter Marion, rising in the business world as a buyer for the Dayton Company and other retailers, supported the family. Sardeson had no known inheritance and no known capital until he sold his house ($10,000 in 1938) and his fossil collection ($10,000 in 1947) to the University.

Sardeson's collection was enormous. Part of his geology library accompanied the fossils and, presumably, became a part of the Winchell Library of Geology. In the fall of 1939, he typed up an inventory showing he had 3400 "sets" of fossils, of which 821 were European.[10] A "set" contained from one to 2500 "pieces." He had five *Camerella volborthi* Billings that "took 50 years to collect, thousands of *Rhynchotrema minnesotense* Sar., some hundreds of thin sections, mostly bryozoans and corals, fine crinoidea . . . ."[11] At that time he was thinking of selling all, or large parts of the collection, before dumping the rest of it, because he thought a time might come when he could not deal with it.[10,11] He also was responding to the urging of his family, which had come to realize that if he died with it on their hands some museum might get the collection free or for too little.[12] Surely also, Marion hoped that the fossils might go, for by then they were stored in her house!

Sardeson's collection was curiously short of trilobite specimens and had not many whole crinoids. Only about 500 trilobites lie in the collection at Minnesota today, mostly from the Platteville Limestone. On the other hand, more recent collecting by Sloan and his students has found trilobites to be fairly numerous locally: they are about 10 percent of the fauna of the Decorah Shale in St. Paul and as high as 7.5 percent in the Prosser Limestone.[13] Although Sardeson worked with and published on some primitive crinoids, he never paid attention to trilobites. This skewing is strange, for trilobites usually attract great interest and attention. Perhaps he did not care to compete with P. E. Raymond, E. O. Ulrich, and C. D. Walcott.

Aware of the great value of his many specimens for research, Sardeson wanted the collection to go to an institution with a major research museum and competent curators,[10,11] which did not then include Minnesota. Thus he approached Nebraska and Wisconsin (W. H. Twenhofel),[14] in addition to Princeton. He was willing to make some price concession in the interest of "having the matter go into reasonably competent hands," and also told J. B. Knight that if he could afford to give it away he would "give it to Princeton, for obvious reasons."[10] But no deal with Princeton ever developed.

He thought of those places partly because of his former animosity toward Charles Schuchert of Yale (the Peabody Museum) and E. O. Ulrich of the USGS (the U.S. National Museum). Sardeson believed that his specimens stored at the university had been given to those two men and Walcott by Emmons after Emmons had had him fired, as described in Chapter 2. His scorn for the unprofessional paleontological work that Ulrich did for Winchell, and the

lurking knowledge of his loss of some binomials to Schuchert (Weiss, 1997) also played their parts. In addition, Ulrich had frequently been condescending to Sardeson.

Late in 1946, because Sardeson's family was preparing to leave Minneapolis the following year, Sardeson began negotiating with W. C. Bell about a possible purchase by the University of Minnesota. Bell suggested that Sardeson give or sell the collection to either the U. S. National Museum or the Peabody Museum at Yale. Both were distinguished repositories of large collections and innumerable type specimens. The Peabody Museum, of course, equated to Schuchert, and although Ulrich was with the U. S. Geological Survey, his office was in the National Museum. Sardeson told Bell that "he would dump his collection off the Lake Street bridge into the Mississippi before giving it to either museum."[15] But then, neither had offered money.

Sardeson several times estimated that his collection was worth $20,000–$30,000, but he was glad to sell it to Minnesota for $10,000 when he left there for Seattle in 1947. The specimens have since been cared for responsibly, and they form the heart of an important collection, mostly of Lower Paleozoic forms from the Upper Mississippi Valley.

DESCRIBING AND NAMING SPECIES

Until recent decades, the naming of species and genera was a sort of "game" to many persons. The object of this game was to accumulate honor and expertise by naming more species than competitors did. The urge to create names infected amateurs and academics alike in the 19th Century. Toward the end of that century, professional paleontologists increased in number and came to dominate the science. The "game" resulted in far more numerous species names than species. Regrettably, many professionals carried on in the old manner far into the 20th Century. Sardeson was not among those who did; in fact, he developed quite modern views of what constitutes a species of invertebrate animal and became sharply critical of those who adhered to the old ways. Even today, workers who nominate many species are sometimes called "splitters," and those who revise and condense such work are called "lumpers." But this is too simple an expression of the old policies against which Sardeson fought. In his view, too many workers named *specimens*, rather than the populations (species) from which they came.

Decades ahead of his time, Sardeson was convinced that valid species must be based on groups of specimens, such that the characteristics of the species included the variations that an assemblage of like creatures might possess. One or a few fossils from the group might be the "type" for which the name stood, but the variation in the group should define the species. His very early work was of the older, 19th-Century style, wherein names were the game, but he soon outgrew that philosophy. Through most of his career, the creation of names for single specimens or aberrant, damaged, or incomplete specimens was anathema to him; he was contemptuous of workers who practiced in this manner, both in his letters and in print.

Sardeson was an early advocate, almost alone in America at the time, of considering ecological aspects of the fossils he studied. He was concerned with evidence of the influence upon them of their living environment, with parasitism, with commensalism, with pathology, with damage and possible repair, and with post-depositional deformation. Consideration of such features was an important part of his concept of species. In this regard, he anticipated what are today considered important defining characteristics of fossil species.

Yet even Sardeson's adherence to the modern concepts in his earliest reports was less than complete. His taxonomic work published before 1897 includes a number of new species; many of his descriptions were somewhat clumsy and incomplete, and some generic assignments were inappropriate (Ulrich, 1896a, 1896b). That earlier work had been started or completed under C. W. Hall, who was not a paleontologist. Hall's animosity toward N. H. Winchell (Weiss, 1997) kept Sardeson from cooperating fully with Winchell's paleontologists, Charles Schuchert and E. O. Ulrich. Also, Sardeson's early work was accomplished mostly before he had been to Germany for the doctoral study that must have improved his skill in paleontology. His German professors, in fact, may have been the source of his better ideas about species, for he wrote often of their scorn for the paleontologic work of C. D. Walcott—probably aimed at Walcott's having gotten the Lower and Middle Cambrian fossil zones in the wrong order. Sardeson never attributed his new principles to the Germans or to any other source. He may well have developed them largely by himself, by reading widely and thoughtfully in the literature of evolution. Sardeson did admit that he had allowed Ulrich's bad example to lead him astray—to his regret.[16]

Natural Species

Paleontologists will never have the determinative measure of species differences available to zoologists—failure to interbreed. Therefore they must compare the trivial residual physical features of fossil hardparts to arrive at conclusions about conspecificity or absence thereof. Sardeson's precepts for defining species amount to a new view of species, as compared to the older mode of just naming bits of stone that looked different from each other. Although he would not have used the modern term, "natural species," to describe what he meant by a valid species, his method considered a similar range of features or aspects of fossils that must be considered when describing and naming a species. He put forth his own criteria straightforwardly in his early work (1897d), and continued to hammer at the distinction by citing examples of careless work or too fine splitting by "species mongers" (1930e). By the late 1930s, Sardeson could clearly spell out the controls on the forms of individuals that he considered significant: geologic distortion, dwarfism and gigantism, effects of the micro-environment, pathology, and stage of growth (1939a). Any of these might lead to species names based on such "accidents," rather than on populations and thereby to unneeded species names. The best

statements of his excellent precepts for distinguishing species are in his letters and, in combination, they teach that species contain variants which must be a part of the description.

In collaboration with his friends W. C. Bell, the invertebrate paleontologist at Minnesota 1946–1953, and R. S. Bassler at the National Museum, Sardeson further clarified his strikingly modern ideas about the characteristics of species in the late 1940s and early 1950s. Sardeson wrote that the "type specimen fixes a name for a morphologic species but not its definition, which is a matter of statistics . . . . Statistical evaluation of fossilization, of damage before preservation, of distortion while living, of environmental effect, of variation, reversion and inbreeding, of commensalism and parasites is a necessity in the description of species."[17] At about the same time, he listed, in addition, the evaluation of growth stages, signs of pathology and "evaluation by comparison to other related species."[18] These recommendations read so much like the genetic literature of the 1950s that one might think Sardeson was just parroting what he may have read recently; actually, he had urged each of these qualities in letters and paleontologic reports many times in earlier years.

To Bell, one of the young paleontologists who were engaged in the new thinking on fossil species, Sardeson claimed, "I could prove to you that new species arise from *few* individuals, from a larger and variable species under some new conditions of environment, as if by accident. Also the variability potential may be much greater in heredity than can or may appear in specimens (fossils) from one stratum or place and surely greater than seen in a 'type' specimen. Species in taxonomy [also] need to be evaluated for differences from effects of food supply." And, further, he elaborated, "total variation in one [species] may show greater differences than those between two related species. A single *fixed* character as difference may separate them."[19] Sardeson recommended to Bassler that paratypes should be selected so as to express the variation within the species.[18] "Inherited variability does not allow all the variable characters of a species to appear in any one 'type' specimen, nor in fossils from one locality or one stratum."[20] Without such safeguards, he twitted Bassler, "I am a species and you are another."[21]

As indicated, Sardeson's views were strikingly modern. A leading paleontologist of the next generation, W. M. Furnish of the University of Iowa, enumerated principles of species determination that agree "very closely" with these precepts of Sardeson's, although Furnish called attention to variations in large populations that may have evolutionary or genetic meaning.[22] An array of numerous similar specimens may have a strong central tendency of form or feature, but still hold a very small percentage of distinctly different individuals. Are they harbingers of a new community or merely exotic members of the group? It is difficult to know, and to give names to a few such specimens may not constitute "splitting." Designation of paratypes, specimens selected to show the range of morphic variation in the species, is one way to deal with this problem.

In adopting a population approach to classifying fossils, Sardeson was using "statistics" loosely, for he never compiled and manipulated measurements of fossil specimens. What he did, abundantly, was compare as many specimens of a supposed species as possible before deciding that it was or was not distinct from other populations. In such cases, he sometimes worked with many hundreds of specimens of a group. In this regard, his lifelong persistent and skillful collecting was an important attribute of sound taxonomy. Realizing, perhaps, that his strictures about the "statistical" approach to species-making did not quite fit with what he himself had done, he quoted E. D. Cope to Bell [and others], "The determination of species (fossils) taxes the highest powers of the scientist."[23]

Sardeson surely believed that he had those "highest powers" and that a number of far more famous paleontologists did not. To agree or disagree with his opinion of himself is too subjective to attempt here. What can be said is that, after his youthful indiscretions in species-making, he was relentless in his attempts to understand the qualities of a population and the variation expressed by its individuals before concluding that it was or was not a distinct group deserving to be called a species.

Sardeson often complained about workers who named specimens rather than species; in fact, he became rather angry and uncomplimentary about such persons. Sometimes the more optimistic Bassler replied that the continued work of specialists would make everything right with the world and that the superfluity of species names would be compacted into a reasonable number that expressed nature accurately.[24] Synthesis has begun at the generic level with the many volumes of the *Treatise on Invertebrate Paleontology*, but the synthesis of countless specific names, if ever attempted, will take many decades. Furnish rather agreed with Bassler, saying that "the descriptive phase has been completed but the synthesis will take a while."[22] Sardeson's typical response to Bassler's homily—that synthesis would put everything right—was that the job was worth doing right in the first place.

Policing Taxonomy

Sardeson attacked the naming of specimens and the irresponsible creation of new species from single pieces or distorted pieces. Ignorant or venal persons might be able "to make distinct species out of the shadows of our common trees," he thought (1925c). He scorned several well-known paleontologists for poorly characterized species. Most often abused in his papers and letters were E. O. Ulrich, C. D. Walcott, and A. F. Foerste.

Sardeson's main complaint against Ulrich was his penchant for naming individual specimens rather than populations. His objection was to Ulrich's work, not his person, for they met cordially a number of times in this century. His animus had two sources. Underlying always was Ulrich's part in the 1892 dispute over species names between Sardeson and Schuchert (Weiss, 1997). Much more important, to judge from the frequency of mention, was the paleontologic work that Ulrich did for Winchell, including some of the annual

reports of the Winchell Survey, but especially Ulrich's chapters in Volume 3, Parts 1 and 2, of the Final Report of that survey (Ulrich, 1895, 1897a, 1897b; Ulrich and Scofield, 1897). There Ulrich wrote on the Bryozoa, Lamellibranchiata [Bivalvia], Gastropoda, and Ostracoda. As a young teacher, Sardeson used those reports in his classes at the University. The multiplicity of seemingly unnecessary species names dismayed the students and offended him. Sardeson believed that Ulrich's work in Illinois for Worthen had been excellent. When Ulrich came to Minnesota his job was to describe and lithograph fossil species collected by the Winchell Survey. He invented species liberally, in order to illustrate morphic variation in genera, but Sardeson's charged that he did it to enhance his income, for he was paid mostly for illustrating "new species."[25,26] R. J. Cuffey, of Pennsylvania State University, believes that Ulrich's Illinois and Minnesota bryozoan works are comparable, although overly split. Perhaps Sardeson's opinions resulted more from animus than careful comparison.[27]

Sardeson often referred to Ulrich's Minnesota work as "bad behavior," dishonesty, or even treason against science, for he believed that Ulrich continued over the years to invent numerous species based on specimens, rather than populations. Bassler sometimes remonstrated that Ulrich and others were not dishonest, but had merely different notions of what constitutes a species[28] and also gave different names to similar forms in different strata, something that Sardeson found anathema. Ulrich would manipulate fossil names for non-paleontologic purposes, however.[29] Sardeson's enduring view of Ulrich's work was best expressed to Bassler, "The work of Ulrich has merit, but whoever corrects his errors will have more."[30] Sardeson put his scorn for the "splitting" by Ulrich (and Foerste as well) into doggerel.[31] "Remember the old Milwaukee slogan Soak-you-well-ah? Abbreviated it is [the trilobite] Saukiella?"

Epitaph to Saukiella.

Here a paleontologist lies:
Both his eyes are fossilized:
 He looks at fossil pieces
 And makes them into species,
He makes them up in any size.

In rolle of Augustinius,
He shows his aweful genius,
 For fractionating species,
 To make up all the pieces
And call them each a genus.

As Sardeson several times charged, works by Ulrich and Resser (1930, 1933) "assigned different ontogenetic stages of the same taxa to different species."[32] This was a serious error that others had begun to avoid a decade or

two previously! The earlier of those two monographs made a great deal of work for Nigel Hughes (1994), who revised some genera that they had overexpanded. Furnish says "It is difficult to believe he [Ulrich] was serious in his treatment" of trilobite taxa in those two papers.[22]

Ulrich's reputation as a species-monger was well known early and interfered with the progress of his career.[33] Ulrich was always annoyed that his disciple, Schuchert, had "gotten to Washington" [1893] before he himself did [1897, USGS, temporary] (Weiss, 1992), but Ulrich's species-naming was partly why Ulrich's ambition had been frustrated.[33] Subsequently, C. W. Hayes did take Ulrich onto the USGS permanently [1901] (Weiss and White, 1998), where he remained the rest of his life.

But to be fair, although Sardeson (1937a) referred to Ulrich as "the mintmaster," Ulrich was a distinguished paleontologist in many respects. He was a skilled anatomist who worked with many phyla, and he was a careful and skillful lithographer who illustrated fossils very well. The distinguished student of brachiopods, G. A. Cooper, who knew Ulrich well in the last decade of his life, believed Ulrich to be a "great paleontologist, but a poor stratigrapher."[34] Ulrich's enormous corpus of work makes him great, whatever faults one may find with his species making.

Yet, to make clear that Sardeson's objections to Ulrich's policies were not simply his own crankiness, a few examples of similar criticism from others will suffice. Rominger (1890) credited Ulrich with great zeal and sharp observation of "trifling modifications" that lead to a "fabulous number of generic and specific distinctions" [of Bryozoa]. He wrote amusingly of some two dozen new genera made out of Monticulipora, more than half of them by Ulrich, as the spokes of a wheel revolving in his mind, such that the characters of each formed a confusing continuum that led to "giddiness" and "agony"!

The Cincinnati lawyer and amateur paleontologist, Samuel Miller (1897), took scathing notice of the abundant synonymy (numerous names for members of the same population) that Ulrich had created in Minnesota paleontology.[35] Miller was himself a "wordsmith" who named too many species, but he was scrupulous about synonymy, illustration, and the protocol of nomenclature from classical roots and forms.

PALEOBIOLOGY

For 50 years Sardeson paid fruitful attention to the paleobiology of invertebrate fossils. His main concerns were anatomy, lineages, ecology, and zonation. As for taxonomy, he described only a few score of species, all but one prior to 1909, and almost all from the Ordovician rocks of Minnesota. His descriptions were typical of those times, concentrated on categorizing more or less everything to be found. Evidently, he soon tired of being a species-maker.

Probably his studies in Germany showed him other aspects of paleontology, for the theme and content of his paleontological work began to change in the late 1890s. The enduring interests of the remainder of his career were anatomy, variation of individuals in populations of fossils, living habits

and hazards of many of the species he studied, and how the conditions of fossilization affected specimens.[36] Such work often led him to conclude that the population already carried too many trivial names. Sardeson was particularly drawn to synonymy, the listing of all the various names given to a species and selecting the oldest of those as the valid name. Putting some names into synonymy not only simplified understanding, but also made the taxonomy represent the ancient communities more accurately.

The variety of his mature work is best exemplified in the numerous papers he published in the *Pan-American Geologist* between the World Wars. During that interval when the orthodox journals were closed to him, he had no regular commerce with other workers and no institutional support for collecting or laboratory work. The literature, his own wits, and his magnificent collection were both inspiration and subjects.

Sardeson's papers of those years were not at all in the style favored then by the orthodox geological journals—they were discursive, had no headings, had few references, and fewer illustrations, the latter of the simplest kind. Actually Sardeson was not in full control of these appurtenances of scholarly reporting. Papers in the *Pan-American Geologist* had no peer review; the editor permitted no headings, and Sardeson received no galley proofs by which he might correct errors. Editor and publisher C. R. Keyes sometimes also rearranged Sardeson's figures in the office, as happened with the bryozoan tree (1937d, 1937g). In spite of such difficulties Sardeson put a great deal of science and many testable ideas into that journal.

Any one of the topics discussed above was rarely the sole subject of an essay; rather it and others were described and discussed together in many of his reports. Nor are the topics sharply distinguished in the discussion of his work in paleobiology that follows; rather, examples of the sorts of things he did are cited by item from his bibliography. Over the years, he wrote on sponges, brachiopods, bryozoans, horn and tabulate corals, nautiloid cephalopods, gastropods, bivalves, crinoids and starfish, and algal and trace fossils. Sardeson made theoretical contributions in both anatomy and ecology. Despite the constraints on his publications, some of the work was pioneering.

Anatomy

Sardeson early described the development of septa in horn corals (1897f). Many years of collecting in Minnesota yielded him only a few specimens of the tabulate coral *Tetradium*. The condition of the specimens suggested that they had not been deposited where they lived. By their anatomy he concluded that the tabulates were intermediate between archaeocyathids (having lost a cloaca) and the tetracorals (1924a).

He offered an hypothesis for the origin of the spiral shell of snails, suggesting also that because their fossils occurred only close to land (1903a [Cambrian]; 1939c [Ordovician]), they had originated in fresh water. This concept has long since been discredited, however.

He suggested an ingenious way that a crinoid footplate or holdfast might form a closed figure that would serve as a float (1908b). From more than 200 loose plates, he arranged complete sets of plates for three calices and offered a possible scenario of the formation and development of the several kinds of plates (1925a). Later, Sardeson published an intensive study of the anatomy and development of the Calceocrinidae (1928a). He is credited by Brower and Veinus (1978) with "the first published reconstruction" [1939a] of a Paleozoic crinoid, although that creation of several calices from over 400 plates actually followed directly from his work of 1925. Not only did Sardeson find a trove of primitive starfish, but he worked out a detailed anatomy and lineage for them as well (1928c).

Sardeson's ingenuity showed in his efforts to get together a picture of a whole cephalopod. The anatomy of nautiloid cephalopods, and the fact that most specimens were incomplete challenged him (1930a). He proposed as an "index of growth" (1931b), the ratio of the length of shell to its expansion (diameter). Years of collecting finally yielded an apical end of *Gonioceras*, and he achieved a reconstruction of the whole shell from many partial ones (1934b), a process he had carried out earlier (1925b) for another genus. He called attention to the close relationship between *Cameroceras* and *Endoceras* and noted that, as each was founded on a species named for fragments of a creature, we can never know whether they may be congeneric (1930b). William Furnish, a distinguished student of cephalopods, thought Sardeson's work on nautiloid cephalopods was "imaginative" and "well founded," and he made use of it in his own work.[22]

Sardeson offered a topological means by which the bifoliate forms of bryozoans may have developed from the basal expansion of arborescent forms (1937c). Although he never described a new species of bryozoan, he studied their anatomy and ontogeny intensively and extensively in a series of papers (from 1935c through 1937g). In those he attempted to develop the lineages of Ordovician trepostomes and cryptostomes described in the section on taxonomic studies. His view of certain bryozoans as tabulate corals is treated below in a separate section. He also described (1929a, 1939c) the anatomy of certain brachiopods in detail.

Ecology

Sardeson's interest in fossil anatomy was linked with a persistent curiosity about the environments in which the creatures had lived. Remarks and suggestions about the life environment of the fossils he studied are characteristic of all of Sardeson's mature works. He cited (1939a) the controls of individual appearance that led to the variation to be expected in natural species—geologic distortion, dwarfism or gigantism, adaptations to the micro-environment, pathology, and growth stages. The following examples illustrate the kinds of insights he developed from careful study of individuals. These ecological aspects of individuals and their communities were concentrated in those papers having "habit" or "behavior" in their title.

Damage from accident or attack can be recognized by the repair of skeletal parts if the animal lives, as in the distortion of a corallum (1897f) or the restoration of ornamentation in brachiopods (1927a), Sardeson noted. Breakage and regrowth may affect the form of a colony of bryozoans (1935c, 1935d). Accident and pathology led to spurious variety in nautiloid cephalopods (1930a, 1931b). Distortion of skeletal parts, particularly valves, during deposition, compaction, and diagenesis, leads to shapes that the living individuals did not have, and to variety that the community did not possess (1902a, 1939d). Sardeson also recorded situations in which he believed the environment affected the growth rate or shape or both of a valve or colony (1931c, 1936b). The changes of shell or colony form with age concerned Sardeson always, and he published several examples wherein stages of a single species, shown by form, size, or shell thickness, had been described as separate species (1924e, 1926f, 1928b, 1931c, 1936f).

The living habit of individuals interested Sardeson greatly, for example in the crinoids (1928a). He once found 1,200 specimens of the rare *Prasopora selwyni*, 0.25 to 2 inches in diameter, all from the surface of a thin zone of Decorah Shale a few yards wide. He remarked what a good study of growth habit and habitat they would provide, especially if combined with others he already had from other localities.[37] Living conditions and adaptations were a constant theme in the series of bryozoan papers of the late 1930s. Perhaps his single most exciting achievement was the discovery that species of the brachiopod *Dinorthis* lived erect and attached to the seafloor (1929a). From that and other evidence he showed that different brachiopods had different living sites in the sea and suggested that their life habits affected their potential for migration. This conclusion derived from the dinorthids found in place, of course, but also from his study of brachiopod pioneers onto a sea bottom that had been devastated by a fall of volcanic ash (1926f). He concluded that those species living above the seafloor survived ash falls and were the first to be deposited in the layers immediately above beds of altered volcanic ash, or bentonite.

Some of Sardeson's paleoecologic work reappeared nearly four decades later in Derek Ager's *Principles of Paleoecology* (Ager, 1963). Specimens of bivalves and brachiopods that Sardeson found in unexpected life positions (1924e, 1929a) were described in Ager's chapter on orientation. Ager also referred to cases of apparent repair of damage to brachiopods, perhaps from trilobite bites, cited by Sardeson (1929a). Ager made full use of Sardeson's work on the succession of species that follow a regional catastrophe, in this case volcanic eruption (Sardeson, 1926d, 1926f, 1929a). The bibliography in Ager's book is 27 pages long and quite cosmopolitan; of works from the 1920s cited therein, nearly half are by Sardeson. Sardeson was not alone as a pioneer in the study of paleoecology, as Ager's bibliography shows, but he was almost alone in relating ecology to taxonomy.

Evolution

Like many scientific contemporaries of his generation, Sardeson was always keen on organic evolution, having studied it, believed it, and taught it (even at some risk) at the University of Minnesota. He extended the evidence for evolution through his additions to the fossil record. Though he did not publish on the subject *per se*, he used "minute evolutionary changes of species" to assist in his zonation of the Galena and Maquoketa series (1897a). Later he sought evidence of evolution in *Orthis testudinaria*, but he found its subspecies were contemporaneous, not in phylogenetic order (1897c). He located a doubtful transition of one brachiopod species to another (1897d). His belief in evolution also showed in many explanations of the ontogeny of fossils and in his constant concern for the place of genera and species in a lineage. For example, he speculated about the origin of monocyclic and dicyclic crinoids (1928a) and derived an interesting hypothesis of the transition from dicyclicity to monocyclicity that still has not been tested (Brower and Veinus, 1978).

Evolutionary theory did not undermine Sardeson's belief in the supernatural. A writer of poems with religious significance and a study of the relation of natural science to religion (1902b), he reacted strongly to reports in the mid-1920s of organized objections to the teaching of organic evolution and opposition to the inclusion of Darwinism in textbooks.[38,39] And in his teaching and public lectures on the subject while at the University, he had been careful to avoid disparaging religion and inciting objection to organic evolution.

BRYOZOA VERSUS TABULATA

Sardeson's youthful enthusiasm for certain ways of reading fossils sometimes blinded him to better ideas that came from others later on. In his doctoral work at the University of Freiburg, Sardeson undertook to establish the intimate relationship of the fossil tabulate corals to the alcyonaria (1896c). He further took the Monticuliporoidea to be tabulate corals and put them into the Order Pennatulacea. For this he had the encouragement of his director, G. Steinmann, and the subsequent approval of K. A. von Zittel, the most distinguished German paleontologist of the day and author of the first comprehensive paleontology textbook. The view that monticuliporoids were corals was shared at the time by Scottish paleontologist H. A. Nicholson (1881) and American paleontologist C. L. Rominger (1890), among others. The methods used in Sardeson's dissertation (1896c) and his conclusions on the tabulates and alcyonaria were severely criticized by USGS paleontologist G. H. Girty (Girty, 1896), but not Sardeson's assertion of the coralline status of the monticuliporoids. Ulrich, an independent paleontologist at the time, was downright contemptuous of Sardeson's dissertation.[40] The contrary view, that the monticuliporoids (= trepostomes) and cryptostomes belonged in the Phylum Bryozoa with the cheilostomes and others, began to be developed by Swedish paleontologist Lindström (1876) and, in the view of many, was established by Ulrich (1890).

Confident of his German education and the superior authority of von Zittel, Sardeson fought a rearguard action in favor of the view that early Paleozoic bryozoans were in fact corals (1901a, 1901b). A part of his argument was the presumed close relationship between the trepostomes and the cryptostomes, which he never quite abandoned (1937d, 1937g), though Cumings (1904) had shown how closely the cryptostomes are related to the undoubtedly bryozoan cyclostomes. By close study of the earliest stages of growth, Cumings (1912) established that the trepostomes, or monticuliporoids, are truly bryozoans; Cumings' conclusion has not been overturned. Following his massive testament to the coralline affinity of the monticuliporoids (1901a, 1901b), Sardeson did not publish on the bryozoans again until 1935, wherein he called them bryozoans (1935c). He acknowledged Cumings' work only grudgingly, arguing that the latter's "conclusions . . . are not altogether convincing" (1936h). Though Cumings was right and Sardeson wrong, a few large, domal "bryozoans" *have* turned out to be tabulate corals; the *Monotrypa magna*, of Schuchert and Dunbar (1934), for example, is the coral *Labyrinthites chidlensis*.[27]

Although the published record suggests that Sardeson retired quietly, if not gracefully, from the bryozoan-coral contest, he definitely did not abandon the field altogether. He argued and re-argued the validity of the contention in his own early work and von Zittel's textbook that the monticuliporoids were corals (von Zittel, 1895).[41] When von Zittel's student, C. R. Eastman, translated the von Zittel textbook (Eastman, 1900), he took advantage of the abundant available studies of American fossils and engaged a number of specialists to revise or extend the chapters that he translated from von Zittel's original. As a consequence, in Eastman's first edition (1900) some genera of monticuliporoids were described in both the chapter on bryozoans (prepared by Ulrich) and the one on corals (translated from the German edition)! In the second Eastman edition (1913), the contested genera were all among the bryozoans.

Sardeson believed that Eastman and his collaborators had "betrayed" von Zittel and had "stolen" his name and reputation.[12] Surely a major part of Sardeson's bitterness was the fact that E. O. Ulrich, his paleontological antagonist, had prepared the chapter on bryozoans for Eastman (1900). That Ulrich's protegé, R. S. Bassler, did the same for Eastman (1913) did not improve Sardeson's disposition. Despite a decades-long and generally friendly correspondence between Bassler and Sardeson, the latter harped on the Eastman-von Zittel matter, a poorly disguised sneer at Bassler himself.[42] When poor Eastman killed his brother-in-law and later himself [1918], Sardeson viewed that as some sort of expiation of guilt for what Eastman had done to von Zittel![43]

Although he believed himself devoted to Truth and Science, Sardeson never faced the fact that his early work on genera that he believed were corals had been superseded most definitively by Cumings (1912), nor ever acknowledged any evidence contrary to his youthful certitude.

SARDESON'S TAXONOMIC WORK

Although the number of species a person named, or had named for oneself by another, is a classic measure of paleontologic achievement, Sardeson's non-taxonomic work, just described, is of greater importance than mere names. Even so, the taxonomic work should not be ignored. The last of Sardeson's rather few names were published in 1908, except for "filling" a crinoid *nomen nudum* left by Ulrich (Winchell and Ulrich, 1897). Several of his taxonomic studies offered no new species, but were concerned only with revisions and lineages. In the case of both corals and bryozoans, no new names were proposed at all. Sardeson did publish names for "sponges," brachiopods, gastropods, bivalves, cephalopods, and crinoids. All of the work reviewed below dealt with fossils from the Upper Mississippi Valley, and nearly all of them were from the Ordovician rocks.

There is no suitable measure of success in taxonomy. One can, however, inquire as to whether the work is still in place and to what degree. That is the purpose of this section. If there were someone who had worked recently on the many genera that concerned Sardeson over many years, that person could state the degree to which Sardeson's conclusions have stood the scrutiny of other paleontologists over the decades. No such person exists, however, so what follows is an estimate of the durability of Sardeson's work.

Many of Sardeson's genera and species have not been studied since his time. Therefore, one cannot say that Sardeson's conclusions were sound. The best one can say is that they were not so blatantly wrong as to attract attention. Thus this section attempts to estimate the quality of his taxonomic work by consideration of only a fraction of the whole. For this purpose I relied on the knowledge and advice of experts in the several phyla that concerned Sardeson. The specialists who have helped me are cited in the relevant sections, as well as in the Acknowledgments. Errors in the judgments of Sardeson's work are mine, and not those of my helpful colleagues.

"Sponges"

In his study of the fauna of the St. Peter Sandstone (1896a), Sardeson found examples of the structureless, sediment-fillings of living- or feeding-burrows, in those days called fucoids by some, or thought by others to be sponges. He named *Rauffella? fucoida* for what he thought were fillings of a now-dissolved sponge. In a later study of miscellanea, he called them "sand sausages" (1925c). Uncertain what they were, he guessed that they probably were fillings of a burrow having tool marks on the walls. He discussed established species of *Rauffella* from the local Ordovician beds and described one new one, *R. ulrichana*. Today the genus *Rauffella* is not recognized as a fossil. It is not even considered a trace fossil, which would deserve a binomial label. That Sardeson's two species have fallen from paleontological grace is not really a reflection upon his skill, because many genera and species of questionable biologic status were named in those years.

Corals

Sardeson's principal concern with corals was his early work associating the monticuliporoid bryozoans with the tabulate corals. His view was refuted and was championed only by himself in the 20th Century. In addition he revised the horn-coral genera *Streptelasma* and *Zaphrentis* (1897f) by collapsing 12 species of the former and three of the latter into one, S. corniculum. Bassler (1915) agreed only partly, listing eight of the 14 "synonyms" as valid species. Sardeson redescribed *Lichenaria typa* and specified its place in the evolution of the tabulates (1899d). His last published consideration of corals was a revision of the genus *Tetradium* (1924a). His work with corals was minimal, compared to the attention he gave to other groups, and is altogether not memorable.

Bryozoa

Sardeson's early work on the Bryozoa was all directed to their affinity to the tabulate corals, as already described. Following that was a long hiatus (1901–1935) during which he published no work on them. From 1935 to 1937 he published a series of 14 papers on the anatomy, growth forms, and living habits of about 25 genera of Minnesota bryozoans. In those papers he revised many of the genera and also proposed a family tree of the genera (1937d, 1937g). The "tree" was of the trepostomes and the cryptostomes, which he suggested belonged together in an "Order" Monticuliporoidea. During 1934–37, Sardeson had some sharp exchanges with R. S. Bassler regarding bryozoans (SIA - Bassler Papers); Bassler tried to rein in Sardeson, pointing out his errors of statement and imagination.

Although Sardeson named no species of bryozoans, his revision of the Minnesota genera and his family tree were major contributions. R. L. Anstey and R. J. Cuffey gave generous help to me by criticizing Sardeson's 60-year-old conclusions on species and lineages. Sardeson simplified the array of species in the Minnesota Ordovician by putting many names into synonymy. A comparison of his "valid" species with current usage shows considerable change from his conclusions. Many of the species names that Sardeson suppressed in the 1930s are in use today, partly because of the views of specialists and trends in bryozoan science, and partly because new genera have provided niches for others. In fact, only within the genera *Prasopora* and *Rhinidictya*, have Sardeson's species revisions stood the test of time well. Furthermore, several species considered valid by Sardeson have been moved to other genera, some new, some old. Modern specialists are more accepting of fine distinctions and multiple species than was Sardeson. Of course, the technology of bryozoan study has changed remarkably since Sardeson's day: methods of observation and measurement that he could not have imagined are in regular use today. On balance, perhaps, we should say that Sardeson did reasonably well for his time, but that he was a "lumper" of bryozoans.

Sardeson's tree of bryozoan genera, the first of its kind, has fared somewhat better than his generic assignments; even so, it agrees only partly with current views and cladistic studies (statistical measures of relationship).

A few genera are now known to occur earlier than they are represented on his tree, but this probably results from new collections, some in other regions. Sardeson's tree had seven "limbs"; most of the same genera he diagrammed would be arrayed on four limbs today. Also, a few of the genera in Sardeson's tree are on quite the wrong limb according to our current understanding of generic and familial relations. Sardeson deserves respect for his innovative try and for having gotten part of the tree "right." Of course, what is "right" today may change with time, but probably not as much as the changes resulting from new techniques and new knowledge since the 1930s.

Brachiopods

As was true of his work with bryozoans, Sardeson's adventures with brachiopods were in two phases widely separated in time. The first phase, during the 1890s, was one of describing and naming species. From 1926 to 1931 he published a few papers on brachiopod habitats, repair of injury, and anatomy, as was described in a previous section. I am indebted to J. Thomas Dutro, Jr. for assistance with this section.

Sardeson described and named 27 species of brachiopods. Most were published in his master's thesis (1892d). Of the 22 species there proposed, six were put into synonymy (suppressed because equal to names established earlier) prior to his publication by a clever ruse worked by E. O. Ulrich for Winchell and Schuchert (1892). Twelve more were put into synonymy by Winchell and Schuchert (1895). The other four species have been credited to Sardeson by various authors over the years. Eight of the 12, in fact, have been "restored" as Sardeson's species by Wang (1949), Cooper (1956), or Rice and Hedblom (1987). The six "stolen" in 1892 by Ulrich for Winchell and Schuchert are explained in detail by Weiss (1997). Those six added to the 12 still used makes 18 good species out of 22 published—pretty good for a master's candidate!

Sardeson named a species of *Crania* from the St. Peter Sandstone (1896a) that has stood the test of time, probably because no one has attempted the paleontology of that almost barren formation since Sardeson's work. *Crania reversa* from the Prairie du Chien Group (1896b) is now in *Petrocrania*. Sardeson assigned a new species, *dolata*, from the Oneota Dolomite (1896b) to the genus *Lingula*, but it has since been placed in *Parobolus*. However, all of his taxonomic work in the 1896b paper called down a scathing review from Ulrich (1896) for poor species descriptions and incorrect generic assignments.

By a full revision of Orthis testudinaria and related forms (1897c), Sardeson attempted a zonation of the Middle and Upper Ordovician limestone and shale section. Along with the revision he proposed three new species that still stand; they rested for a while in Dalmanella, but are now in Paucicrura. He proposed no new species in his review of the genus Crania (1931c), but he did put some species into synonymy.

Gastropods

Sardeson's work on Cambrian snails was welcomed by T. C. Chamberlin, Editor, for *The Journal of Geology*, but Chamberlin suggested that it showed some haste in preparation and recommended some revision.[41] Whether it was revised is not known, but it was published by Chamberlin (1903a). Sardeson seemed always to be offending Chamberlin, somehow.

Between 1892 and 1902, Sardeson named 24 species of Ordovician gastropods and two Carboniferous species. His subsequent work with snails was all faunistic reports on the Upper Cambrian and Lower Ordovician beds of the Upper Mississippi Valley (1903a, 1932b). Peter J. Wagner determined the fate of some of Sardeson's trivial names for me. The Ordovician gastropods of the Upper Mississippi Valley have never been monographed, so that task was not easy. Sardeson's first-named nine species (1892d) include one based on a poor interior cast and another based on a difference in spire height—now known to be a plastic quality. Another is a likely synonym and several are in different genera today. Two of the three species named in 1896a are in different genera now, but the species have not been evaluated.

Of the dozen species named in 1896b, three have survived by being made the type species of new genera: *Helicotoma peccatonica* of *Lophonema*, *Murchisonia putilla* of *Gasconadia*, and *Raphistoma leiosomellum* of *Chapultepecia*. All three species are poorly known, but are valid by the rules. Of the other nine species newly named in the same report, six are probably synonyms of species in the same or other genera. They were based on poor material or on differences no longer thought to be significant. Here we have a perfect example of the "irresponsible" species-making against which Sardeson railed in later years, and about which he was himself embarrassed later on. Many of these taxons were accepted by Bassler (1915), and some put into different genera, but the modern views are against both Sardeson and Bassler in those cases.

Bivalves

Sardeson named 20 species of bivalves, in three papers from 1892 to 1902; the last new species, Carboniferous in age, is not considered here. He returned to the study of bivalves a quarter-century later, to revise certain genera by putting some species into synonymy and to discuss the ontogeny of species. Finding a specimen of *Cyrtodonta megambonum* (Whitfield) with some original material preserved, Sardeson (1924e) restudied many specimens to learn the kinds of distortion that they suffered in the sediment and rock. He also described changes in the shell with growth and the growth habit—attached by the foot to a hard substrate. Along the way he reviewed the plethora of Minnesota species as well and put 13 into synonymy. A study of the numerous Minnesota species of *Vanuxemia* (1939c) consisted mostly of reducing the essential names by synonymy, but he also repeated his idea from earlier years that those bivalves restricted to nearshore locales in the St. Peter Sandstone were probably immigrants from fresh water—this notion is no longer considered valid. The last paper on bivalves (1939d) returned to the genus

Cyrtodonta. Sardeson concluded that variation within the genus and in its species, together with the effects of distortion by compaction in the sediment, justified the reduction of about 30 species to only three.

My summary of Sardeson's new species names rests on the evaluation provided me by John Pojeta, Jr. Of the six species Sardeson named from the Ordovician in 1892, four are subjective synonyms today, even as Bassler (1915) had scored them. One is still a useful and valid species. Of the 13 species named from the Ordovician in 1896 only one, *Psiloconcha senecta*, is known to be valid; another four will probably remain limited to the type specimens. The remainder are poorly preserved and, although Bassler (1915) accepted them, they may well become junior synonyms when and if they are studied. On balance, Sardeson's creations among the bivalves have not fared well. They show the youthful nomenclatorial exuberance of which he himself was later aware. A thorough restudy of Minnesota Ordovician bivalves probably would reduce most of Sardeson's and Ulrich's many species names to just a few.

Cephalopods

Sardeson named six species of nautiloid cephalopods (1896a, 1896b). According to Rex E. Crick and the records of Curt Teichert, all six have lapsed into obscurity and have been used rarely, if at all, since their introduction. *Orthoceras minnesotense* (1896a) was from the St. Peter and surely has never been restudied since. His species *Ascoceras gibberosum* (1896b) is not recognized in the ascocerids today, nor has it been placed in another genus. Despite acceptance of all six names by Bassler (1915), none has been reviewed since their introduction. Of course, some type specimens may have been dispersed from the University of Minnesota in 1913–14, as described in Chapter 2. It is likely as well that Sardeson's specimens were not of the best condition, in common with his bivalve and gastropod samples. Whatever the case, his names for cephalopod species have come to naught.

Sardeson returned to the study of nautiloid cephalopods some years later, but he was then intent on revision of genera, anatomy and ontogeny, and reconstruction of whole shells from fragments (1925b, 1930a, 1930b, 1931b, 1934b). He was able to associate the problematic fossil *Nanno* with *Endoceras proteiforme*, finding that the former is actually a fibrous calcite filling of the siphuncular point of a youthful *Endoceras* shell (1925b). Furnish's approval of Sardeson's work on nautiloids has already been described.[22]

Crinoids

Sardeson published seven papers on echinoderms. The two on starfish were mentioned in a previous section; the others were on crinoids, of which he named two genera and nine species. W. I. Ausich kindly helped me make a considered judgment of Sardeson's work on crinoids. His *Strophocrinus dicyclicus* (1899e) survives in *Carabocrinus*, although he had believed his specimens intermediate between some "cystocrinoidean ancestor" and *Carabocrinus* itself. Bassler (1915) put the species into *Carabocrinus*, as Sardeson did himself from new material (1925a); at that same time Sardeson also described

C. conoideus. Later (1939a) Sardeson realized that it was only an immature *dicyclicus*, a junior synonym. Brower and Veinus (1978) suppressed *Strophocrinus* and agreed that *conoideus* is a junior synonym of *dicyclicus*. After a long search Sardeson found some large plates that may have inspired Ulrich's *nomen nudum*, *Carabocrinus magnificus*, of Winchell and Ulrich (1897, p. cxxiii). Having found such plates, he applied the name, and the species is still valid.

Sardeson sorted and studied a large number of crinoid holdfasts for which he erected the genus *Podolithus* and five "species" (1908b). Ingeniously, those "species" names were the generic names of the presumed owner of the holdfast! He made those binomials provisionally, against the day when holdfasts might be correlated with certainty to calices already named. That day still has not come; furthermore his naming scheme is not allowed by the rules of the International Commission on Zoological Nomenclature, so the genus and the five species suggested are not proper biological names (Brower and Veinus, 1978). Also, holdfasts generally are not given binomials today.

For many years *Cremacrinus punctatus*, from the Decorah Shale, had been thought to be the oldest form in the lineage, but Sardeson found specimens of the genus in the Platteville Limestone as well (1928a). Observing a significant difference from *C. punctatus*, he named the species *C. arctus*, which is still valid.

So, three trivial names survive today from Sardeson's crinoid work. His "form genus" *Podolithus* and its five "species" would not be erected today, but times have changed. Giving binomials to materials of very uncertain affinity was not frowned on in Sardeson's day. Brower and Veinus (1978) noted that Sardeson speculated on the evolution of carabocrinoids very early (1899e), and they complemented him as well on several other aspects of his work. Furthermore, their own comprehensive study was made possible only by the large and carefully documented collections that Sardeson had made. On the whole, his modest creations among the crinoids have survived well.

TAXONS NAMED FOR SARDESON

Only seven groups of fossils have been named for Sardeson—a paltry testament to his innovations and his abundant contributions to paleontology. The first was a species of bivalve, *Vanuxemia sardesoni* (Ulrich, 1892); it was small honor to Sardeson, who discovered that Ulrich had "restored" the specimen irrationally. Sardeson remarked on it (1924d) and said it was not a species, whereupon Ulrich wrote[45] to wonder why Sardeson had said that in print and to say, further, "I wish I could agree with you." Ulrich's accumulated annoyance from Sardeson made him wish he had never honored Sardeson with the name! An alga, *Chaetocladus sardesoni*, was named for him by Ruedemann (1909). In 1915, Bather erected the cystid genus *Pyrgocystis*, with *P. sardesoni* as the type species, in appreciation for some materials from the Decorah Shale that Sardeson had sent him in 1901 (Bather, 1915). Sardeson's friend Bassler created several new genera of bryozoans, including *Sardesonina*, for "bryozoan specialists who had contributed much to the science" (1952). Sparling (1964) named a bryozoan, *Prasopora sardesoni*, implicitly

acknowledging Sardeson's contributions to the study of bryozoans. Brower and Veinus (1978) named *Pycnocrinus sardesoni* because of his great contribution to the knowledge of Middle Ordovician crinoids of the Twin Cities area. This nomination would have pleased Sardeson greatly, for it recognized both his paleobiologic work and his massive collection. The Sardesoninidae, a family of bryozoans based on the genus *Sardesonina*, was named by Lavrentjeva (1985).

NOTES

[1]Thomas C. Chamberlin (TCC) to Frederick W. Sardeson (FWS), 6 February 1907, [UC, TCC papers, Letterbook XX, p. 328–329 and p. 585–586].

"It now appears from the copy of his [Weller's] letter which you furnish that he was unfortunately brief and unexplanatory in his request for the specimen and that it is possible to put such an interpretation on it as you did. This indeed, would not have been unnatural to one wholly unacquainted with the practices of the Journal, but you had previously enjoyed its hospitality to the extent of several papers dealing with fossils and were not unfamiliar with its liberal attitude and it was appropriate for you to have ascertained by direct inquiry whether your inference from a single unexplanatory sentence in a short and obviously hurried letter was really the rule of the Journal or not." He then explained the policies of the journal and said that they will consider further papers from Sardeson even though he "is prone to see evil." Chamberlin ended "I trust, therefore, that you may find some way to give your productions printed expression with as little tax upon our hospitality as possible."

[2]FWS to Ray S. Bassler (RSB), 21 December 1946, [Smithsonian Institution Archives (SIA), R. S. Bassler Papers (RSB), RU 7234, Box 7].

[3]Interviews and correspondence with his daughter Marion Sardeson Buyken, 1984 to date.

[4]FWS to RSB, 21 August 1934, [RSB].

[5]FWS to RSB, 23 August 1934, [RSB].

[6]*Ariel*, University of Minnesota newsletter, 7 February 1891, v. 14, no. 5, p. 63.

[7]FWS to RSB, 24 June 1931, [RSB].

[8]FWS to RSB, 30 September 1928, [RSB].

[9]FWS to RSB, 11 November 1931; he had recently sold some starfish to Bassler for $25, FWS to RSB, 22 February 1930, [RSB].

[10]FWS to J. Brookes Knight (JBK), Paleontologist, U.S. National Museum, 30 October 1939, [SIA, J. B. Knight Papers, RU 7251, Box 8, Folder 19].

Further to the issue of the gift or sale of Sardeson's fossils: A former student of Sardeson's, Franklin B. Hanley, was a member of the Geology Faculty at Minnesota (1937–1944). At an alumni reunion in June 1943, he suggested that Sardeson give his fossils and library to the University. Writing to Bassler, 10 July 1944, Sardeson said, "I might have been tempted to give at least some of the fossils, for him," [RSB]. Hanley, however, died in 1945.

[11]FWS to Benjamin F. Howell, Sr., Prof. of Paleontology, Princeton University, 19 August 1939. Sardeson wrote in the hope that Princeton might purchase the collection; having no funds to do so, Howell forwarded the letter to JBK.

[12]FWS to Fred B. Snyder, 17 December 1939, [UOM, F. B. Snyder Papers (FBS), Folder 38].

[13]Robert E. Sloan, personal communication, September 1996, and Eric P. Hedblom, personal communication, October 1996.

[14]FWS to RSB, 8 October, 1939, [RSB].

[15]W. Charles Bell (WCB), personal communication, 1949.

[16]FWS to RSB, 25 February 1933, [RSB].

Sardeson drew Bassler's attention to an instance in which E. O. Ulrich had misrepresented a fossil. "Personally my first experience with Ulrich was in sending him some fossil collection from here for him to identify, at Professor Hall's instigation [1890 or 1891]. Among them was a specimen of *Cypricardites rectirostris* H. which lacked the posterior hinge [it was broken off]. In time Ulrich describes [Ulrich, 1892] that as C. [now *Vanuxemia*] *sardesoni* n. sp., and *mirabile dictu* the specimen not only has a posterior hinge but that hinge is such as no other Ordovician fossil has, curved," Sardeson recalled.

"I was enlightened. It was a temptation for me to do likewise,- but I did not although its effect on me was to take the seriousness out of me and permit me to act carelessly to a degree which I always regret."

[17]FWS to WCB, 18 March 1950, [WCB].

[18]FWS to RSB, 15 January 1952, [RSB].

[19]FWS to WCB, 20 November 1950, [WCB].

[20]FWS to RSB, 4 December 1950, [RSB].

[21]FWS to RSB, 7 November 1950, [RSB].

[22]William M. Furnish, Prof. Emeritus of Paleontology, University of Iowa, oral communication, August 1996.

[23]FWS to WCB, 13 December 1950, [WCB].

[24]RSB to FWS, 12 January 1931, [RSB].

[25]FWS to Percy E. Raymond, Prof. of Paleontology, Harvard University, 4 April 1944, with a copy to RSB, [RSB].

Sardeson wrote to Raymond that Ulrich's agreement with Winchell was that he be paid by the square inch for lithographic plates prepared for the Final Report. "Winchell, ignorantly, limited Ulrich to the figuring of new species and in order to adiquately [*sic*] illustrate them, Ulrich split the species up into 'new' ones and old. Else he worked gratis throughout for Winchell."

Edward O. Ulrich (EOU), Paleontologist on the Winchell Survey, to Charles Schuchert (CS), New York State Museum, 2 February 1891, [YMA, MSS 435, Charles Schuchert Papers, Box 43, 44].

Ulrich reported that he had done nine figures of corals at one dollar each and described them for an additional $1.50 each. He also said he had been paid $230 for two plates of figures of bryozoa; with an average of 30–31 figures per plate he earned $3.60 per figure, which price must have included their descriptions. EOU wrote to CS, 24 September 1892, that he wanted to draw the cephalopods for Winchell as well, perhaps 100 figures. He wanted to charge $1.50 per figure, but would take $1.25, because "Winchell would think the other too high."

FWS to RSB, 1 August 1950, [RSB].

Sardeson studied Ulrich's ". . . plates and figures intensively and [I] see that they represent the variation and diversity of preservation of few species, well and

fully. But in the text he diverted to making many new species, out of those differences, to meet unreasonable restrictions by Winchell, of course. N. H. Winchell was hard to get along with."

FWS to RSB, 8 October 1939, [RSB].

Sardeson called Ulrich's practice "deliberate deception."

[26]A persistent story, apposite even if apocryphal, is retold in the chambers of the U. S. National Museum of Natural History, as old stories are. It is thought, but not with certainty, to have come from Josiah Bridge. It is to the effect that Ulrich once showed Winchell a plate of snails of one species; Winchell said he couldn't afford to spend money on a whole plate for just one species. When the plate was submitted for publication the same pictures represented several species!

[27]Roger J. Cuffey, Prof. of Paleontology, Pennsylvania State University, personal communication, October 1996.

[28]RSB to FWS, 17 October 1937, [RSB].

[29]Josiah Bridge, Paleontologist, USGS, to James S. Cullison, Prof. of Geology, Missouri School of Mines and Metallurgy, 21 September 1933, [U. S. National Museum, Bridge correspondence files].

This long letter details a story wherein Ulrich learned that a snail genus of his had several species *nom. nud.* and one valid one, named by Sardeson long before. By rule, that species was the genotype, but Ulrich "had a fit." "He hates Sardeson," for the latter knows Upper Mississippi Valley paleontology, and has not hesitated to tell Ulrich what he thinks of his species making, "And there is a lot of truth in what he says." Finally, Ulrich split the genus into two, putting Sardeson's species into the much smaller one and the most into the other, and then wrote a long argument about the spurious similarity between the two genera and explained that they had undergone parallel development. Bridge then wondered how many other such, or similar, cases there were in the literature!

[30]FWS to RSB, 6 November 1933, [RSB].

[31]After receiving a copy of Ulrich and Resser (1933), Sardeson was so amused by the splitting of trilobite genera and species that he wrote a short poem to lampoon Ulrich.

Sardeson sent copies [16 and 17 February 1933] of this to Bassler, C. K. Leith, Schuchert, then Prof. of Paleontology, Yale University, and Ulrich, then Paleontologist, USGS. Bassler gave his copy to Bridge, so copies remain in Bridge's and Ulrich's Papers [U. S. Geologi 0cal Survey correspondence, Record Group 57, National Archives] and in the Leith Papers, [University of Wisconsin Archives, Box 26, S-misc. file].

Schuchert twitted Ulrich about the same issue 10 April 1933, by writing that Ulrich and Resser's report reminded him of Sardeson's poem and noting that "five genera have seven old species and ninety-five new ones." [YMA, Box 39].

[32]Nigel C. Hughes, Asst. Curator of Invertebrate Paleontology, Cincinnati Museum of Natural History and Science, personal communication, April 1996.

[33]Charles D. Walcott, Director of the USGS, to Ulrich, 14 December 1898, [NA, Record Group 57, U. S. Geological Survey Correspondence, E. O. Ulrich's file].

"I think the great source of your difficulty in obtaining a permanent position [Ulrich was on the USGS temporarily in 1897] in connection with either State or National geologic work has arisen from the fact that rightly or wrongly, whichever

it may be, you acquired early in your work the reputation of being a species maker. The impression also gained ground that you were pugnaceous [sic], owing to the squabbles among the Cincinnati collectors and describers of species." The reader should note that this was written after publication of the Minnesota paleontological reports and after Miller's scathing indictment of Ulrich's work there.

[34]G. Arthur Cooper, Paleontologist Emeritus, National Museum of Natural History, personal communication, 1990.

[35]In his second appendix, Miller (1897) called attention to four cases of Ulrich having named the same "species" twice, with different names! Then, referring to Ulrich's work for Winchell, he wrote (p. 720), "The synonymy in Ohio Geology, Vol. VII, and in Geological Survey of Minnesota, Vol. III, if not appalling, is without parallel in natural history."

[36]TCC to FWS, 7 April 1899, [TCC].

The letter is a gracious rejection of a proposal to do "biological work" at the University of Chicago. Had Sardeson realized that he needed more groundwork, or was he thinking of leaving Minnesota? His letter does not survive. Chamberlin may have been miffed because Sardeson had turned down the fellowship offered him at Chicago in 1894.

[37]FWS to RSB, 21 August 1934, [RSB].

[38]FWS to RSB, 27 October 1928, 5 May 1931, and 8 October 1939, [RSB]. FWS to WCB, 18 March 1950 and 20 August 1950, [WCB].

[39]FWS to FBS, 10 March 1926, [FBS, Box 17].

[40]EOU to CS, Paleontologist, U.S. National Museum, 9 September 1896, [Yale Manuscripts and Archives (YMA), MSS 435, Charles Schuchert Papers, Box 44].

[41]FWS to RSB, 9 January 1937, [RSB]. FWS to WCB, 13 December 1950, [WCB].

[42]FWS to RSB, 12 May 1937 and Postscript no. 3 of FWS to RSB, 10 September 1944, [RSB].

[43]FWS to RSB, 15 May 1953, [RSB].

[44]TCC to FWS, 24 June 1903, [TCC].

[45]EOU to FWS, 6 January 1925, [NA, Ulrich's letter files].

4

STRATIGRAPHY

Sardeson's work on stratigraphy was the second of his three major contributions to geologic science. Stratigraphy here includes his work on sedimentation and sedimentary petrology, as well as petroleum geology. His "commercial" geologic work is covered in a separate chapter. Virtually all of his stratigraphic work and publications described the Lower Paleozoic rocks of the Upper Mississippi Valley—the Cambrian and Ordovician of Minnesota and Wisconsin and a bit of Iowa. He concentrated on the Ordovician beds and their fossils, and most of his interest centered on their occurrence in Minnesota. The major themes of Sardeson's work, a local geologic column (for extra-regional correlation), stratigraphic control of faunal elements, primacy of biostratigraphy over lithostratigraphy, and the chronostratigraphic utility of key beds (corrosion zones and bentonite layers), extended throughout his work and publications, from his earliest efforts to the end.

STRATIGRAPHIC PHILOSOPHY

Although Sardeson's educational background was broad, as was typical for those times, his first love and principal competence was paleontology. Over many years he investigated stratigraphic and paleontologic problems in tandem, but he always considered that the fossil zones were fundamental to stratigraphy and more reliable for areal and regional correlation than the lithic units or beds defined by rock or mineral composition. Of course, he was a keen and successful collector of fossils, and he very early had an exhaustive knowledge of the faunas of the region. Sardeson found it easy to discover zonal markers at obscure sites. Few rock exposures show whole formations, and at small outcrops the rock composition alone may not identify its place in the column with the ease and accuracy that the fossils did for Sardeson.

When Sardeson began work the region was still poorly known geologically, geologic maps were highly generalized or nonexistent, and the thousands of artificial exposures (including wells) that we now take for granted had not been made. Stratigraphic division by biozones was not only traditional in those days, but also effective. Nowadays, after several generations of workers have combed, mapped, and correlated the many beds in those thousands of square miles, it is easier to distinguish detailed lithofacies changes from, say, Minneapolis to Janesville, Wisconsin, a 260-mile distance (airline). Now that numerous formations and members have been defined—from a column that had only four or five named parts

a century ago—it is easier to identify short intervals of the column in small exposures. Thus the earlier workers such as Sardeson faced and surmounted greater difficulties.

In his stratigraphic work as elsewhere, Sardeson set a high standard for scientific work. He believed that dividing the rock record by the succession of species or faunas in major rock units was "scientific" geology, whereas defining, naming, and correlating lesser lithic units—according to the nature of the rock alone—was "commercial" geology. He considered the latter a far inferior mode, and maintained that view all his life. Persons with less knowledge of the fossil faunas had the opposite view, of course.

What Sardeson overlooked, or was too close to the problem to recognize, was the subjectivity of paleontologic taxonomy and the comparatively greater objectivity of lithostratigraphy. Paleontologists were far more numerous, relatively, in Sardeson's day than now, although many were "species-mongers" and of no real help to stratigraphy. Most geologists were trained in the same several subspecialities of geology, and many, though weak in paleontology, thought themselves competent to name specimens and to erect biozones! The irony is that Sardeson was himself frequently bitter about the "decline" of paleontology, which he felt was caused by such people. In print and in many letters, he complained about species-mongering and careless and irresponsible taxonomic work. Such a trend indeed existed, although perhaps it was not so very bad as Sardeson made out.

That trend "played into the hands of" the lithostratigraphers; they could find a change from sandstone to dolomite, and they believed they could follow it across country. Too, they had a practical understanding of facies changes, and could deal with them, even though some of the great gurus of geology did not believe in lithofacies at that time. Thus, not needing training in paleontology nor advice from an expert, lithostratigraphers could do more work in less time. "Commercial" that may have been, but their method was—and still is—effective. In fact, lithostratigraphy and its relationships to biostratigraphy and chronostratigraphy, as expressed in the current North American Code of Stratigraphic Nomenclature, have become the accepted way of life (NACSN, 1983).

STRATIGRAPHIC INNOVATIONS

At the beginning of his career, Sardeson got an idea that was not only innovative, but proved to be useful in the study of the rocks of the Upper Mississippi Valley. His method of classifying and correlating the Middle Ordovician strata led ultimately to today's practices. Geologic study of the region had started before the middle of the 1800s, and from then into the first decade of this century the local rocks were divided and named mostly according to the divisions and names used in the eastern United

States. Workers there had given up trying to use divisions and names from Europe and had created their own geologic column with locally based divisions. That lesson seems not to have been received by the early workers in the Midwest, however. Most of them were from the eastern United States and had studied geology there first. Therefore they usually thought in terms of eastern U. S. equivalents, which meant mainly New York names for the rocks of Wisconsin and Minnesota (Winchell and Ulrich, 1895). The name Galena Limestone, of the Middle Ordovician, was an exception to this general rule because it came from extreme northwestern Illinois.

Sardeson initially accepted units called "Trenton," Galena, and "Cincinnati" in customary practice by previous authors (1892c, 1892d). Yet by the late 1890s he had abandoned the New York–names policy (1896e, 1897e) used in that early work, although he sometimes afterward used the old terms, such as "Trenton," either in a generic sense or as a statement of correlation with the New York section. Pioneering a new direction, he took the view that what was needed most in order to understand the rocks of the Upper Mississippi Valley was a series of local names, applicable over the region, accompanied by full knowledge of the faunas of each named unit. On the basis of the faunas, the local section could thus be compared, contrasted, and correlated with that of any other region in North America. These new regional comparisons would contribute to nationwide stratigraphic knowledge. Such a practice would obviate the endless arguments over whether this or that rock was "Trenton," as well as the confusion of trying to correlate the so-called "Trenton" of Iowa with the alleged "Trenton" of Wisconsin, for example. Further, once the local biozones were well known, a reasonably confident statement of correlation with the type Trenton of New York could be made.

More than mere local names were involved in Sardeson's concept. Fossils had to be collected stratigraphically; that is, the place of each collection must be carefully located in the rock succession. Only in this way could useful "scientific" biozones be developed and lineages be distinguished. Discriminating between fellow scientists on the basis of this practice, he noted once that Ulrich was careful to keep track of the stratigraphy of his sample sources, but that Schuchert was not.[1]

Sardeson never indicated whether the superior stratigraphic philosophy was his own independent idea, or whether he learned it from someone else. There is no reason to believe that he learned it from Professor Hall; if Hall had been so inclined, he would probably have taken that tack in his earliest joint publication with Sardeson (Hall and Sardeson, 1892). Instead, it seems likely that Sardeson picked up the idea from Winchell, who used local names for parts of the Cambrian "Potsdam" in the early years of the Minnesota Geological and Natural History Survey (the Winchell Survey). Winchell gave local names to several lithic units (e.g., Jordan, St. Lawrence, Shakopee) of what is now the Upper Cambrian and Lower

Ordovician of Minnesota. Even if the concept was original with Winchell and adopted by Sardeson, it is to Sardeson's credit that he too applied it, to the Middle Ordovician part of the column. Unfortunately, as was also true of Winchell's innovations, Sardeson's new stratigraphic classification was largely ignored by others. Winchell himself, in fact, used a mixture of exotic and local terms to divide the local column in the paleontology volumes of his Final Report (Winchell and Ulrich, 1895, 1897).

It goes without saying that Sardeson insisted that species should be identified accurately. He also believed it was wrong to give more than one specific name to a single group of organisms simply because the group ranged across a formational boundary.[2] We saw in Chapter 3 that, within paleontologic work, one of his abiding interests was trying to follow lineages up the rock column. But to do that effectively, one needs to find the changes in the fossils, not in the names. Nevertheless, changing names at formation boundaries (or worse, at state lines!) was a great temptation to the species-mongers, and Sardeson was continually disappointed by his colleagues in this regard. This practice that he condemned is unfortunately still popular today.

PALEOZOIC ROCKS OF THE UPPER MISSISSIPPI VALLEY

Sardeson's early work, before his study in Germany, included the whole lower Paleozoic section of the Upper Mississippi Valley. Upon his return to Minnesota in 1895, he began to concentrate on the younger part of the section, that which is now recognized as Middle Ordovician in age. Even so, he never ignored the Upper Cambrian and Lower Ordovician parts of the section, and returned to them frequently in later years. In the mid-1930s a serious stratigraphic conflict occurred between geologists at the University of Iowa and those in Minnesota, where members of the Geology faculty held appointments both in the academic department and the Minnesota Geological Survey (MGS). Because Sardeson loved a scrap and because he knew the local stratigraphy and the history of its development better than anyone else, he flung himself—joyfully, no doubt—into the fray.

This section describes Sardeson's contributions to the study of three aspects of midwestern stratigraphy—a) the whole column, b) the older Upper Cambrian (Croixan) beds, c) the Lower Ordovician formations—and shows how his work placed him in disagreement with the Iowans. Here and in all the following sections of this account the beds under discussion will be dated and named according to current knowledge of their age and nomenclatural usage, e.g., Ordovician instead of Lower Silurian, or Platteville instead of Trenton Limestone. Changes of emphasis on the several subjects are recognizable only in the amount of work published, for he remained interested in and knowledgeable about the whole local column.

The Whole Column

Sardeson took the Paleozoic of Minnesota for his first challenge while cooperating with his mentor, Professor Hall (Hall and Sardeson, 1892). Together they described some 740 m (2400 ft), at maximum thickness, of Cambrian, Ordovician, and Devonian sedimentary rocks (the Silurian being absent there). Areal extent of the Devonian rocks in Minnesota is trivial, and they are not well exposed; analysis and naming of those beds had and has been left to the Iowans. Hall and Sardeson (1892) described and divided the Cambrian section in a rather standard way, but the Ordovician strata above the St. Peter Sandstone were divided into twelve "beds," most being distinguished by and named for fossils. This was Sardeson's work, developed in two parts of his master's thesis (1892c, d), wherein he gave more detailed descriptions of the stratigraphy than was customary. Although the number of beds and his names for them subsequently underwent some revision, he had proposed a scheme of stratigraphic units, traceable over southeastern Minnesota and adjacent Wisconsin, that had never before been attempted (Table 4.1). It is clear that the innovations in the work by Hall and Sardeson (1892) were provided by Sardeson, as they were derived largely from his master's degree work (1892c, d). Hall and Sardeson (1892) was published in June of 1892, and Sardeson's thesis (1892a–d) was published, as preprints, that April (Weiss, 1997). Indeed Sardeson began developing his subdivisions of the Middle Ordovician while still a college senior, for Ulrich had mentioned Sardeson's "beds" in February 1891.[3] (Sardeson's thesis work is treated extensively in a following section.)

The Upper Cambrian (Croixan) Rocks

Probably in connection with his work on the Lower Ordovician dolomites (Hall and Sardeson, 1895; Sardeson, 1896b), Sardeson continued work on the subjacent Cambrian sandstone units. He spent the summer of 1908 working on the Cambrian of the Croixan type area, but was prevented from publishing his results because E. O. Ulrich of the U. S. Geological Survey (USGS) was taking over that territory.[4] This problem arose in connection with an ill-fated manuscript map and text of the St. Croix Dalles folio that Hall and Sardeson had submitted to the USGS in 1906. The manuscript was rejected for cause, but also apparently because of a possible conflict with T. C. Chamberlin, described below in the chapter on glacial geology.

To avert conflicts between geologists with rival claims to contested turf, USGS Director Charles Walcott attempted to foster better communication between the state and the national surveys. In the course of developing the new policy, Walcott met with Sardeson and other geologists at the Geological Society meetings in New York.[5] Sardeson mentioned to Walcott his hope of working on the Cambrian section in

Minnesota and noted that E. O. Ulrich, an employee of Walcott's, was working on it also. Walcott took the comment as a claim for rights of turf and asked Ulrich to prepare a statement regarding Sardeson's "claim" to work in Minnesota. Ulrich's reply, laying claim to the entire state, was forwarded to Sardeson.[6] Sardeson promptly replied to Walcott that he had not meant to assert sole rights to Minnesota, but what he said to Walcott may well have sounded that way. Walcott upheld Ulrich's claim and barred any further work by Sardeson from publication in official channels.

At that time there was no state geological survey in Minnesota. The Winchell Survey terminated 1 October 1900, and the current Minnesota Geological Survey (MGS) did not appear until 1911, when W. H. Emmons was hired by the University to establish it. What Walcott did in 1907 was to deny the most prestigious publication outlet to an individual in a state where one of Walcott's USGS workers laid claim to the privilege of doing geology unimpeded by possible contrary views. Ulrich was then, of course, preparing his great work on the Ozarkian and Canadian Systems (Ulrich, 1911) and did not care to have anyone carping about his conclusions. Then too, Walcott had had difficulty in getting zones of the American Lower and Middle Cambrian in the right order; he may well have not wanted another skillful and assertive stratigrapher working on the type Croixan! This episode was doubtless the one that led Sardeson to complain many times that Walcott had proscribed criticism of the USGS. As Sardeson summed it up years later, "I did a lot of work in the St. Croix valley which I could not publish because it would disagree with Walcott and Ulrich."[7]

In 1912 Sardeson attempted to cooperate with Samuel Weidman of the Wisconsin Geological and Natural History Survey on joint studies of the Cambrian and Lower Ordovician units common to both Minnesota and Wisconsin. They had two projects in mind. First, he and Weidman had finished mapping the St. Croix Dalles quadrangle,[8] the same area for which Hall and Sardeson had submitted the 1906 folio draft that had been rejected. At that earlier time the USGS had suggested that the two states might undertake the project jointly, which would have relieved the USGS of obligation to T. C. Chamberlin. In 1913 W. C. Alden, Geologist-in-charge of Glacial Geology at the USGS, encouraged Weidman in this project, and suggested that Weidman prepare a contract for his and Sardeson's work.[9] No money came, for Chief Geologist David White soon discovered he could not fund the proposed contract, but he expressed again the hope that the two states might support the work.[10] Although Sardeson's field work was largely completed, and he wanted funds only for the writing stage,[11] Weidman and Sardeson's quadrangle map and description were never published, probably because Walcott and Ulrich intervened.

Their second project, on Weidman's initiative, was to determine the physical and faunal continuities of the Cambrian beds from southeastern Minnesota into south-central Wisconsin. They also were curious about

the relation of fossiliferous Cambrian sedimentary rocks to the underlying unfossiliferous Red Clastic Series.[12] Weidman and Sardeson had planned this project prior to Sardeson's dismissal from the University of Minnesota in early July 1913 and the two had hoped to begin work in that summer. Sardeson, who was still on the roll of the USGS, hoped later that summer that such a project (in addition to the ongoing work with Leverett on glacial geology) would provide employment in substitution for his lost university work. According to Sardeson, both projects were suggested to him by Weidman.[12, 13]

This Cambrian correlation project was quashed as well, this time by W. O. Hotchkiss, Wisconsin State Geologist, who simply told Weidman he could not hire Sardeson or join him in field work on the Cambrian formations. Furthermore, no contract for the work appeared from the USGS. Walcott and Ulrich had prompted Hotchkiss's edict, as Sardeson had thought.[13, 14] Hotchkiss was an intimate friend and protegé of Ulrich (Weiss, 1992). Once again Ulrich had succeeded in protecting his revisions of the Cambrian and Ordovician sequences and nomenclature (Ulrich, 1911) from criticism.[15] A telling remark about that proposed work on the Cambrian correlations was made by David White, Chief Geologist of the USGS, to Frank Leverett, to the effect that, if Sardeson wanted to work on the Paleozoic of the region, "He'll run into Ulrich."[16]

Despite those troubles, in September 1913 Hotchkiss, Sardeson, Ulrich, and Weidman spent about a fortnight together on a regional review of the Cambrian deposits of Minnesota and Wisconsin.[17] David White joined them for part of that time. Sardeson felt that Ulrich was trying to get his notes and conclusions for his (Ulrich's) own future use.[15] He reported years later that on the trip Ulrich "did not ask for criticisms! I gave none."[18]

Although the USGS never adopted Ulrich's Ozarkian and Canadian Systems (Weiss and Yochelson, 1995), apparently Hotchkiss did. Between his actions and Ulrich's, Sardeson was thus closed out of the Cambrian of the region for good. As an example, in reply to a query from Sardeson about the Mendota Formation, Ulrich wrote long letters calling Sardeson's view of the Cambrian stratigraphy insufficient and unsophisticated, sent a table of his own classification, and demanded to know why Sardeson thought more faunal evidence was needed before Ulrich's proposed new systems for the early Paleozoic Era could be substantiated.[19] In the event, lack of sufficient evidence troubled other stratigraphers as well and caused the Ozarkian and Canadian to be ignored by almost everyone (Weiss and Yochelson, 1995), thus confirming Sardeson's impertinent opposition to them. Even though the cards in the hands of the powerful were stacked against Sardeson, Ulrich's names Ozarkian and Canadian were never adopted for systems, and it is somewhat ironic that the classification of the Croixan of today resembles Sardeson's old view of the situation (except for an increase in unit names) more closely than it does Ulrich's!

Sardeson's letters often referred to the depositional history of the units that he studied, but they add nothing to his published work on the Cambrian. He was distressed by the fact that his successor at Minnesota, C. R. Stauffer, paid no attention to Sardeson's publications and even seemed unable to get the history of Winchell's early work straight.[20]

Sardeson returned to the older part of the section in four papers (1924b, 1932b, 1936d, 1939e). In the first he showed the development of the nomenclature of the Upper Cambrian and Lower Ordovician beds of Minnesota, and untangled the confusion that Winchell had created when he carried names from the Minnesota River Valley to southeastern Minnesota and put some names on the wrong beds. Describing the fauna of the Jordan Sandstone (1932b), Sardeson pointed out that it contained no body fossils (in contrast to trails and other organic evidence), admitted his own errors of statement much earlier (1896b), and corrected those errors. He also traced its history as a stratigraphic unit. Though Sardeson denied the occurrence of fossils in the Jordan, Stauffer (1925) reported a few species, mostly trilobites. It is now known that fossils occur in the bottom four meters (13 feet) of the Jordan (Nelson, 1956; Hughes, 1994). This leaves *most* of the Jordan unfossiliferous, but it seems that Sardeson's usual particularity should have led him to the same fossils, for it is unlikely that Stauffer or later workers moved the St. Lawrence–Jordan contact.

Sardeson's reviews of the Cambrian of the region in the 1930s (1936d; 1939e) were concerned more with its nomenclature and history than with the strata. Regrettably, the best explanations of the history of the confusion caused by Winchell in the nomenclature of the Cambrian formations in Minnesota were those offered by Sardeson in the *Pan-American Geologist*, a journal that contained much dross and was pretty well ignored by "orthodox," particularly academic, geologists. Sardeson regretted the slowness with which younger workers, including Professor Stauffer, came to understand the section. That was partly, of course, because they had read the literature selectively.

Sardeson deplored the growing number of units and names and also inveighed against the "economic convenience" of requiring that a geologic formation should be mappable. In 1957 he still believed that Dresbach, Franconia, St. Lawrence, and Jordan were names enough.[18] He also preferred that strata should be both defined and correlated by their biotic content.

Sardeson had less competition regarding the so-called "Lower Magnesian" beds, which included the St. Lawrence and Jordan Formations, now long known to be Cambrian in age. Two major works of the 1890s (Hall and Sardeson, 1895; Sardeson, 1896b) represent his principal contribution to this part of the Upper Mississippi Valley section. In those two old reports he was not in conflict with Walcott and Ulrich, for those men never staked a claim to those beds. The Jordan is unfossiliferous,

except close to its base, and therefore was of no interest to either Walcott or Ulrich; the overlying dolomite beds (now called the Ordovician Prairie du Chien Group) were too young for Walcott, and seem never to have interested Ulrich. In the stratigraphic paper, Hall and Sardeson (1895) described units from the St. Lawrence through the Shakopee Formation, correlated them with units in Wisconsin, and summarized the biostratigraphy of the units.

The Lower Ordovician Formations

Sardeson's early work on the "Magnesian Series" (1896b) included what are now both Cambrian and Ordovician formations, as we have just seen. Some of the systematics, that is the biological classifications, that he came to regret in later years, as well as the fauna he assigned in error to the Jordan Sandstone are contained in that paper. Ulrich (1896b) was highly critical of the work, but was upset most by the generic assignments of the numerous species. The Lower Ordovician part of that old "Magnesian Series" is now called the Prairie du Chien Group.

In the 1930s Sardeson turned to the Prairie du Chien Group, and concentrated on a few aspects of that group. First, he demonstrated that the faunas of the Oneota and Shakopee Dolomites were different; those two great thicknesses of dolomite can be distinguished by their faunas.* Second, although Sardeson had very early used Wooster's New Richmond Sandstone concept (Hall and Sardeson, 1892; Sardeson, 1896b), he subsequently insisted that it was not a valid lithic unit, and that it was unnecessary for recognition and correlation of the Oneota and Shakopee Dolomites. Sardeson contended that scattered lenses and stringers of clean quartz sand occurred almost anywhere in the Oneota and were common in the lower Shakopee. At the New Richmond type locality a zone several meters thick that includes several beds of clean sandstone lies between the lower, thicker Oneota Dolomite and the overlying, thinner Shakopee Dolomite (1934c). The sandy zone is widespread in the eastern and southern parts of the region and locally is a single thick sandstone, as in the Root River valley of southeastern Minnesota. Where the sandstone is thin, as at Shakopee, it represents the feather edge of the deposit. As a regionally valid lithostratigraphic unit, it has proven useful over time, as Keyes (1935) clearly stated.

Sardeson always defended the concept of the lower (Oneota) and upper (Shakopee) dolomite units, distinguishable by faunal characteristics, but mappable only together. He explained several times the history of Winchell's early work in Minnesota, showing how Winchell had named several units along the Minnesota River and then misapplied those names when he worked in southeastern Minnesota. For example, Winchell mistook the Oneota Dolomite there for the unit he had named St. Lawrence in the

* Furnish (1936, 1938) found this difference also true of the microfaunas.

Minnesota River Valley. Winchell's pioneering field work was done before much mapping had been done and he also had little knowledge of the faunas; he does not deserve blame for that and Sardeson did not blame him. Sardeson scoffed at the name "Prairie du Chien Group," for he regarded it as an unnecessary complication of the successive Oneota and Shakopee Dolomites. Denying, as he did, the validity of the New Richmond Sandstone as a formation made it easy for him to find "Prairie du Chien" unnecessary.

Sardeson's report on a peculiar phenomenon that he called "cone-domes" (1926a) is discussed in connection with his work on petroleum geology. It is interesting to note in passing that Sardeson's association with the rocks of the Shakopee Dolomite dates from his childhood, for the town quarry of Argyle, Wisconsin, and its nearby mill, once owned by Sardeson's father, is in the Shakopee Dolomite.[21]

The Conflict Over the Shakopee Dolomite

From its establishment in 1911 to about 1960, the MGS's contributions to stratigraphy and paleontology consisted of grants to some graduate students and faculty for projects in "soft-rock" geology. In the Department, until 1946, stratigraphy and paleontology were in the hands of C. R. Stauffer, whose chief interests were petroleum geology and micropaleontology. Stauffer seems to have made little use of the historical development of Minnesota stratigraphy, as contained in reports of the Winchell Survey; he re-invented wheels and confused several already established concepts. The MGS had a strong economic and basement-rock emphasis. Sardeson, the while, had no official position in Minnesota geology, published most of his work in the *Pan-American Geologist*, and was largely ignored by academic geologists in the region, including Stauffer. The work of those people was diminished in quality and value because of their snobbery.

Where Minnesota failed to tread, professors from Iowa and Wisconsin moved in. A new generation of stratigraphers was anxious to put lessons from petroleum geology into practice. They began to stitch together a composite picture of the Lower Paleozoic rocks of the Wisconsin–Minnesota region (e.g., Trowbridge and Atwater, 1934; Twenhofel, Raasch, and Thwaites, 1935). Several doctoral candidates working with Trowbridge at the State University of Iowa played major parts in that effort. In addition to their major professors' interest in regional problems, two economic factors were supplementary motives for graduate students to enter this work: jobs were scarce, and students practiced in stratigraphy and paleontology were welcomed by the geological departments of oil companies. There is no evidence that Minnesota geologists objected to those "invaders" from nearby states.

Trouble developed when C. W. Couser, a student of Trowbridge's, asserted in 1933 (Couser, 1934; Trowbridge and Atwater, 1934, p. 71) that:

1) the dolomite unit exposed at Shakopee, Minnesota, lay on the Jordan Sandstone; 2) it was too thin to be the whole Prairie du Chien Group; 3) it therefore must be only the Oneota Dolomite; and 4) there could be no Shakopee Dolomite at Shakopee. Couser (1935) insisted that the dolomite at Shakopee was of "Oneota age" (a statement consonant with the times, when rocks and ages were often used interchangeably). As Couser was Trowbridge's student and had completed some work in 1933, he must be regarded as the source of the similar opinion stated by Trowbridge and Atwater (1934).

This denial of the decades-old understanding of the Shakopee section raised hackles in Minneapolis! Couser's claim, endorsed by Trowbridge and Atwater (1934), went against everything that Winchell and Sardeson had reported and what the Minnesota geologists, particularly Stauffer and G. M. Schwartz (Schwartz, 1936), believed. Stauffer and Trowbridge had been graduate students together at the University of Chicago and were close, intimate friends. They just could not agree on anything.[22, 23]

No "official" rebuttal from Minnesota is known to have been offered, but Sardeson was incensed and responded fully (1934c, 1935e) in an attempt to enlighten the confused people from the south. Because C. R. Keyes regarded Sardeson highly, and also because he disliked the state and academic geologists in Iowa City, he joined the defense of the Shakopee and Oneota (1934, 1935). Apparently because Stauffer himself was not fully conversant with the regional stratigraphy, he could not rebut Trowbridge's work effectively, and Trowbridge "castigated"[22] Stauffer for his belief in the Shakopee, despite their friendship.

At that time, W. M. Furnish was studying the conodonts of the Prairie du Chien Group for his doctoral degree at Iowa. He had conferences with Sardeson before and during his work and found the latter very knowledgeable as to details of that group. Sardeson was "very rational," had a "serious mien," and was "not demeaning to a graduate student."[24] Uncertain of Couser's work, although Couser had done a plane-table survey, Furnish checked out the Shakopee exposures to his own satisfaction and decided that Couser had "messed up,"[24] and that the Oneota Dolomite did indeed underlie the Shakopee Dolomite at Shakopee. Furnish agreed with the Minnesotans, and argued this point with Trowbridge, but the latter would not change his mind. Even though Couser's surveying may have been precise, he very likely was misled by his confidence in the contacts on the driller's log of a new city well in Shakopee, which suggested that the type Shakopee Dolomite lay close to the top of the Jordan Sandstone (Couser, 1935).

Interestingly, Furnish (1936, 1938) found that the microfaunas of the Oneota and Shakopee differed, just as Sardeson had learned from the macrofaunas years earlier, and this reinforced his conclusions about the validity of the Shakopee Dolomite. Academic geologists in Minneapolis

were seriously annoyed by the conclusions of Couser and Trowbridge, and an air of irritation about the denial of the validity of the type Shakopee at Shakopee still hung in the air of the Minnesota Geology Department after World War II!

As with so many "wheels," this one has turned over again, 60 years later! Smith and Clark (1996) reviewed conodonts from the Prairie du Chien Group from Michigan, across Wisconsin, Illinois, and Iowa, to Shakopee, Minnesota. They confirmed Furnish's conclusion that the Shakopee and Oneota faunas are separate (Furnish, 1938), and found that they are unmixed over all that area. Further, the dolomite at Shakopee contains only Oneota conodonts. Evidently Couser was right if, perhaps, for the wrong reasons!

A corollary of the conflict over the Shakopee was Trowbridge's new structure-contour map of the Upper Mississippi Valley region, first announced in 1933 (Trowbridge, 1934a). That map, which does not depict the Twin Cities basin, depended in part on the work of his graduate students, and the map was published twice (Trowbridge, 1934b, 1935). Trowbridge probably missed the Twin Cities basin because of a poor (or incomplete) selection of well records. Thiel and Schwartz (1941) included a copy of Trowbridge's map with their much more detailed and more accurate structure map of southeastern Minnesota. It rather seems that they were having the last laugh at Trowbridge thereby.

THE ST. PETER SANDSTONE

Except for numerous data developed by later workers on thicknesses and by newer methods on the hydrologic and petrologic characteristics, most of what we know of the St. Peter Sandstone in Minnesota is owed to Sardeson. Sardeson's work on the St. Peter Sandstone was the most extensive and intensive of any ever done on that formation in the Upper Mississippi Valley, more so even than Thiel's later detailed sedimentary petrographic study of the unit (Thiel, 1935). The St. Peter is less widely exposed in Wisconsin or Iowa than in Minnesota. It has received exhaustive treatment only much farther south, in Illinois and Missouri. Sardeson's first work on the St. Peter was his discovery and description of some fossils (1892b), as mentioned in the chapter on paleontology. Had he not discovered fossils in the St. Peter, he might never have studied the unit, for fossils surely were his main interest at that time. In numbers of fossil specimens obtained, he succeeded to a degree not matched since.[25] Of course, most persons have not looked for fossils in the St. Peter because they have a pre-conviction that it is barren. Almost barren it is, surely, for even Sardeson was able to find rather few fossils in it.

Sardeson's major paper on the St. Peter (1896a) included paleontology, petrology, and stratigraphy, and was grounded on all previous reports as well as his own years of field work.[26] In a review of that report, Ulrich (1896a) called the paleontological results the most useful, although

he objected to some of the generic assignments. Though many others had earlier found fossils in the shaly zone at the top of the St. Peter (now called the Glenwood Formation), Sardeson discovered more than two dozen species of a varied shelly marine fauna in the sandstone proper in the Twin Cities region. They were present in three crude zones: the upper-middle, middle, and lower-middle parts of the formation. He believed them to be related to Trenton forms, and concluded that the St. Peter correlated closely with the Chazy of New York. Recent work has shown that the St. Peter Sandstone is of different thicknesses and different ages regionally. It is Chazyan in age to the south, in Illinois, but younger in south-central Minnesota. Sloan regards it as Ashbyan or lower Black Riveran there (Sloan, 1987a, frontispiece; 1987c).

Sardeson recognized cross-bedding and fine laminae in the St. Peter where local coloration enhanced their visibility. He must have done some microscopic work, for he recognized euhedral overgrowths on some of the quartz grains, as well as the modest complement of kaolinite in the interstices. On the basis of the marine nature of the now-dissolved shelly fauna, he suggested that the formation was neither a chemical deposit nor a mineralogically pure clastic accumulation, as had been suggested earlier, but that it was diagenetic in origin. That is, a weakly calcite-cemented sandstone was subsequently leached by percolating (presumably meteoric) waters. He suggested further that such leaching accounted for the "supposed" basal unconformity and for local irregularities of the upper part of the underlying Prairie du Chien Group. Sardeson was a little hesitant about his new idea, for he offered it as a theory not proven, but Ulrich (1896a) thought it a worthy, ingenious idea.

Much later, Sardeson (1926b) returned to the St. Peter to consider the sedimentary tectonics and the source of the quartz grains in it and the subjacent formations. He expanded his hypothesis of the origin of the St. Peter by suggesting that the quartz sand had been mixed with calcite when it was spilled into the St. Peter sea, but that the sand came in such abundance and so rapidly that the calcite was never altered to dolomite, as it had been in the underlying Prairie du Chien Group. Because of this and the dominance of quartz, the St. Peter was readily susceptible to the massive leaching he believed had occurred. Sardeson referred to a long-known distinction between the lower and upper St. Peter (1932c); Hall and Sardeson (1892) had noted that the lower St. Peter was faulted locally near gentle folds in the subjacent Shakopee Dolomite. The upper part of the St. Peter had no such faults; presumably there had been some sort of interruption in St. Peter deposition. Logs of some wells drilled in the early 1930s in the Twin Cities area showed a marly or muddy interval between the lower 50 feet and the upper 100 feet of the formation there. At what he considered the same horizon in South St. Paul he found a "marl-rock" zone, with fossils both above and below it; he correlated this interval with

similar divisions of the St. Peter that had been identified in Illinois and Missouri.

Sardeson always objected to the concept of an unconformity at the base of the St. Peter, believing that the irregularity of the contact was the result solely of differential solution of the underlying dolomite. This was an extension of his idea that a quartz-calcite deposit had lost its calcite by leaching. Even by the 1930s, Sardeson had lost this contest, for the unconformity was by then generally agreed upon by knowledgeable geologists, as it is today. The sinkholes in the Prairie du Chien Group are now thought to be too large to be other than subaerial features, although some large sinkholes elsewhere are known not to have been formed subaerially.[27] Doubtless, if Sardeson's hypothesis were true, there should have been additional solution along the formational boundary in later Middle Ordovician time that would have enhanced the irregularity of the contact surface.

On the basis of his extensive observations Sardeson concluded in 1930 that the St. Peter contained *four* faunal zones which together matched the Chazyan interval in New York.[14] Evidently they were the zones Sardeson described (1932c) from several widely separated sites, but the zones are so poorly characterized faunally as to be of little service.

The concept of the Glenwood Formation—the thin (in Minnesota) interval of various mixtures of limy shale, sandstone, claystone, and limy mudstone—annoyed Sardeson (1933d). In that paper and in letters, he denied the need for naming this interval as a separate formation. Partly, he found the type section and type description (Calvin, 1906) carelessly done, but he also pointed out that the unit was too thin to be mappable in Minnesota (it has since proved useful in the subsurface in Illinois, however). Having learned about bentonite a few years earlier, Sardeson believed that the claystones in the Glenwood were bentonite. In those days before ready X-ray diffraction analysis, he could not know that they are not bentonite (Allen, 1929); the clays are not smectites but mostly illite and detrital feldspar, some of which has authigenic overgrowths.[28]

THE GALENA AND MAQUOKETA "SERIES"

The major body of Sardeson's stratigraphic work dealt with the Middle and Upper Ordovician rocks of the Upper Mississippi Valley region, a section he called the Galena and Maquoketa Series. The interval included all the rocks from the top of the St. Peter Sandstone (Glenwood of today) through the Upper Ordovician Maquoketa Formation. His "Galena Series" has subsequently been divided into several named units, among which the name Galena survives for a group of late Middle Ordovician age. Maquoketa remains as a formation (Iowa, Minnesota) or group (Illinois) of Late Ordovician age. The Galena-Maquoketa sequence is the one for which Sardeson developed his innovative series of "beds," characterized

by their fossil content and lithic qualities (Table 4.1). Assessing the work, G. M. Kay (1929), from the State University of Iowa and a student of the Ordovician rocks of the region, wrote, "The best resumé of the beds in Iowa has been published by Sardeson [1907a]."

In fact, Sardeson's innovative approach to the description and classification of the Ordovician beds in the region constituted a small-scale "scientific revolution," in the sense suggested by Thomas Kuhn (1970) in his work on the way scientific knowledge is produced. An old paradigm, in which unit names had been dragged west from New York, Ohio, and Tennessee led only to confusion and wasted words. Sardeson's new paradigm (although he did not think of it in such grand style) promised to organize the local rock column in such a way that it could be correlated with those of other regions with confidence, within the limits of the surviving faunas to indicate chronologic comparisons.

Sardeson ranged from Green Bay to Beloit, Wisconsin, through nearby Illinois, to Dubuque, Iowa, and up to Minneapolis to prepare for the work with Hall (Hall & Sardeson, 1892b) and his master's degree (Sardeson, 1892c), but much of the regional detail was not included in those reports. It was developed in the relevant reports of 1896 and 1897. Sardeson must have worked on this project as early as 1890, for Ulrich mentioned Sardeson's "beds" to Schuchert early in 1891.[3] The work was completed by December 1891, and Hall read their resulting paper at the Columbus meeting of the Geological Society of America that month. The early format of the beds of the Galena and Maquoketa Series (Table 4.1) was published by Sardeson on 6 April 1892 (Weiss, 1997) and also by Hall and Sardeson (1892) that June.

Sardeson (1892c, 1897a) included a thin section of sandy, limy shale and clay shale in the bottom of Bed 1 (Table 4.1) which is now called the Glenwood Formation. In all of his later work on this part of the section, Sardeson objected to the concept of the Glenwood Formation, which he considered part of the St. Peter Sandstone. Compare Table 4.1 with the Frontispiece of Sloan (1987a), Table 2.1 of Sloan (1987b), and with the report by Rice and Hedblom (1987).

In several papers (1892c, 1896e, 1897a, 1897e) Sardeson expressed the concept of a series of beds that can be traced across the entire Upper Mississippi Valley region, including changes of facies as the Decorah Shale grades southeastward to carbonates. The faunas of the beds permitted correlation with those from other regions. The wisdom of this approach was not spelled out fully in the earliest reports, but the reasoning and intent are implicit in the body of the work and in the fact that the beds could be traced over more than 40,000 square miles. Only later did Sardeson detail the folly of the older practice of putting New York or Tennessee names on various rocks in Illinois, Iowa, Minnesota, or Wisconsin, and then trying to correlate them within the Upper Mississippi Valley region by using those

TABLE 4.1. COMPARISON OF SARDESON'S STRATIGRAPHIC COLUMNS WITH CURRENT NOMENCLATURE.

[Notes on facing page]

1	2	3	4	5
92 / 96, 97	Sardeson 1892a Hall & Sardeson 1892b	Sardeson 1896e 1897a 1897e	Sar'n 1897e	Generalized modern equivalents for Minnesota Twin Cities ← → SE Minn.
12 / 14 13 12	Wykoff Beds 50'	Orthis Bed 10' est. / Leptaena Bed 30' / Orthoceras Bed 25'	Wykoff Fm. ["Richmond Ls."] / Maquoketa (Utica) Fm.	Elgin Member of Maquoketa Formation
11		Diplograptus Bed 20'	Maquoketa (Utica) Fm.	Depauperate Zone of Elgin Member
11 / 10	Maquoketa Shale 20'	Triplecia Bed* 30'	Transition Fm.* / Maquoketa (Hudson River) Series	Dubuque Formation
10 / 9	Maclurea Bed 50'	Maclurea Bed 35'	Galena (Trenton) Formation / Galena (Trenton) Series	Stewartville Formation / Galena Group
9 / 8	Lingulasma Bed 20'	Lingulelasma Bed 30-35'		Prosser Formation
8 / 7	Camarella Bed 30'	Camarella Bed 30'		Cummingsville Formation
7 / 6	Orthisina Bed 20'	Orthisina Bed 40'		
6	Zygospira Bed 8'			
5	Fucoid Bed 20'	Fucoid Bed 18-20'	Black River	Decorah Shale
4	Stictopora Bed 30'	Stictopora Bed 20-35'	Beloit Formation[x]	
3	Stictoporella Bed 10'	Stictoporella Bed ?	"Stones River" or Birdseye	Carimona Mbr. / Platteville Limestone
2	Blue Limestone 12'	Bellerophon B. 12-15'		Magnolia M. / Hidden Falls M. / Mc Gregor Mbr.
1	Buff Limestone 15'	Buff Limestone 18-23'		Mifflin Mbr. / Pecatonica Mbr. / Glenwood Formation
	St. Peter Sandstone 160'	Transition / St. Peter Sandstone 150'	St. Peter 150'	St. Peter Sandstone
Σ	285'	308-323'		

names. He gave the synonymy of the myriad names that had been applied to the rocks between the St. Peter Sandstone and the Devonian strata (1896e). Soon afterward, he explained the new principle behind the nomenclature he was proposing:

> For more than fifty years geologists have attempted to apply the names 'Trenton' and 'Hudson River,' in this area with varying results, and unless better uniformity prevails soon, exact correlation with the Ordovician of New York state may be called a failure, and a local classification and nomenclature must be preserved intact. Further, it may be suggested that no one thing will be a greater aid in establishing the desired correlation with other areas than will a locally exact and uniformly applied system of nomenclature under which to assemble the accumulating evidence [1897e].

Nowhere did Sardeson describe the origins of his concept of "beds" that are part faunal zones and part lithic units, but it probably was his own idea. Surely he had it and used it while still an advanced undergraduate student. It may have developed as he studied and worked. He did not get it from Hall, for no such system appears in Hall's published work. He apparently did not get it from Ulrich either, for in the stratigraphic summary prepared for the Final Report of the Winchell Survey (Winchell and Ulrich, 1895) Ulrich cited Sardeson's earliest work of this sort. At the same time, Ulrich included a succession of named "beds" of his own in his table, clearly following Sardeson's lead; his beds corresponded closely to Sardeson's in many cases and he used some of the same names.

NOTES FOR TABLE 4.1

Column 1 shows Sardeson's "Bed" numbers for Column 2 above the diagonal line and for Column 3 below the diagonal line.

Column 2 shows the early version of the Bed sequence (B = Bed).

Column 3 shows the later version of the Bed sequence (B = Bed).

Column 4 shows Sardeson's matching of rock-unit names to the Beds of Column 3.

Column 5 matches modern lithostratigraphic units in Minnesota to the Beds in Column 3. Differences between the Twin Cities and southeastern Minnesota sections are suggested by the inclined contacts and the symbol of lateral equivalence. Only Beds 1–6 occur in the Twin Cities.

The diagram is scaled approximately (1 inch = 58 feet) to the thicknesses in Column 3. The thickness values cited are for Minnesota. The thicknesses in Column 2 are less, but the sum for each column is given at the bottom (Σ).

The fossil names are Sardeson's; at least five of the eight are now in different genera. *Lingulasma* = *Lingulelasma*, for the spelling of the genus was changed.

x = Beloit Formation, named by Sardeson (1896e). In southern Wisconsin, it includes the dolomite equivalents of the Decorah.

* = *Triplecia* Bed = Transition Bed; was named the Dubuque Formation by Sardeson (1907a).

Ulrich also later revised his bed sequence (Winchell and Ulrich, 1897). Ulrich complained that he found that *Orthisina* sp. occurred in *two* zones instead of the one named by Sardeson, but he did not publish that information.[26] Later Sardeson modified that bed (cf. Columns 2 and 3 of Table 4.1). Most of Ulrich's beds (Winchell and Ulrich, 1895, 1897) are congruent with the early ones of Sardeson (Table 4.1), but most have different biozone names (Agnew *et al.*, 1956). Ulrich also started the confusing practice of renaming a bed because its zone fossil had been assigned to a different genus; followed consistently, this practice would have made chaos of the "bed" stratigraphy before long, and points up the difficulty of characterizing rock bodies by their faunas. Ulrich could, of course, have avoided this problem by using only the trivial names. It is perhaps no surprise that T. C. Chamberlin, who always seemed to deprecate Minnesota geologists, complained about Ulrich's correlations (Winchell and Ulrich, 1897) and preferred those of Calvin, the State Geologist of Iowa.[29]

After returning from Germany, Sardeson promptly took up again the project of transforming the classification of the Ordovician section. He urged fellow geologists to learn what rocks they had and then correlate them, rather than the other way around (1896e). He recognized that the varied and ill-fitting nomenclatures already in use in the several states had some basis of truth, which he tried to preserve as he worked toward a regional classification and nomenclature that would serve all. That 1896e report which described the new method was the first of five: other pieces followed as he improved the lithologic details, the faunal features, and the regional extent of his system of beds (Table 4.1, Column 3). Sardeson's avowed goal was describing a succession of rather thin "beds" that might permit the recognition of evolutionary patterns in the fossils (1897a). In this respect he was ahead of his time, for looking for small-scale evidence of evolution was rarely done.

Both faunal and lithic means of diagnosis were more difficult as Sardeson worked southeastward into the Wisconsin Lead District and beyond, for much of the section goes from shale and limestone to limestone and dolomite, and the fossils become fewer and harder to obtain. He described and named the Beloit Formation (1896e, 1897a) to consist of the beds there that lie between the long-established Galena Formation and the St. Peter Sandstone, and then he carried the new name back into Minnesota by following his beds. Beloit thus had priority over the name Platteville Limestone that Bain (1905) applied to substantially the same rocks. Beloit was little used and soon lost favor, except that Beloit is still valid for the dolomitic lower part of the section in south-central Wisconsin. Sardeson pointed out (1907a) that no one had objected to or mentioned the Beloit Formation, and he complained about the introduction of "Platteville" without reference to "Beloit," and about Bain's confusion of the fossils

associated with Bain's Platteville-Galena contact. The name Platteville has overspread the rest of the Upper Mississippi Valley, probably because it was published by the USGS and appeared to carry more "authority." If so, it is a pity, for Sardeson had recognized the priority of rock-unit names in Wisconsin and had avoided overriding them.

Taking the Galena Series as a whole, Sardeson described the regional sedimentational setting. In a 1907 report, he noted the abundant evidence of diagenesis in the Lead District and suggested that diagenesis may have led to the facies changes from the Lead District to Minnesota and Iowa (1907a). Most important, he prepared the first stratigraphic panel cross section of the facies changes; that section included all the rocks from the top of the St. Peter Sandstone (Glenwood Formation of today) to the top of the Dubuque Formation. Among the many subsequent accounts that generally follow Sardeson, the most complete and up-to-date expression of the same cross section is used for the frontispiece of Sloan (1987a).

Sardeson offered a regional picture of the sedimentary tectonics of the Galena limestone, a part of his Galena Series, including the shoreline of the Galena sea, which he appears to have placed too close by 20 miles to the Twin Cities (1927d). His analysis of the facies that survived convinced him that the Wisconsin Arch had remained stable through the Ordovician and had not warped until the Devonian Period. He later reviewed the history of the Galena nomenclature in detail (1937f) and urged that the Beloit be juxtaposed to it, but he provided no new insights. The very last paper (1940) that Sardeson published took up the matter of diagenesis of the Galena limestone; dolomitization and infusion of the lead and zinc in the Lead District were discussed, but little light was shed on the problems. It is puzzling that that paper was also his 89th and last paper in the *Pan-American Geologist*, which continued publication until Keyes' death two years later. A hint at a reason why there were no papers by Sardeson in the last two volumes of the journal may lie in his August, 1944 letter to Bassler: "Even Charles Keyes backed down in his promised support, as I sought to show that young paleontologists are now doing a lot of details and falsifying to a large extent, without solving the real problems as they arise."[30]

Sardeson lamented that his good idea and the name Beloit had come to little.[31] Doubtless other stratigraphers, particularly those with weak paleontological skills, were discouraged by his system of beds, for their use required some sophistication in paleontology. The later USGS policy of maintaining priority in names was not observed in this instance because it had not been established and because Bain published there. Suppression of the name Beloit was simply not proper historical policy, but it happened. Sardeson would have felt somewhat vindicated to know that Agnew *et al.* (1956) and other later experts on the Lead District found that Bain's stratigraphic work there was poor and confused. By then Sardeson was 90 years old and long gone from Minnesota.

SEDIMENTARY PETROLOGY

Sardeson's extensive work on sedimentary petrology was concentrated on the discontinuity surfaces and bentonites in lower Middle Ordovician rocks, the Beloit and lower Galena Formations of Sardeson's "Galena Series," now the Platteville Limestone and the Decorah Shale. He referred to the lower ones of his series of beds (Table 4.1, Column 3) in his reports on intraformational (or corrosion) conglomerates and bentonite beds. He continued to use "Beloit Formation," in preference to "Platteville," in his publications.

Discontinuity Surfaces

Sardeson was one of the first, if not the first, to study diastemic surfaces of submarine corrosion and abrasion, with which thin conglomeratic deposits commonly are associated. Sardeson first called them intraformational conglomerates or corrosion zones (1898c), and later, corrosion conglomerates (1914b). These features are widespread in the Middle Ordovician rocks of the eastern United States and in rocks of the same age in Balto-Scandia. Similar features occur worldwide in Cretaceous chalks, where they are called hardgrounds, because of the cementation of the subjacent surface. Long considered to be the result mostly of chemical corrosion of the compacted or cemented seafloor (e.g., Weiss, 1958), they are now known to be of both abrasional and corrosional origin, following cessation of carbonate sedimentation, and the general term discontinuity surface is preferred (Weiss, 1978). These surfaces are different from surfaces of either non-deposition or subaerial erosion; they are a special case of diastem.

Sardeson described nearly all of the features characteristic of such surfaces: irregular to dove-tailed contacts, truncated fossils in the lower bed, cavities in the lower bed filled with sediment of the upper bed, mineralization of the subjacent rock, blackening of the lower surface from sulfides, and in places black-stained pebbles of the subjacent rock contained in the superjacent rock. He attributed most of their origin to submarine corrosion and overlooked only the fact that the surfaces locally show abrasion. Because of the conspicuous and widespread evidence of corrosion, he coined the term "corrosion zones" (1898c); the column of rock containing the features is not thick (a few centimeters; an inch or so), but "zone" is not inappropriate. He found also that these surfaces seem to lie at the boundaries of some of his beds, and this confirmed for him his belief that his beds, defined mostly on faunal content, were what we would today call sequences. Given the wide area of occurrence of many of these surfaces in the Ordovician rocks of Minnesota and nearby states, he was convinced of their regional significance as stratigraphic marker horizons. He credited Chamberlin with having described one at the top of the Glass Rock (Quimbys Mill Member) in the Lead District (Chamberlin, 1882), but

Sardeson seems to have been the one who figured out the chronological significance of such discontinuity surfaces and applied that to stratigraphy. The features were there; most persons simply had ignored them. They made Sardeson curious and he demonstrated their utility for correlation.

He returned to the subject later (1914b) and drew the distinction between "corrosion conglomerates" and those caused by bottom scour by storms and currents, because pebbles of the subjacent deposit show all the hackly, corroded quality and the mineralization seen in the top of the underlying bed. He also had found that scoured surfaces contained no burrowing fossils, but carried encrusting fossils locally. From these and other features he hypothesized that submarine solution had occurred because of a lapse of input of fine terrestrial sediment. (A lapse of sediment accumulation surely occurred, but according to today's orthodoxy it was a lack of both terrestrial and marine carbonate material.)

Bentonite

When W. A. Nelson announced, in the early 1920s, his discovery of beds of altered volcanic ash in the Ordovician rocks of Tennessee (Nelson, 1922a, 1922b), Sardeson was on the topic like a duck on a June bug. He realized that some of the strange, waxy or soapy clays he had noted in earlier years were also altered volcanic ash deposits—bentonites. He promptly returned to the field and restudied many sections, although he worked mostly in the Twin Cities region. By this field work he demonstrated the occurrence of several such beds in the Upper Mississippi Valley and their relation to other stratigraphic markers. Sardeson thought of corrosion surfaces and bentonite beds as punctuation marks in the stratigraphic chain. His results appeared in a number of papers over a decade; his analysis of the effect of an ash fall on the faunas is discussed in the chapter on paleontology.

Sardeson's earlier papers on bentonites report additions to his knowledge of outcrops and zones as he learned more about their occurrence in the region and also contain some of his ideas about the effect of ash falls on sedimentation and on the biota. He was interested in their petrology and the coincidence or parallelism of some of them to discontinuity surfaces and faunal zones. C. S. Ross (1925) was the first to publicize the chronostratigraphic potential of the bentonite beds. Sardeson soon had better confidence in their correlative power as well (1928f). He received a $200 grant from the new Penrose Fund of the Geological Society of America in 1933 to study bentonites in Minnesota.[32] His report on that work (1934a) correlated bentonite beds from the Twin Cities to southwestern Wisconsin. But because he did not quite realize how useful they would be for interregional correlation, he wrote Bassler that the fossil zones were still indispensable.[32] Sardeson could not have dreamed that they might one day be correlated over great distances by their chemical "fingerprints" (Kolata *et al.*, 1987; 1996).

Sardeson laid a foundation all the same. He located the potassic bentonite clay bed, now named the Deicke K-bentonite, in the Carimona Member of the Platteville Limestone and fitted it into the regional stratigraphic frame, using his system of beds (1924c, 1926d, 1927g). He later did the same for the bed now named the Millbrig K-bentonite in the Spechts Ferry Member of the Decorah Shale (1928f). Regarding these seams, he repeatedly described the swelling and local granularity of the clay and discharge of water, believing the bentonite seams to transmit the water. We now know that the clays are 70 percent illite on the average, and smectites make up only a subordinate fraction at most sites. Such clays swell only locally, where smectite minerals are more abundant, but not nearly as much so as do true montmorillonite-bearing bentonites.[33] The Ordovician K-bentonite clays yield mostly by distortion from the weight of overburden. The granularity arises from the formation of authigenic crystals of K-feldspar (Weiss, 1954), the reason they are now called K-bentonites. The wetness of the seams at outcrops is caused because they are aquitards (confining beds), and formation water seeps out over the tops of the seams.

Sardeson identified the plastic clay beds in the Glenwood Formation, his topmost part of the St. Peter Sandstone, as bentonites (1926c) and suggested that volcanism had had something to do with the onset of Platteville carbonate deposition (his Beloit Formation) (1926d). His view was that some fine sediment was necessary to permit establishment of a benthic flora and fauna that would lead to carbonate deposition. Because there had been shelly fossils in muddy zones in the St. Peter, that idea is not so bad, but terrestrial clays would also have provided a suitable substrate. Sardeson had no X-ray diffractometer, of course, so he can be forgiven for mistakenly reporting bentonite in the Glenwood. By X-ray diffraction, Allen (1929) was able to show that the Glenwood clay was like the regional Ordovician clays. Thiel (1937) showed that there are no smectites (swelling clays) in the Glenwood Formation. Those plastic clays are now known to be illite (Schutter, 1978), but the Glenwood also contains both detrital and authigenic K-feldspar.[28]

Relying on years-old notes, and not returning to the field, Sardeson reported that a bentonite bed lies at the top of the Shakopee Formation locally; it was the lowest one of the five that he described (1934a). D. R. Kolata believes that bentonite does lie on the Shakopee Formation near Hanover, in south-central Wisconsin and at Clayton, in northeastern Iowa,[33] but whether this is what Sardeson meant cannot be discovered. Abundant illite also occurs at the base of the St. Peter Sandstone at Clayton. Of the five bentonites in Sardeson's report, the second is in the Glenwood Formation, the third is the "lower pipe clay" (= Deicke) of the Lead District, the fourth is the "upper pipe clay" (= Millbrig) of the Lead District, and the fifth is a local K-bentonite 8 feet above the Millbrig at St. Paul. The

mistaken second, in the Glenwood, must be deleted from the series, of course. More K-bentonites have since been discovered in the Galena Formation far south of the Twin Cities (Agnew *et al.*, 1956) and also in higher beds (Weiss, 1954; 1957). This is no strike against Sardeson, however; because he just did not work that area in the 1920s and 30s.

Only Sardeson's papers include attempts to work out the succession of the bentonite seams in the Upper Mississippi Valley. Marshall Kay (1931) followed Sardeson's lead when he found the Millbrig bed in Iowa, correlated it with the Hounsfield K-bentonite of New York, and gave it the New York name. Sardeson's papers, although pioneering, were marred by his reiterated complaints that other geologists did not use the name Beloit Formation and ignored his faunal/lithic beds. Similarly, his use of Beloit and refusal to adopt Glenwood tended to separate him from the community of stratigraphers, and this redounded to his own disadvantage.

PETROLEUM GEOLOGY AND MISCELLANEOUS

Sardeson published very little stratigraphic work other than that already described, but the small residue pertaining to petroleum geology and a few other minor interests add a bit to our understanding of the man and his science.

Petroleum Geology

Rarely employed by an oil company, Sardeson played only a small part in developing the theory and literature of petroleum exploration. Yet he did a great deal of work in the "oil patches" as an examiner for the Minnesota Securities Commission. The Midwest, at least, was prey to promoters who wished to sell stock in mineral and petroleum ventures; they worked numerous frauds on unsuspecting or ignorant persons, particularly in rural areas. In the years just before the entry of the United States into World War I, states adjoining Minnesota all had laws requiring licensure of promoters and salesmen of such stocks. Minnesota finally protected itself with a similar law in 1917.

Sardeson's job as examiner was to evaluate the assets and real properties on the basis of which an oil or mining company's stock might be licensed for sale in the state. In the course of this work he visited oilfields in Kansas, Oklahoma, Texas, Colorado, Wyoming, and Montana, at least. Reports were submitted to the Commission, but not published (none survive, in fact). He also testified before the U. S. Senate Committee that investigated the Teapot Dome scandal in 1924.

Sardeson's first paper on petroleum geology—his description of features that he called "cone-domes" (1923a)—was based on his experience in the Kevin-Sunburst oilfield in north-central Montana. His explanation of the structures he called by this curious term is not very helpful. He gave a clearer view of what he meant in his paper on petroleum genesis

(1923d), wherein he explained that cone-domes are sites where the oil-bearing strata are upturned toward the center of a circle or an oval, rather than toward a linear axis, as in the then-prevailing anticlinal exploration concept. Further, he felt that such cone-domes might arise by differential compaction (although he did not use those words) around some resistant or more competent center—such as a reef, a sandstone lens, or even a topographic high of some sort remaining from regional erosion. He called salt domes a special case of cone-domes, but his generic name never caught on. The modern view of the production from the Kevin-Sunburst field is that it derives from Mississippian and Lower Cretaceous beds on noses on the irregular flanks of the Kevin-Sunburst uplift. He claimed to have found cone-domes in Texas, Oklahoma, and Kansas as well (1929e), but his description suggested only domes with very shallow dips, of unknown or uncertain origin. "Domes" in the Shakopee Dolomite (1926a) were local upwarps of a few meters and a few tens of meters across, but they are trivial by comparison to those Sardeson described from oilfields.

Noting the fact that petroleum geologists believed that oil and gas were derived from organic matter in shales near reservoir rocks, Sardeson questioned (1923d) why organic matter is still found in black or oil shales near oil and gas reservoirs, but not in barren porous limestones and sandstones. Perhaps organic shales had less to do with petroleum generation than the organic matter formerly in limestones and sandstones. This was a good question. As organic matter is soluble in water over time, he suggested salt water may have leached organics from calcareous and quartzose sands, the deposits more permeable than shales. He speculated that seismic action may have disturbed oil-and-water emulsions so that globules of oil might have been expelled and concentrated in a reservoir; today this seems a bit on the wild side.

The title "Crude aspects of the petroleum genesis problem" (1923d) must have caused Sardeson to chuckle every time he thought of it. Yet the clumsy pun, not very funny, is typical of the way in which some of Sardeson's later work almost invited neglect. Add to that the fact that his work needed a good and stout editor. Dr. Keyes evidently printed Sardeson's manuscripts in the *Pan-American Geologist* about as he received them, without much proof-reading, and as we have seen Sardeson received no galleys. In short, Keyes ran a high-handed and not very helpful editorial process, whereas Sardeson's imagination needed some sort of rein, and his arguments needed greater clarity. After he ceased (about 1920) to have access to the peer-review process required by the standard journals and by the USGS, his published work deteriorated, more so in stratigraphy than in paleontology. This is much to be regretted, for he continued to tackle new problems, as well as old problems in new ways. The excess baggage of complaint about his lot and the neglect of his work, the frequent sarcasm, and the sometimes abstruse argument and language impeded the

transfer of his knowledge and ideas to other geologists. That he was exiled to the *Pan-American Geologist* for an outlet was not solely his fault, but he also contributed to his professional isolation, and that is very much to be regretted.

Miscellaneous

Sardeson published very little work outside of the Cambrian and Ordovician projects already described. He had once hoped to study the Cretaceous deposits statewide, but was prevented from doing so. His brief adventures into the Precambrian and the later Paleozoic must have occurred because of travels for other purposes, or in response to some published stimulation.

Working the Precambrian basement rocks in the Minnesota River Valley near New Ulm, he discovered a volcanic dike rock cutting the Sioux Quartzite. He also distinguished a quartzite conglomerate having a different attitude from the Sioux in an isolated hill named Redstone, and suggested that the conglomerate "may" be Cambrian in age, by comparison with rock type and attitude in well samples from near Mankato in south-central Minnesota. Time and further work have shown that idea to be incorrect; in addition, the formation names that he applied in his report (1908d) have been rejected. The quartzite is all part of the Early Proterozoic Sioux Quartzite.

A report in a textbook that copper nuggets had been transported from Keweenaw Point, Michigan, to northern Missouri brought a response from Sardeson (1929f). He pointed out that Wisconsin ice crossing Keweenaw Point had stopped in Wisconsin and that older ice from Keweenaw Point had moved eastward. Glaciers that entered Missouri had reached there by crossing the Dakotas and western Minnesota—where no source of copper is known. He suggested that copper-bearing Keweenaw rocks or igneous dikes may have been a source in southwestern Minnesota, but that if they existed they remained covered by glacial drift. The textbook in question may have been Pirsson and Schuchert (1920, 1924), of which he had quite recently received a copy from Schuchert (Weiss, 1997). Writing back to Schuchert,[35] Sardeson attributed the report of the nuggets to a statement on page 120 of Pirsson and Schuchert (1920), but no such statement appears there or in any other likely source. He must have had some other source in mind.

Sardeson's short report on Mississippian and Pennsylvanian rocks of north-central Iowa (1902c) may well have been written from notes made while he was working for the Chicago Great Western Railroad in 1896 and 1897, because the timing is reasonable. Characteristically, he found fossils where the Iowa geologists had failed to do so, and—also characteristically—he took care to make their failure plain.

Sardeson had an enduring interest in Cretaceous deposits, but his one publication on that subject may have developed from his field work on the Ordovician of southeastern Minnesota, where Cretaceous deposits unconformably overlie it (1898b). In that report he reviewed a number of deposits of alleged Cretaceous age, including some in south-central Minnesota, and determined that some were much younger clays associated with the Wisconsin drift, as near Austin in Mower County. Although the large clay deposit in Goodhue County contained Cretaceous plant material, Sardeson pointed out that only a part of the area mapped as clay was truly Cretaceous in age. With that project he was correcting the county maps of Winchell (1884, 1888a). Sardeson's report on the futility of making Portland cement with native Minnesota clays (1923b) harked back to his work on the Precambrian rocks in the Minnesota River Valley near New Ulm (1908d). In that earlier report Sardeson acknowledged Dr. O. C. Strickler, a Regent of the University, and the New Ulm Commercial Association for supporting his field work in the area; it had been a search for limestone suitable for a Portland cement plant. Many years later, Sloan (1964) found Sardeson's published work on the Cretaceous beds there generally accurate.

During the years of working on the state map of Pleistocene deposits with Leverett, Sardeson doubtless learned a great deal more about the Cretaceous beds of Minnesota, especially in the northern and the western districts. Also, wells that penetrated the drift to bedrock were increasing in number, so that he believed he knew a lot about the Cretaceous of Minnesota. As the Pleistocene mapping and description approached completion, Sardeson wrote a lengthy proposal to David White of the USGS, asking that he be permitted to finish up some details and prepare a statewide report.[36] Frank Leverett had even urged the idea upon White at the Toronto meetings of the Geological Society of America. White replied that there was no money for the Cretaceous during the current fiscal year, nor could any be promised for the following year.[37] White said further that he would refer Sardeson's proposal to M. R. Campbell and T. W. Stanton for comment. Both later replied to White that they did not favor the proposal.[37]

BLOCK-FAULTING IN THE ST. CROIX VALLEY

In 1924 Eunice Peterson completed a master's degree in Geology at the University of Minnesota (Peterson, 1924). The theme was stratigraphic, but she discovered some faults in the Cambrian and Ordovician rocks of the lower St. Croix valley. The block-faulted area, in the northeastern corner Washington County, Minnesota, was of special interest because only the Hastings fault, at the mouth of the St. Croix River some 50 miles to the south, was known at that time. Additional field work followed, so that a map of the area and details of the stratigraphic separation

on the faults were published (Peterson, 1927). Miss Peterson was a niece of C. R. Stauffer, a fact that is a key to this story.

Sardeson had not known of Miss Peterson's thesis, but he saw from her published work that she did not agree with his old notes for the aborted St. Croix Dalles folio. He returned promptly to the field to prove her—and, by association, Dr. Stauffer—wrong (1927e). He did recognize that the area is a difficult one—poorly exposed, heavily vegetated, and largely covered by Pleistocene deposits. Nevertheless, he scorned her conclusion and said that the facts could be explained by a succession of glacial erosion and deposition, and by the stoss (shoving) action of ice from the west. He contended that the Decorah Shale in the graben mapped by Peterson (1927) was not faulted down, but had been shoved in by a glacier from the west or northwest. He summarized:

> Correct interpretation of geologic conditions as a whole in this case is as easily made as the wrong one. That a good example of a glacial 'stoss' is called a 'reverse fault,' and a glacially buried old ravine is made to be a veritable 'graben,' is not interpreted by me further, however, as indicative of perversity in the author . . . , but rather the case may be that of a maiden effort [he thought this a capital pun, surely] of the student to be orthodox, geologically. Responsibility in such misinterpretation in such a case, rests, of course, on more than one person. [i.e., her uncle and thesis advisor, C. R. Stauffer].

Stauffer had, in fact, surveyed the topographic base for Peterson's geologic map.

R. E. Sloan and R. K. Quaschnick restudied this area in detail many years later (Quaschnick, 1959). They found that Peterson was substantially correct, though Quaschnick added many details. Attitudes of folds and faults were recorded and the field identification of formations was supplemented by laboratory examination (including heavy minerals and conodonts), something that Peterson had not done. The results of the restudy changed the traces of the faults somewhat (Quaschnick, 1959), but on the whole corroborated her structural analysis (Peterson, 1927). Quaschnick did correct her identifications of formations at a few sites. At the most critical site, Peterson had identified Decorah Shale as lying on Dresbach (Ironton and Galesville) Sandstone. Actually it lies on the St. Peter Sandstone, which is overlain in course by the Glenwood Shale, the Platteville Limestone, and the Decorah Shale. Peterson's mapping of Decorah on Dresbach may have influenced Sardeson (1927) to agree with her identification of the shale on the sandstone and to put forward his idea that the Decorah had been shoved in there by glacial ice. Quaschnick confirmed the scale of the stratigraphic separation on the faults that Peterson

had cited. Mossler and Bloomgren (1990) have endorsed Quaschnick's conclusions. Furthermore, Thwaites (1932, p. 738) reported several large faults in the area and noted that they existed despite Sardeson's objections.

So, Eunice Peterson was vindicated, too late to be of use to her. The regrettable thing, pointed out by Quaschnick (1959), is that she identified the farthest north occurrences of the Middle Ordovician formations in the region, and that they had been ignored by everyone up to 1959. While Sardeson pretended shock at the idea of faults on the "Grand Prairies," he ignored the fact that he himself, in 1893, had recorded several small faults and one with 60 feet of stratigraphic separation in the lower St. Croix valley![38] In his rush to make fun of Peterson, Sardeson (1927e) even denied the validity of the long-known Hastings fault, saying that the illustration of it published by Hall and Sardeson (1892b) showed only a slump block and not a fault! Faults are now known to be common in the region (e.g., Morey, *et al.*, 1981; Morey, 1993).

Sardeson's stratigraphic work was almost entirely on the Cambrian and Ordovician beds and fossils of the Upper Mississippi Valley. Prevented as he was from detailed work on the Cambrian beds by the obstruction of E. O. Ulrich, he concentrated on the Ordovician section. There he established a basis of classification and correlation for all subsequent workers in that region. His greatest contribution was the notion of dealing with the rocks and fossils of the region instead of dubbing this bed or that zone the equivalent of another long known from New York or Tennessee. His long-term significance in this corner of geologic science is attested by the fact that his strategy, new in his day and established against opposition, is now standard practice in stratigraphy.

NOTES

[1]Frederick W. Sardeson (FWS) to Ray S. Bassler (RSB), 23 August 1934, [RSB].

[2]FWS to RSB, 22 June 1952, [RSB].

[3]Edward O. Ulrich (EOU) to Charles Schuchert (CS), 2 February 1891, [YMA, Box 43].

[4]FWS to RSB, 13 December 1957, [RSB].

[5]USGS Memorandum for consideration at a meeting of "Official Geologists" [state geologists], in New York, dated 28 December 1906, [NA, USGS General Correspondence, File 470–128].

That meeting took place at the winter meeting of the Geological Society of America (Yochelson, 1998, p. 460–461).

In an extended bitter exchange with J. C. Branner during 1906 [*Science*, v. 24, pp. 532–537; 692–693; 722–728], Walcott made clear what he thought of professors working part time for the USGS: ". . . men whose first obligation is to a university cannot work as efficiently for the national survey as can the geologists constantly in its employ. . . ."

[6]Charles D. Walcott to FWS 12 January 1907, [NA, File 470-128]. Part of Ulrich's reply (apparently prepared in the third person) to Walcott states, "If priority

and length of service in a certain field confer any rights in same to the person having so served, it is certain that Mr. Ulrich has ample justification for his recent investigations in the Upper Mississippi Valley." So he took the Cambrian of several states for his own property!

[7]FWS to W. Charles Bell (WCB), University of Minnesota June 1952, [WCB].

[8]FWS to EOU 18 October 1924, [NA, E. O. Ulrich's correspondence files].

[9]William C. Alden, Geologist-in-charge, Section of Glacial Geology, USGS, to Samuel Weidman, Geologist, Wisconsin Geological and Natural History Survey, 23 May 1913, [NA, File 248].

[10]David White (DW) to FWS, 5 July 1913, [NA, FWS correspondence, File 298].

[11]FWS to DW, 23 June and 30 June 1913, [NA, File 298].

[12]FWS to C. K. Leith, 15 October 1927, [CKL].

[13]FWS to RSB, 5 August 1932, [RSB].

[14]FWS to RSB, 9 March 1930, [RSB]. In a letter to G.F. Kay, Iowa State Geologist, 19 April 1926, Sardeson stated that "Prof. Van Hise" had prevented Weidman from working with him. Van Hise was President of the University of Wisconsin at the time, but perhaps he did advise his state geologist in the matter. [UIA, Kay Papers, Box K-7, Folder 13].

[15]EOU to FWS, 16 October 1924 and FWS to EOU, 18 October 1924, [NA, File 298] This particularly bitter exchange concerns events during a field conference in Wisconsin, in 1913, and suggests that criticism of Ulrich's Ozarkian System was at the bottom of the affair.

[16]DW to Frank Leverett, 18 July 1913, [NA, File 470-128].

[17]Numerous communications arranging the conference are in File 298, [NA].

[18]FWS to WCB, 29 November 1957, [WCB].

[19]EOU to FWS, 6 November 1913 and 4 December 1913, [NA, EOU's letter file].

[20]FWS to EOU, 14 November 1925, [NA].

[21]FWS to EOU, 15 October 1913, [NA].

[22]William M. ("Bill") Furnish (WMF), Prof. Emeritus, University of Iowa, to the author, 10 June 1984.

[23]WMF to the author, 2 February 1993.

[24]Interview with WMF, by the author, 8 April 1989.

Bill Furnish offered constructive testimony about Sardeson as well in this interview, saying that he (Furnish) had sought advice from many workers in the region before and during his research, and that Sardeson had been very helpful. Sardeson was generous with his encyclopedic knowledge and was cordial to Furnish. Even Furnish's revered Prof. Trowbridge would not help, because he thought conodonts were a waste of time. As Bill summed it up, the two most helpful and knowledgeable people he consulted were Fred Sardeson and Fred Thwaites (University of Wisconsin), "the two cranky outcasts of Upper Mississippi geology."

[25]WMF to the author, 10 August 1996.

Roland Oberg, a student of W. M. Furnish, tried diligently in the 1960s to recover body fossils from the St. Peter Sandstone in Minnesota, but failed, although such are known to occur in the Winnipeg Sandstone and in similar quartzose sandstones of the Roughlock Formation in the Black Hills of South Dakota.

[26]EOU to CS, 7 August 1892, [YMA, Box 44].

Ulrich wrote from Minneapolis, saying "Sardeson has been in the field for a month, and is still there, looking up St. Peter, he says."

[27]WMF, personal communication, 23 July 1996.

He described documented cases of subterranean sinkholes in Arabia: in Cretaceous limestone beneath thick porous sandstone, and in Paleocene gypsum and anhydrite below porous sands.

[28]John H. Mossler, Minnesota Geological Survey, personal communication, 1996.

[29]T. C. Chamberlin to Samuel Calvin, Iowa State Geologist, 26 July 1904, [UC, TCC, Letterbook xviii].

[30]FWS to RSB, 10 August, 1944, [RSB].

Sardeson's last paper was published in April of 1940, but the *Pan-American Geologist* did not terminate until May of 1942, when Editor and Publisher C. R. Keyes died. Sardeson had been welcomed by Keyes for so long! What happened? The answer must lie in Sardeson's remark to Bassler. Evidently, Keyes had tired of paleontological diatribes and Sardeson's persistent "defense" of von Zittel.

In the same letter, Sardeson refers to having been considered "deranged" about 1940, or at the same time that he began seriously to market his collection of fossils. Further, "I gave up my library and put my collections out of sight . . . ." His daughter Marion told me that, tiring of having her father called "crazy," she consulted a lawyer concerning combating such rumors. She was advised to ignore the talk, for a legal action would disturb her and her father even more. He may have been troubled and disagreeably disputatious—as he expatiates to Bassler in this letter—but hardly certifiable. We have seen, too, that he endured some very serious setbacks in his life. Some were clearly uncalled for, such as Ulrich's preventing Sardeson from working on the Cambrian beds of Minnesota.

[31]FWS to RSB, 7 November 1950, [RSB].

[32]FWS to RSB, 6 September 1933, [RSB].

[33]Dennis R. Kolata, Geologist, Illinois State Geological Survey, personal communication, 1996.

[34]Sardeson never mentioned working *for* an oil company in letters or in a publication, but his daughter recalled that he once worked for Sun Oil Company. Record of interview with Marion Sardeson Buyken, 1984.

[35]FWS to CS, 12 November 1929, [YMA].

[36]FWS to DW, 23 October 1915; received at the USGS Geologic Branch on 25 October, [NA, FWS correspondence, File 298].

Filed with Sardeson's letter are two memos. M. R. Campbell [no date] wrote that FWS trivialized the western Cretaceous (simply not true), that the problem was not important, and that "he was looking for work" (certainly true). T. W. Stanton wrote [11 November] that the edge of the Cretaceous was important, but FWS's price was too high, and the benefit was mostly "local."

[37]DW to FWS, 26 October 1915, [NA].

[38]FWS's 1893 field notebook, p. 71–75, [MGS, Notebook 29].

5

GLACIAL DEPOSITS AND PLEISTOCENE HISTORY

Sardeson took an active interest in the Pleistocene geology of the Upper Mississippi Valley for nearly 50 years. During that interval he participated in most of the developments leading to today's understanding of Pleistocene history and deposits in the interior of the United States. Although there is no evidence of Sardeson's interest in glacial matters before he went to Germany, he was born and raised in the Driftless area—an area mostly in southwestern Wisconsin, where the absence of glacial till suggests that it was an unglaciated enclave between glacial lobes. He also encountered many kinds of glacial deposits while studying stratigraphy in Wisconsin and southeastern Minnesota.

In 1894, early in his stay at Freiburg, he was taken into the field by his German professors to study loess deposits in southwestern Germany.[1] He was always proud of his ready ability to recognize different sheets of loess and to map their contacts, to the evident satisfaction and pleasure of his German professors (1929b). It was there also that he adopted their view of loess as a strictly eolian deposit, and he was thereafter a staunch opponent of the theories of an aqueous or dual aqueous/eolian origin of loess. His experience in Germany surely gave him the impetus for his early work on glacial geology.

His publications on Pleistocene materials and history fall rather cleanly into several categories: 1) occurrence and origin of loess, 2) mapping of Pleistocene deposits, 3) evidence for and numbers of glacial stages of the Pleistocene, including the question of an Iowan glacial stage, 4) dating of Wisconsin ice sheets by recession of waterfalls, and 5) histories of river changes and meanings of certain local glacial phenomena. The few that fall outside these categories are discussed briefly at the end.

OCCURRENCE AND ORIGIN OF LOESS

The cold, dense air over ice caps creates strong winds blowing off the ice. As a glacier "retreats" (by melting) it leaves barren glacial till, the poorly sorted rock material brought by the ice. Meltwater will sort some of that till into outwash gravel and sand; the blanket of mixed till and outwash is called a drift sheet. The wind will remove finer grains from both the till and the outwash—fine sand into dunes, silt into broad blankets of loess, and clay-size particles blown so far and wide as to leave no distinct deposit. A century ago many American geologists believed that loess was formed by water, or by both water and wind. Chief among

those were T. C. Chamberlin and R. D. Salisbury of the University of Chicago (the "Chicago school") and Samuel Calvin, the State Geologist of Iowa.

Sardeson brought from Germany the theory that loess was solely an eolian (wind-laid) deposit and was among the first to urge it against the Chicago school. Even so, his first work on Pleistocene history (1897g) did him little credit. He reported that the Driftless area was free of the "northern" drift, but some deposits there suggested local development of glaciers. No one else has confirmed those observations, which suggests that what Sardeson saw was the product of mass-wasting, a concept not well developed at the time. He also studied the loess that mantles the uplands above the Pecatonica River valley in Wisconsin, and he noted the relationship of the loess to deposits of loose St. Peter sand that had been blown up and over the valley walls from outcrops in the valley and intermingled with the loess.

In December 1897, at the Des Moines meeting of the Iowa Academy of Sciences, Sardeson compared the regional loess deposits with those he had seen in the Rhine River valley and described the sorting of till sheets during and following the melting of ice. Both water and wind affect glacial residues, he acknowledged, but their effects were easy to tell apart and clay or silt accumulating on the "modified drift" (in ponds, presumably) was readily distinguished from real loess. (He did not mention the fact that wind may have removed some clay-size grains and carried them wholly out of the region.) Where vegetated, the loess accumulated organic material (the "loess loam") and the upper part of a loess sheet became leached and oxidized. Both changes are easily distinguished from unweathered loess, and none shows the effects of deposition by water.

Sardeson's opposition to the dual theory espoused by the "Chicago school" took courage, for Chamberlin was the most influential figure in Pleistocene geology in the country because of his early work in Wisconsin, the position he held at the University of Chicago, his editorship of The Journal of Geology, his administrative prowess, and the great influence he had as Geologist-in-Charge of the U. S. Geological Survey's (USGS) Section of Glacial Geology. Opposing the older view, for the next two years Sardeson continued working with his mentor and patron, C. W. Hall, on the eolian deposits of east-central Minnesota (Hall and Sardeson, 1899), reinforcing the concept that loess was an eolian deposit and different from the associated water-laid outwash gravels and sands. The two defined the materials about the same as we would today, but they did not suggest a wind direction or specific source for the eolian silt (Hall and Sardeson, 1899c). Sardeson asserted flatly that those who believed loess to be eolian were never misled. They could recognize the local effect of other factors such as slump, or redeposition by water (Sardeson, 1899a). In contrast, those who believed that some loess was partly water-laid had to explain why none occurred in thousands of Minnesota lakes, both extant and dried-

up, and why one loess blanket could lie on the extremes of topographic relief.

The Chicago school had the misleading notion that the dual origin of loess proved that an Iowan till sheet had once existed. They presumed a genetic relation of the Iowan loess to till, thinking the loess was a deposit marginal to the alleged till sheet (Sardeson, 1899a). Their belief in an Iowan till reinforced the need for loess to be partly aqueous in origin; the two beliefs supported each other, circularly. By contrast, Sardeson viewed the Iowan loess blanket as lying on top of and beyond a till sheet or sheets, rather than lying only at the margin of any such sheet (1899a). This persistent demonstration of the solely eolian origin of loess undermined the argument in favor of an Iowan till sheet.

In this work Sardeson described a process of alteration of post-glacial surfaces that is still underway. He argued that the weathering and washing of till yields several sizes of sediment particles, including silt, some of which may be blown to form loess. The water-laid and the eolian deposits of silt are readily distinguished in the field. Because loess is highly permeable, thick deposits will stand in steep bluffs ("bluff loess") rather than washing down. As a loess blanket ages and weathers the upper part is leached of lime and oxidized, and may become loamy, but may remain fresh at depth. The two parts are easily distinguished with acid. The depth of leaching and oxidation is related to the original thickness of the deposit, the amount of vegetation on the surface, and time. The deep soils of the upper part, the "loess loam," had been called sedimentary, i.e., water-laid. Sardeson asked why are they not calcareous, as are the known aqueous silty deposits in ponds and lakes?

Sardeson suggested (1899a) that Professor Chamberlin had almost adopted the eolian hypothesis in 1897. Indeed, in a criticism of Sardeson's earlier paper (1897g), Chamberlin (1897b) had alluded only to the casual haste with which Sardeson had done his work and not to Sardeson's views on the loess. But Chamberlin (1897a) had, to the contrary, reasserted the hypothesis of dual origin of loess elsewhere in that same number of his journal.

Chamberlin still believed in the dual-origin hypothesis in 1906 when he, Sardeson, and others met on a field trip. Sardeson (1929b) was "drafted" to accompany Chamberlin and a group of federal geologists led by Frank Leverett. Members of the group were well informed on the glacial deposits of the region, but some also were infected with the belief in the dual origin of loess. Sardeson was genuinely glad to be able to learn from such a distinguished group, but abashed by their "ill concealed amusement" when he acknowledged knowing only the Minnesota glacial geology about which Winchell had written much earlier. The party traced the "scalloped" edge of the loess from Wisconsin through Minnesota to Iowa, remaining always outside the margin of a till sheet. With smug satisfaction, Sardeson (1929b)

then showed them "islands" of loess *within* the perimeter of that same till—patches of loess lying *on* that till. It was a moment of triumph for Sardeson's views, for believers in the eolian-plus-aqueous theory in the party were at a loss to explain those patches. Even so, none of the party were converted at that time apparently, although Leverett (and Calvin) later supported the eolian origin of loess.

In subsequent years Sardeson returned to the eolian genesis of loess a number of times to pillory the political urge that had led the Chicago school and Calvin to establish a till sheet named "Iowan," and to cling to the fiction that the Iowan loess was marginal to, and genetically related to, a till sheet.

MAPPING OF PLEISTOCENE DEPOSITS

As Americans developed the Midwest in the 19th Century, it began to be apparent that knowledge of the type and distribution of unconsolidated Pleistocene deposits was important—to agriculture and development. Mapping of bedrock and economic deposits had been done for decades, of course, but mapping of glacial deposits was a development of the late 19th Century. Sardeson produced some excellent work in this genre, beginning very early.

The St. Croix Dalles Quadrangle

As a consequence of yet another entanglement with T. C. Chamberlin, Sardeson's first mapping project was not published. C. W. Hall had worked for years on the St. Croix Dalles 15-minute quadrangle, and Sardeson had worked on the bedrock paleontology there. Hall offered a map and draft text for "preliminary" review to the USGS in July 1904.[2] The preliminary review never took place because C. W. Hayes (Geologist-in-Charge of Geology) never got to Minneapolis to see their work that fall, as he had planned. So the Hall and Sardeson draft and preliminary drawings were submitted to Hayes in March 1906, for the USGS folio series. Hayes passed them along to W. C. Alden, E. O. Ulrich, and Bailey Willis of the USGS for review. Alden objected to their treatment of the Labrador and Keewatin ice sheets and their tendency to "agree with Berkey [1897]," with whom Rollin Chamberlin (1905) disagreed. He also raised the question of T. C. Chamberlin's prior right to study the area. Ulrich carped about their draft's organization, form, choice of words, and formation descriptions, but said that the map was acceptable. Willis suggested that the manuscripts be referred to T. C. Chamberlin for review because most problems in the draft text related to the Quaternary deposits, Chamberlin's specialty. Willis suggested further that Hall and Sardeson might be asked to complete the work under Chamberlin's direction.[2] Chamberlin had been in charge of glacial geology for the USGS for years, but had been replaced in that post in 1904 because USGS Director Walcott wanted the section chief in Washington, not Chicago.

The result was unhappy for Sardeson and infuriated Hall. Hayes took Willis's advice, and T. C. Chamberlin, agreeing with the comments of the three reviewers, said that nothing those authors could do would be likely to make the product acceptable to the USGS. Chamberlin had great standing with the USGS. But perhaps the crucial factor, as Chamberlin reminded Hayes, was that he had asked Hayes in the summer of 1905 to reserve the Taylors Falls quadrangle for his son, Rollin,[2] and Hayes had agreed to the reservation. So, Hall and Sardeson were out, partly because Hayes had not realized that Taylors Falls and St. Croix Dalles were two names for the same quadrangle! A triangular correspondence (Hall-Hayes-Chamberlin) continued for some months, and even involved USGS Director Walcott, as Hayes and T. C. Chamberlin attempted to craft a face-saving way out that would satisfy Hall. Hayes and Chamberlin suggested that Rollin Chamberlin and/or Hall and Sardeson might prepare a bulletin or professional paper for the USGS, while Hall complained about the harsh treatment of a preliminary work he had submitted in search of editorial suggestions. Hall finally ended the matter by withdrawing the manuscript in February of 1907, and no folio was prepared. Rollin Chamberlin (1905, 1910) published his results as journal articles. However, because of this unseemly episode, Walcott did undertake to improve the communications between the federal and the several state surveys in order to prevent such misunderstandings of purpose and "turf."[3]

Sardeson made another attempt at a St. Croix Dalles folio several years later, under different circumstances. Apparently at the instigation of Samuel Weidman, of the Wisconsin Geological and Natural History Survey, he and Sardeson sought support from the USGS in 1913 to prepare that folio. W. C. Alden, Geologist-in-Charge of the Section of Glacial Geology, sent a contract (pending funding) to each of them. Frank Leverett supported their plan strongly,[4] and Rollin Chamberlin also offered his support. The work was to be done under Leverett's direction, through the 1913 season and ending 30 June 1914. However, Chief Geologist David White soon wrote that the USGS could not afford to support and publish that project.

Some years later Sardeson completed USGS Folio 201, on the Twin Cities area (1916), and Folio 210, on the Pleistocene deposits of west-central Minnesota (1919). The manuscripts of those folios are in the archives of the USGS[5] in Denver, and they carry only minor criticisms from reviewers. By that time, of course, Sardeson was a seasoned author and also had completed his work of mapping Minnesota with Frank Leverett. Surely it was Hall's part of the St. Croix Dalles manuscript—the Pleistocene geology—that had been most criticized by the USGS. Even so, that Weidman and Sardeson were barred from completing the St. Croix Dalles folio suggests some political interference at the USGS, for 1913 was the same year in which Ulrich had prevented those two men from working

on the Cambrian beds. Sardeson's two published folios are major contributions, but they pale by comparison with the three great map sheets and companion bulletins by which he and Frank Leverett described the surficial deposits and soils of the entire state of Minnesota.

The Minnesota State Map Sheets

Between 1906 and 1915 Frank Leverett and Sardeson finished the field work for three detailed maps of the Pleistocene deposits of Minnesota—printed as the northwestern, northeastern, and southern parts of the state. Leverett had begun regional mapping of Minnesota's glacial deposits in 1906 for the USGS.[6] Sardeson joined him on a temporary basis for a month in the fall of 1909 and for the summer of 1910.[7,8] They already knew each other, probably from the 1906 field conference on the loess of the region. In 1911, Sardeson was commissioned USGS Geologist No. 67, at $7 per day and expenses, in the "Glacial Section,"[9] and he and Leverett worked together in the summers of 1913, 1914, and 1915 on the state maps.[8] In 1912 he worked on USGS Folio 210 in western Minnesota.[7] He remained on the USGS roll until 1924, but did no field work—and had no allotments—after 8 December 1915.[9]

The Minnesota Geological Survey (MGS) had shared the cost of Sardeson's work for Folio 210, but it did not pick up his expenses for the statewide work until 1912 (Leverett, 1915). The MGS also bore the costs of printing their maps and reports (Leverett, 1914, 1915; Leverett and Sardeson, 1916, 1917a, 1917b, 1919). Sardeson worked with Leverett from 1909 through 1915, and their collaborative editorial work continued long after that period.[1,10] Two of Sardeson's field notebooks, for 1913–1915, are still preserved.[11] Sardeson also took credit for "inviting" Leverett to Minnesota to map the glacial deposits; perhaps he had suggested it during the field conference on the loess in 1906.[10]

When W. H. Emmons joined the University of Minnesota and the MGS in 1911, he promptly sought and received both financial and scientific cooperation from the USGS. He dismissed Sardeson in 1913, while the latter was involved in the federal mapping of glacial deposits. He allotted $800 for Sardeson's work that July, August, and part of September, knowing Sardeson was to be fired within days.[12] State support for Sardeson's work after that September was provided, apparently because Leverett specified Sardeson for his assistant. That Emmons kept Sardeson's name as co-author off of the first map and bulletin (Leverett, 1914, 1915) of the Leverett-Sardeson series seems to have expressed his personal desire to have nothing to do with Sardeson, but sheer meanness may have played a part as well. Sardeson complained of the omission to USGS Director, George Otis Smith, and as a result he was included as co-author on the four remaining parts of the series.

Sardeson worked alone all his life, with two exceptions—his first geology teacher, C. W. Hall, and Frank Leverett. Solitary work was not

uncommon before the days of teamwork and multiple authors, but Sardeson's strong personality and habitual sarcasm may have discouraged possible co-workers. He felt genuine respect and affection for Hall and high personal and professional regard for Leverett. While he and Leverett were working near the Twin Cities or were enroute to or from the field, the Leveretts stayed at Sardeson's home, Sardeson's daughter Marion recalled, and Mrs. Dorothy Leverett stayed there for extended periods—even a summer—while the men were in the field. Marion remembers vigorous after-dinner discussions of geological problems, when the two geologists pencilled maps and sketches on her mother's linen tablecloths, much to that lady's distress![13]

From Leverett, Sardeson learned a great deal about regional glacial stratigraphy and glacial processes. At that time he was also carrying forward his work on the folios (Sardeson, 1916, 1919), and thus his knowledge grew rapidly. H. E. Wright, Jr., dean of upper Midwest Pleistocene geologists, suggested that Sardeson's participation in the state mapping was minor (Wright, 1972, p. 516). Actually, Sardeson's notebooks in the USGS archives show that in the 1913–1915 field seasons alone, he worked in 34 counties of Minnesota in the southwestern, southeastern, central, east-central, and northeastern areas of the state.[11] Leverett (1929) acknowledged Sardeson's assistance in northeastern Minnesota. Thus Sardeson seems to have worked more widely in the state than was suggested by Wright. His experience in northern Minnesota with Leverett was extensive. They set up headquarters in Duluth several seasons (1909, 1910, and 1913–15), and from there they leveled (surveyed) tilted beaches of Lake Superior and the larger lakes of northern Minnesota. Sardeson himself ranged widely along both shores of Lake Superior.[8]

Studies of the Pleistocene deposits of Minnesota increased in number following Leverett and Sardeson's work, and almost exponentially after World War II (e.g., Goebel, 1976). It is interesting to compare the older maps with the one published about 60 years later (Hobbs and Goebel, 1982), although it is not possible to see an evolutionary development from the older map sheets to the new (a single sheet, but at the same scale). The advent of Carbon-14 dating revolutionized glacial stratigraphy in a way that Leverett and Sardeson could not have dreamed, so that any newer map is certain to be different from theirs. The older maps were designed to display agricultural conditions in addition to the geology, and evaluating the suitability of the various soils was an important purpose of the symbols used on them. In fact, in later years Sardeson referred to that work as soil mapping. The modern geologic map, in contrast, is designed to show glacial stratigraphy and history, without reference to soil conditions.

On the older and the modern maps, there is gross similarity in the glacial stages represented; both maps distinguish deposits of the Kansan, Illinoian, and Wisconsin glacial stages. However, the Wisconsin was not

labeled as such by Leverett and Sardeson, and it was partitioned into four lobes and 16 moraine associations by Hobbs and Goebel (1982). It is interesting, also, that Leverett and Sardeson mapped only those three stages, although they discussed a putative fourth [the "Iowan"] in the texts that accompanied the map sheets.

Some clear differences between the Leverett/Sardeson and Hobbs/Goebel maps can be discerned by comparison of the two maps and reference to certain sketch maps published later by Sardeson (e.g., 1935a). The Superior lobe of glacial ice is now known to have moved much farther southwestward, into central Minnesota, than Leverett and Sardeson had imagined. Hobbs and Goebel (1982) mapped no dune sand, either along the eastern margin of the middle Mississippi River drainage or on the Anoka sandplain; they included it in outwash because the dunes there are not imposing and consist of outwash that has undergone some deflation and redeposition by winds. Much of the Boundary Waters Canoe Area Wilderness of northeastern Minnesota was mapped as bedrock by Leverett and Sardeson (1917b), but the newer map shows drift deposits over much of that area. Surely access to this region was very difficult for Leverett and Sardeson, and it was an agricultural wasteland as well. Whereas the older map showed much bedrock in southeastern and southwestern Minnesota and along the Minnesota River, the new one shows colluvium and loess in the southeast and the adjacent till or outwash in the southwest, and it indicates by symbols that some of the terraces along the Minnesota River expose bare rock. Here again we see the philosophical difference between the two mapping projects, the older having agricultural and land-use objectives. Leverett and Sardeson mapped much greater widths of outwash trains along the major rivers than did Hobbs and Goebel (1982).

The Published Geologic Folios

In contrast to Sardeson's lack of success with the St. Croix Dalles folio, he prepared and published two important USGS folios in Minnesota. His Minneapolis–St. Paul Folio No. 201 (1916) contained topographic and geologic maps of four 15-minute quadrangles (scale of 1:62,500): Anoka, Minneapolis, St. Paul, and White Bear. Little bedrock was exposed in the area, and Sardeson was already familiar with the Ordovician formations and biozones. The map was almost wholly of Quaternary deposits that were shown in exquisite detail and complexity. That and his many references in the text to exposures "too small to map" suggest that this work was carefully and thoroughly done.

As preparation for this job, he had written on the eolian materials with Hall (Hall and Sardeson, 1899), shared the St. Croix Dalles work with Hall, worked with Leverett, and extensively revised Winchell's analysis of the history of St. Anthony Falls and the time of the withdrawal of the last ice sheet (1908a). The story of the falls in the folio was taken directly

from that earlier work, a study that is remarkable for its careful detail and precision of marking the changes in the Minnesota and Mississippi Rivers and the stages of the falls recession. Sardeson was able to work on the problem before urban construction had overwhelmed the margins of the rivers and otherwise altered the rivers' banks; his work probably could not be reproduced today.

Sardeson's Folio No. 210 (1919) dealt with the glaciated plains in west-central Minnesota, the area of the southeastern shore of Glacial Lake Agassiz; the field work was done in 1912. The folio contained topographic maps and geologic maps of four 15-minute quadrangles (scale of 1:62,500): Barrett, Chokio, Herman, and Morris. No bedrock was exposed in the area, but a number of wells reached bedrock, so that Sardeson had some idea of the subglacial terrain and the thickness of the drift. The cost of publishing this folio was shared by the USGS and the MGS.[14] The Twin Cities Folio seems to have been paid for only with USGS funds.

This western area is geologically rather simple, with few map units, but the care and precision characteristic of Sardeson's mapping are evident in the detail illustrated. The area was all drift covered, but much of that had been modified by outwash and by Lake Agassiz, which left lake-bottom and lake-margin facies and some beaches. Ground and end moraines, outwash, and kames were mapped in addition to the lacustrine features. Sardeson was able to track preglacial drainage lines as well, by well data and surface features of the drift and outwash. His history of the Pleistocene deposits was quite detailed, as was his discussion of the types of soils developed on them. Here, as in his work with Leverett, soils were indicators of some types of the glacial deposits.

Comparison of the folio maps with the state map of Hobbs and Goebel (1982) shows that Sardeson's work on the folios has stood the test of time well, though the difference in scale makes direct comparison difficult. Only one major difference shows—the newer map does not distinguish dune sand on the Anoka sandplain.

GLACIAL STAGES OF THE UPPER MISSISSIPPI VALLEY

Sardeson (1911 through 1939f) held to the now-accepted view that four stages of ice advance had sculpted the Upper Mississippi Valley region and, further, that the most recent ice sheet had come, nearly simultaneously, from three different sources: Keewatin in northwest Canada; Patrician north of Lake Superior; and Labrador. These concepts were codified in MGS Bulletins 12, 13 and 14, the texts that accompanied the maps of Pleistocene deposits that Leverett and Sardeson had prepared. The till-sheet names used in those reports (Leverett, 1914, 1915; Leverett and Sardeson, 1916, 1917a, 1917b, 1919) were a compromise between an older classification, urged by T. C. Chamberlin and others, and a newer trend embodied in

Sardeson's later works on the subject. The general scheme in the Minnesota mapping reports is given in Table 5.1 in stratigraphic order, with the source regions named. The oldest till sheet, the Nebraskan, is not exposed in Minnesota and adjacent parts of Iowa, and was never treated in the Minnesota reports.

A great loess blanket [the Iowan Loess] lies between the Wisconsin till lobes and the older lobes. The names of the two older till sheets were changed over time, according to various views of the Iowan(?) stage. Some years later, as we shall see, Leverett withdrew support for an Iowan glacial stage, and the four main stages became, in ascending order, the Nebraskan, Kansan, Illinoian, and Wisconsin, the four state names used by Sardeson (1922b). Whatever names he or others used, Sardeson always spoke of the four main stages as an "older gray till," an "old gray till," a "young red till," and a "younger" body of tills that mapping showed to have been deposited by the three lobes of the Wisconsin ice. In this way he avoided the sectarian squabbles over the names and followed Weidman (1907), who used numbers for the till sheets and glacial stages.

Sardeson's "older," "old," "young," and "younger" system recalls the one he had used three decades earlier when he tried to describe the Lower Paleozoic stratigraphy of the Upper Mississippi Valley objectively and to use local terms for the rock units, rather than to import a sequence of names from New York and elsewhere and force the local strata to fit those names. The use of state names and, particularly, the changing of those names always troubled Sardeson. He poked fun at those who wanted the name of their own state attached to a Pleistocene stage—which he could easily do because the name Minnesota had never been proposed for such!

Regarding the possible number of major till sheets, Sardeson pointed out (1927b) that wads of different till beneath a drift sheet cannot prove the existence at that place of an additional, older drift sheet. Many such wads are of till displaced locally from either the superjacent or the subjacent till sheet; evidence of an intervening till sheet must be regional. He discussed till sheets east and west of the Midwest and their stages (1928d); but as he had no secure correlations from those regions to the Midwest, he did not advance the science. He did repeat his earlier suggestion that any till sheet older than those in the Midwest must lie in the Canadian Cordillera (1927b). He prepared an excellent account of the origin, spread, and wastage of a North American ice sheet, including the recognition of its melting at the bottom except near the periphery (1929c). Therein he suggested the hypothesis that the necessary massive accumulations of snow had begun first in the Canadian Cordillera, and that the effect on high-latitude weather had caused similar accumulations progressively farther east. This seems to have been his basis for thinking that an "oldest" till sheet must lie in western Canada.

TABLE 5.1. STAGES USED BY LEVERETT AND SARDESON.

Wisconsin—Gray Keewatin; red Labrador (Superior); red Patrician.

Illinoian—The "old red" Labrador.

Iowan(?)—Tray Keewatin in southeastern Minnesota and adjacent Iowa; thought by some to be a phase of the Illinoian ice advance.*

Kansan—Gray Keewatin, both loess-covered and loess-free.

*This stage, mentioned in their texts, does not appear on the maps that accompany these texts because those authors did not believe in the validity of an Iowan drift sheet, as described below.

Sardeson tried (1930c, 1930d), as had others, to develop a series of relative time intervals between glacial advances, but before the development of Carbon-14 dating it was an exercise in futility. Recognizing that Rollin Chamberlin had discovered the older red till sheet (Illinoian) in 1905, Sardeson undertook to compare and contrast it with the red Patrician drift (Superior lobe) of Wisconsin age (1935a). Like others before him, he noted the copper nuggets from the Upper Peninsula of Michigan incorporated in the Patrician drift, but did not bring the occurrence of nuggets to bear on the comparison of the two red drifts. This paper therefore made no real contribution to glacial history.

Question of an Iowan Glacial Stage

For a long period some workers had urged acceptance of a fifth glacial stage, younger than the Illinoian and older than the Wisconsin. T. C. Chamberlin (1895a) had named a till, first described and mapped in northeastern Iowa by McGee (1891), the "East Iowan." Working in southwestern Iowa, he considered it to be the second (next to oldest) sheet, lying upon the Kansan, and soon simplified the name to "Iowan" (Chamberlin, 1895b). Then he moved the name Kansan to the second drift (covering northeastern Kansas) and the name Iowan to a younger drift, supposedly the fourth (Wilmarth, 1938), and added Illinoian, from unpublished reports by Frank Leverett, for the third (Chamberlin, 1896)! Chamberlin postulated that fourth till sheet, beneath the Wisconsin till (the fifth), because of his belief in the aqueous + eolian origin of loess. With such a start, it's no wonder the term "Iowan" has had a checkered career!

The concept of a drift sheet distinct from both the Illinoian and Wisconsin was proposed by Beyer (1897), Finch (1897), and Calvin (1897a), and it was named Iowan by Calvin (1897b, 1897c) because he presumed it to be the fourth drift sheet postulated by Chamberlin in 1896. The concept was defended thereafter by Iowa geologists and T. C. Chamberlin. Samuel Calvin, State Geologist of Iowa, was its staunch supporter (Calvin, 1897d,

1899) until his death. After 1896, the alleged Iowan drift was always considered younger than Illinoian and older than the Wisconsin, without regard to numbers of drift sheets.

In this controversy, once again, Sardeson and Chamberlin were on a collision course. According to Sardeson, two errors led to the concept of an Iowan till sheet between the Illinoian and Wisconsin tills (Sardeson 1922b, 1926e, 1929b, 1930c, 1930d, 1932a, and 1939f). The first error was what Sardeson said was the mistaken identification by Iowa geologists of the spoil from the Chicago Great Western Railroad cut near Oelwein, Iowa as an exposure of a valid till sheet. Sardeson knew the outcrop well, for he had worked for that railroad during the summers of 1896 and 1897, while the Oelwein cut was being made.[7,15] He claimed that the vegetated spoil, from two older tills, became the till that was "calcareous to the grass roots" (Calvin, 1897b, 1897c). It was thought to lie thinly over the region and to harbor giant granitic boulders (Calvin, 1899). The second error was the Iowans' assumption that loess had a mixed eolian and aqueous origin. The blanket of loess overlying the Illinoian till sheet was thereby erroneously considered evidence that a till sheet coeval with and related to the loess lay buried somewhere under northern Iowa and southern Minnesota (e.g., Calvin, 1899).

Calvin believed early on in the dual origin of loess, but he seemed to espouse both the dual and the eolian origins in his first major paper on the Iowan glacial stage (Calvin, 1899). In 1902, he argued cogently and forcefully that all loess is eolian.[16] He was by that time, however, convinced of the reality of the Iowan drift, so had no further need to support a dual origin of loess. He still believed in that drift, however, saying that the interglacial loess is "intimately related" to the Iowan drift (Calvin, 1911).

The notion of a mixed origin of loess lost favor with glacial geologists before World War I, and removed the second basis of support for an "Iowan" drift sheet. Frank Leverett worked out the difficulties in northeastern Iowa that had long been cited as evidence for the "Iowan" (Leverett, 1909, 1910, 1939). Leverett was an early employee of T. C. Chamberlin, his assistant and professional disciple, and he inherited Chamberlin's mantle of leadership in the Pleistocene geology of the Midwest. Partly because of this, certainly, Leverett did not give up the Iowan drift early or easily, although he early suspected that it did not exist (Leverett, 1909). While he deferred then to the judgment of the Iowa geologists insofar as their own terrain and publications were concerned, he ultimately did show that no drift sheet lies between the Illinoian and the Wisconsin sheets, something that Sardeson had claimed for years. Sardeson (1929b) credited Leverett's skill in observation and persistence in field work with developing the evidence that led him to change his earlier opinion.

The other alleged error in the reasoning of advocates of the Iowan drift took longer to be resolved. The first published reports by Iowa

geologists on the type outcrop of the Iowan do not suggest the incompetence that Sardeson blamed on them (Calvin, 1897a; Beyer, 1897; Finch, 1897). T. C. Chamberlin was bitterly contemptuous of Sardeson's oft-repeated claim that the Iowa geologists mistook a spoil heap for a drift sheet. Chamberlin pointed out that the spoil in question had been thrown well back from the cut so that, in contrast to Sardeson's claim, such confusion would have been impossible.[17] Chamberlin apparently didn't care to face Sardeson respecting this issue, for in field conferences in Iowa and Minnesota in 1915, Chamberlin deliberately arranged to avoid Sardeson. Chamberlin went out with Leverett, and Leverett (at a different time) joined W. C. Alden and Sardeson (Smith, 1915)! As we shall soon see, the allegedly non-existent "Iowan drift sheet" was an erosion surface, so that both Chamberlin and Sardeson were partly correct.

From 1925 into early 1934, Sardeson wrote intermittently to George F. Kay, Iowa State Geologist. Two themes dominated his letters: arguments against the Iowan drift and requests for employment by Kay. He urged Kay to give up the Iowan, saying that Kay's publications merely papered over the errors of his predecessors (mainly Calvin and Chamberlin). In nearly every letter he asked for support for cooperative field work in southern Minnesota and northern Iowa, in order to resolve the differences in the bedrock and surficial maps of the two states—including, of course, expunging the Iowan drift. Kay replied, graciously, to about one third of Sardeson's letters, some of which were truly insulting to Kay and his staff. Kay always seemed to have some budgetary impediment that prevented employing Sardeson![18]

Resolution of the Iowan

Argument over the "Iowan drift sheet" was longer, perhaps, than any comparable controversy over any other midwestern glacial concept; certainly the debate was bitter. As matters have stood now for about 30 years, no Iowan till sheet is recognized in the midwestern Pleistocene stratigraphic column. One might say that Sardeson had been vindicated, but that would be too simple; current reasons for excluding the Iowan are not the same as the ones that Sardeson championed. Several dozen reports are required to track fully the evolution of the Iowan concept, but a few trace the essentials of the story (Kay and Apfel, 1929; Ruhe, 1969; Wright, 1972; White, 1973). A much-condensed account follows here.

The first published hint that this alleged drift sheet might be either a weathered surface or drift belonging to either the Illinoian or the Wisconsin came from Leverett (1909), although he deferred to the Iowans. Soon after, Leverett (1910) suggested that the Iowan drift was possibly of Illinoian age. In that report, he was actually comparing European glacial stratigraphy with that of North America, and he may well have been tempted to fit four stages in the Midwest to the four then already established

in Germany. Even so, he concluded that the features typical of the Iowan were only the weathered surface of a drift sheet—the same conclusion to which Ruhe came 60 years later (Ruhe, 1969)!

Chamberlin (1910) demurred, and Calvin (1911) blew up, but Leverett (1913) persisted. He asserted that the putative Iowan was not separated from the Illinoian by a "definite interglacial stage," but was instead a late substage of the Illinoian. Leverett and Sardeson (1919) sidestepped the issue diplomatically (Table 5.1), for they were reporting on deposits in Minnesota and wished not to offend the Iowa Geological Survey; even so, their lack of confidence in an Iowan drift showed in their language. Leverett (1926) equivocated regarding a fifth glacial stage to be called Iowan.

In his history of glacial studies in Iowa, G. F. Kay, who had succeeded Calvin as state geologist, came out four-square for the Iowan drift (Kay and Apfel, 1929). Although Leighton (1931) concluded that the Iowan was not much separated from the Wisconsin and removed its name from his stratigraphic table, he soon reinstated the term Iowan, but he made it the earliest part of the Wisconsin (Leighton, 1933). Leverett (1932) was less definite about the Iowan, perhaps because he was again writing mostly about Minnesota, but he did come down hard on the notion of an Iowan drift that could be defended on the basis of an Iowan loess, because whatever their names, the alleged Iowan drift and alleged Iowan loess were not the same age.

Leverett remained opposed to the Iowan as a separate glacial stage, but he vacillated as to the true position of the surface represented by the name. When he was no longer active in field work, and doubtless writing from his memory and old field notes, he never returned to his own suggestion of 1909 that it might be only an erosion surface, but instead wrote that the Iowan was closely related to the Illinoian (Leverett, 1939), that it was probably Illinoian (Leverett, 1942a), and that it was a Keewatin product of early Wisconsin age (Leverett, 1942b). Ruhe (1969) gave the coup de grace to the Iowan drift when he wrote, "[It] does not exist." Rather, he held, it was an erosion surface cut onto Kansan till. Today it is still so regarded, and it is codified as the Iowan erosion surface (IES); see for example Bettis and Kemmis (1992).

After berating the concept of the "Iowan" drift for years, and after Calvin, Chamberlin, and other older protagonists had died, Sardeson (1939f) suggested a change in naming that preserved the name Iowan, but not for the alleged till sheet against which he had so long argued. He pointed out the numerical priority of publication of the five most-used names for the midwestern glacial stages, Illinoian (4), Iowan (2), Kansan (3), Nebraskan (5), and Wisconsin (1). The name Nebraskan had been proposed by Shimek (1909) from southwestern Iowa, but without proper correlation with the older work already completed in northeastern Iowa. Sardeson reminded readers that Chamberlin had moved the name Kansan in 1896 from the

oldest till sheet to the second oldest and concluded that the proper sequence of names was Kansan, Iowan, Illinoian, Wisconsin. Sardeson's two principal papers on the subject (1929b and 1939f) contain extended descriptions of the evidence, as well as some relevant personal history.

It is unlikely that a complete history of the name Iowan and its applications would exist but for Sardeson's work. That he was able to publish those papers is owed to the Pan-American Geologist, which was published in Des Moines by C. R. Keyes. Keyes had been passed over in 1911, when Calvin died, for the post of Iowa State Geologist and was antagonistic toward the geologic work prepared under State Geologist G. F. Kay. Keyes may have welcomed Sardeson's papers partly for that reason. Kay, on the other hand, espoused the Iowan drift sheet, largely from provincial pride, as Sardeson believed, but Kay was also devoted to Chamberlin.

DATING ICE-CAP MELT BY RECESSION OF FALLS

An outstanding piece of geologic field work, analysis, and exposition was Sardeson's study (1908a) of the retreat of waterfalls on the Mississippi River and Minnehaha Creek that began with the draining of Lake Agassiz through River Warren. He followed the lead of N. H. Winchell (1888b), who was the first to date the passing of the midwestern ice cap by the stages of recession of a waterfall that resulted from that passing. A similar study had been made of Niagara Falls, but with less certainty. Sardeson did not improve on Winchell's concept, but he had a greater store of completed regional geology with which to work; to that he added careful and extensive studies of the remnants of falls and rapids that still clung to the walls of the Mississippi gorge and tributaries in the Twin Cities. He also took into account the different hardness of the members of the Platteville Formation forming the lip of the falls, together with the dip of the beds and its resulting effect on the elevation and rate of recession of the falls. Sardeson's estimate of the age of St. Anthony Falls was much greater than Winchell's, because he was able to demonstrate that it had begun in the city of St. Paul (where it is called River Warren Falls by Wright, 1990, Fig. 4), rather than at Fort Snelling, as Winchell had believed. This exemplary paper was somewhat condensed for the section on falls recession in the Twin Cities Folio (1916).

HISTORIES OF CERTAIN RIVER SYSTEMS

Some years after his field mapping in Minnesota with Leverett, Sardeson wrote several closely argued studies of how Pleistocene stages had changed stream or lake systems into what is found today. These well-crafted vignettes of geology are reminiscent of the "Studies for Students" that used to appear in The Journal of Geology. His illustrations are of the simplest kind, so that the texts are best read together with the state

maps for reference. They describe Lake Mille Lacs (1923f) and the Mississippi (1933a), Cannon (1933b), and St. Croix (1936c) Rivers. Combining inspired imagination and some widely spaced geological facts, Sardeson (1939b) sketched a reasonable picture of west-flowing streams in the Late Cretaceous (his Old Blue River) and of east-flowing pre-Pleistocene streams across the Upper Mississippi River Valley (cf. Wright, 1990, Fig. 2). Some of these studies may have derived from thesis topics that he had identified for possible use by graduate students while he was an academic.[19]

Sardeson's studies of deflation of fine materials from outwash and river channels after the melting of ice lobes are described here, because of their relationship to rivers. Terraces along the Mississippi River in central Minnesota were grass-covered rather than wooded in their pristine condition. These terraces were the early transportation routes of the white pioneers, and the earliest farms in the region were established on them. The farms were worn out within a few decades. The well-sorted and hence permeable sand of the terraces did not hold water—nor much organic matter—and lacked the soil moisture and nutrient fines of the nearby till plains. Even wooded outwash surfaces in the region provided better soil. Sardeson explained (1923g) that as the postglacial Mississippi River waned each fall and winter, the terraces dried out and their fine constituents were deflated. The fine sand and silt from the terraces formed the dune fields that lie east of the river between Brainerd and Minneapolis. The largest of these dune fields was the Anoka sandplain, northwest of the Twin Cities, which Sardeson studied for Folio 201 (1919) and for the state map (Leverett and Sardeson, 1916).

The Anoka Sandplain Controversy

The broad rolling landscape located between the Mississippi and St. Croix Rivers in Minnesota, the Anoka sandplain, has a generally sandy surface, local areas of sand dunes, and patches of thin loess. It is centered on Anoka County and extends southeastward into the northern edge of the Twin Cities. The sandplain lies on a substrate of ground moraine and local thin lacustrine deposits that were developed during the northeastward advance of the Grantsburg sublobe, an offshoot of the main Des Moines lobe of the late Wisconsin ice. The Grantsburg sublobe dammed both the early Mississippi and St. Croix Rivers to form Glacial Lake Grantsburg. Both the lake and the ice lobe were short-lived (Wright, 1972) and left behind a rolling till surface dotted with lakes and ponds, together with scattered outwash deposits derived from the melting of the ice lobe and the draining of Lake Grantsburg. This region had been described by Upham (1888) as a mix of glacial and fluvial deposits.

Evidently Sardeson was assigned by Leverett to map the region of the Grantsburg sublobe for their map of the glacial deposits of southern

Minnesota (Leverett and Sardeson, 1919). Sardeson concluded that Upham was wrong and that the region was better characterized by its eolian deposits. Referring to "thin" settlement and heavy "brush" cover, Sardeson (1932d) noted, "On this account exposures are scarce and so shallow as to throw little light on the thickness of the sand." He knew that the loess was thin and that the dunes were confined to certain areas, but he believed that the extent of the two types of deposit, rather than their thickness, justified assigning a mostly eolian origin to the surficial deposits (Sardeson, 1932d). In so doing he elaborated on a hypothesis he had developed with C. W. Hall more than 30 years earlier (Hall and Sardeson, 1899). In addition to the older conclusions, he suggested that southwest winds had deflated the outwash along the Mississippi River during the dryer seasons of each year. He had applied this analysis more broadly in a paper that described eolian action much farther upstream along the Mississippi River (1923g).

In the late 1920s, W. S. Cooper, Professor of Botany at the University of Minnesota, became interested in the vegetation of the Anoka sandplain.[20] Cooper was a plant ecologist who later became internationally distinguished and a recognized authority on coastal sand dunes. Cooper considered geology and soil critical to the understanding of plant communities and succession, and he carried out laboratory analyses of such materials for his study. He also used aerial photos to assist in understanding the terrain and the interrelation of the several types of landforms on the sandplain. Cooper, beginning to believe that eolian features were less prevalent than shown on the state map, asked Leverett for advice on the geology of the Anoka area, and Leverett recommended to Cooper that he apply to Sardeson, who lived near the University.[21] In those years Leverett's Professional Paper 161 (Leverett, 1932) was dammed up in the editorial stream at the USGS, and he was reluctant to copy or quote passages to Cooper, who continued his ecological studies in the Anoka County region.

Cooper also worked on similar deposits along the Mississippi River above the Anoka sandplain, and reported to Leverett his conclusion that Sardeson's eolian-origin explanation was wrong.[22] He said that only about six percent of the area was eolian sand, and further that Upham had been right in the first place, 40 years earlier. Leverett replied that he supported Sardeson's view generally and that, taking the thin dust layers into account, the region must be about 60 percent eolian—Leverett was acquainted with the area himself, and described dust storms he had experienced.[23] He declined to change Sardeson's text (1932d) in the manuscript for Professional Paper 161. Cooper had already said that he would withhold publication until Leverett's monograph was published. It was published in 1932, but Cooper's work did not appear until 1935. Cooper had, of course, more data in the way of road cuts, cleared ground, and water wells than had Sardeson—but with fewer data Upham (1888) had come to the same conclusion Cooper did in the 1930s.

When Leverett's monograph was published, Cooper explained to Leverett his earlier difficulties with Sardeson. Cooper had studied Sardeson's publications early in his project and attempted to discuss a "relatively minor point of this [dis]agreement with Sardeson," he reported. Sardeson "soon informed me that he would accept my work as a botanist, but would have nothing to do with my geological work."[24] Shortly after Cooper's 1935 report appeared, Sardeson wrote to thank him, to belittle the work of Upham generally, and to repeat that he himself had "invited" Frank Leverett to come to Minnesota to lead the glacial work. In closing, he said, "Our personal contact leads me to expect grand opera from you sooner than good geology."[10] Cooper, who was an accomplished pianist and patron of the Minneapolis Symphony Orchestra, wrote in the margin of that letter, "I've written a libretto for a one-act 'grand' opera, which has had six performances. WSC."[10] Cooper got a good laugh from that one for many years afterward. Sardeson also wrote to the University President concerning Cooper's report, to ask what brand of whiskey his botany professors were using; President Coffman replied that his staff did not share such information.[25]

Cooper had sent a copy of his bulletin to Leverett, who wrote to thank him and compliment him upon several aspects of it. He also suggested that the text showed him to be not "friendly" with Sardeson and Sardeson's views.[26] But Leverett did not know the depth of Sardeson's feeling toward Cooper. Cooper replied to Leverett, respectfully and in kind form, but stood his ground on his conclusions.[27]

Although these men disagreed as to the significance of eolian action on the Anoka sandplain, they all agreed that the dunes there had been derived from outwash deposits dried and deflated by southwest winds. Subsequent workers (Farnham, 1956; Stone, 1966; Keen, 1985) have all agreed with Upham and Cooper that the area has the form and substance, mainly, of glacial and fluvial deposits with only very local dune fields. Keen, however, has shown that the dunes are parabolic dunes formed by northwest winds, and that they were later modified superficially by southwest winds that deflated the central parts of some of the dune fields. All the workers prior to Keen were also mistaken about the age of the deposits. Keen and Shane (1990) have shown them to be mid-Holocene in age, rather than late Wisconsin and early Holocene. They, of course, had Carbon-14 dating at their disposal, but if the older workers had investigated the internal structure of the dunes, as Keen (1985) did, they might have discovered the true direction of the generating wind!

MISCELLANEOUS

Several of Sardeson's publications do not fit into the themes above. Important contributions to the early development of glacial studies were Sardeson's demonstrations that an advancing ice sheet may dislodge and

transport large blocks of bedrock (1905) and incorporate or fold masses of older till (1906). McGee (1891) illustrated instances of contorted drift in St. Paul, but the locations are unknown. Till folded by the shear of younger ice lies atop the bedrock bluffs across the river from downtown St. Paul; the exposure may have occurred by collapse into the abandoned brick-clay quarry after McGee and Sardeson made their observations, for neither mentioned it.

Sardeson's explanation of the various ways in which depressions may be developed in outwash surfaces (1937e) was not entirely convincing. He referred to the depressions as "pits," but several of the types he suggested would by their origin not have the steep sides that the term "pit" conveys. He recognized kettles, of course, and suggested also depressions between the heads of outwash fans, those formed by the collapse of Pleistocene material into sinkholes, and blowouts. The last, however, are an unlikely source of pits, even if annual ice-shove alters their margins, as he suggested (but cited no examples). Describing the Clearwater Lake basin in Wright County, Minnesota, Sardeson postulated a large proglacial lake into which deltas of outwash fell, but without filling the whole lake basin, and thus left a pit in an outwash plain. This really seems to be only a special kind of kettle.

Several publications are less significant than their titles suggest. The discovery of Paleozoic fossils in Pleistocene drift (1892a), a simple curiosity at the time, was doubtless repeated at many times and places since. Although Minnesota has many conspicuous eskers, his report on them (1923e) adds little to the glacial history of Minnesota. Sardeson's discussion of the geology of the name Minnesota (1923c) accurately describes the junction of the Minnesota and Mississippi Rivers, but has almost nothing to do with that title. Sardeson's two articles on "Minnesota Man" and glacial geology (1935b, 1938) are glacial only in that the skeleton was found in a Pleistocene deposit; otherwise the reports merely poke fun at the persons who believed the skeleton to be of Pleistocene age (it was not), and Sardeson's comments border on the polemical. About this same time, Sardeson wrote several times to ridicule G.F. Kay's belief in the Pleistocene age of "Minnesota Man" (="Pelican Rapids Girl").

Taking his many reports together, Sardeson's contribution to the study of glacial deposits was considerable, and his work on the stratigraphy and history of such deposits in the upper Midwest was both extensive and important. His publications also contain a record of much of the history of the development of our present-day knowledge of the Pleistocene events in the Midwest. Regrettably, many workers are unfamiliar with both his work and the history of the early work on those Pleistocene deposits. By education and preference, Sardeson was a paleontologist and stratigrapher, but his work on glacial deposits and history bulks nearly as large as that on the other two subjects.

NOTES

[1]For example: Frederick W. Sardeson (FWS) to Herbert E. Wright, Jr. (HEW), Professor of Geology, University of Minnesota, 26 January 1953, [HEW].

[2]The numerous letters from Christopher W. Hall, Charles W. Hayes, Thomas C. Chamberlin and Charles D. Walcott describing this episode (15 July 1904 to 2 February 1907) are in File 470-128, NA, USGS General Correspondence.

Chamberlin's request, 26 July 1905, on behalf of his son is in his Letterbook XIX, p. 347, 348, [UC, TCC].

[3]Meeting and exchange of letters between Walcott and Sadeson, December, 1906 and January, 1907, [NA, File 470-128]. See also Yochelson (1998, p. 460–461).

[4]William C. Alden, letters and draft contracts to Samuel Weidman (SW) and Sardeson, 23 May 1913; Frank Leverett (FL) to David White, (DW), 29 June 1913; FWS to DW, 30 June 1913 and reply of 5 July, [NA, Files 248 and 298].

[5]Field Records Section, USGS Library, Federal Center, Denver.

[6]FL to DW, 2 June 1917, [NA, RG 57, File 528].

[7]Biographical data [1917] on Sardeson in: State of North Dakota, Complainant v. State of Minnesota, Transcript of Record Number 17 Original, Vol II, p. 851–910. Supreme Court of The United States, October Term 1919, (Washington, D. C., Judd & Detweiler).

[8]Biographical data [1917] on Sardeson in: State of Minnesota, Complainant v. State of Wisconsin, Transcript of Record Number 18 Original, Vol. II, p. 910–938. Supreme Court of The United States, October Term, 1918, (Washington, D. C., Judd & Detweiler).

[9]Biographical data on Sardeson in: Congressional Record, 68th Congress, First Session, hearings [1924] before Senate Committee on Public Lands and Surveys, S-223-O, v. 3, p. 3016–3035, [Sardeson's testimony during the Teapot Dome investigation].

[10]FWS to William S. Cooper (WSC), 21 December 1935, [WSC]. Also, FWS to Ray S. Bassler (RSB), 4 March 1956, [RSB].

[11]FWS's field notebooks, 257 and 262, for 1913–1915, in Frank Leverett's "unaccessioned work" in Field Records Section, USGS Library, Federal Center, Denver.

[12]FWS to DW, 23 June 1913. He wrote that he could not join E. O. Ulrich at Winona, Minnesota, because Leverett was sending him to northeastern Minnesota July 1, [NA].

[13]Interviews and correspondence with Marion Sardeson Buyken, 1984 and subsequently.

[14]Agreement for Federal-State cooperative work, for fiscal year 1912–1913, signed by George O. Smith, Waldemar Lindgren, and William H. Emmons, [NA, File 265]. The budget for Sardeson's salary and expenses was $1200, provided in equal shares by the USGS and MGS.

[15]FWS to HEW, 5 December 1952, [HEW]. Sardeson's 1917 testimony (Note 7) gives 1896 and 1897.

[16]Samuel Calvin to Warren Upham, 6 [or 8] October 1902, [UIA, Calvin Papers, Box 2].

[17]Unpublished draft, "revised 8 February 1928," of a review by T. C. Chamberlin of F. T. Thwaites (1927), [UC, TCC, Box IX, P].

[18]Letters passing between FWS and George F. Kay, October, 1925 through February, 1934, [UIA, Kay Papers, Boxes K-7 (Folder 13), 15, and 16].

[19]FWS to RSB, 22 February 1936, [RSB].

[20]WSC to FL, 15 March 1928, [WSC].

[21]FL to WSC, 17 March 1928, [WSC].

[22]WSC to FL, 18 December 1931, [WSC].

[23]FL to WSC, 15 January 1932, [WSC].

[24]WSC to FL, 19 February 1932, [WSC].

[25]FWS to Pres. Lotus D. Coffman, 23 December 1935, and reply of same date, [PP].

[26]FL to WSC, 9 January 1936, [WSC].

[27]WSC to FL, 24 January 1936, [WSC].

6

APPLIED GEOLOGY AND PUBLIC GEOLOGY

On being dismissed from the University of Minnesota, Sardeson tried several ways to establish a new income stream, preferably one that would permit him to continue to do paleontologic work. The glacial geologic work with Frank Leverett was still going on at the time, but it was to end when their map of the state was completed. That work and Folio 210 (1919) did help bridge the years between 1914, when Sardeson's University salary ended, and 1917, when he was appointed to a post with the State Securities Commission.

Except his brief hope in 1913 for a possible position as paleontologist with the U. S. Geological Survey (USGS), there is no evidence that Sardeson considered moving from Minnesota. He may have concluded that having been dismissed from one institution he would be blackballed by any others. Perhaps Sardeson hoped to continue work as a geologic consultant, doing the short-term "missions" that he and others had done while University employees, but this route had probably been closed to him by the establishment of the Minnesota Geological Survey (MGS) in 1911, when W. H. Emmons arrived at the University.

Sardeson's first attempt to continue part-time work for the USGS, by studying the Cretaceous beds of Minnesota, had failed, but he tried again in 1920. Although he remained on the roll of the USGS until 1924, his last salary from the agency was in December 1915 for preparation of USGS Folio 210, which was published in 1919.[1] Sardeson contacted Leverett in 1920, partly to inquire about possible geologic work, something that Leverett was unable to provide.[2] We saw in the last chapter how Sardeson dunned G.F. Kay for support from the Iowa Geological Survey, but without success. While on the roll of the USGS (Geologist No. 67), Sardeson believed that the possibility of conflict of interest prevented his doing commercial work. He also did none during the war years, he told Leverett, to avoid "conflict" with the University, where the geologists were "commercially organized."[2] But he held himself aloof from commercial work during 1920–1924 as well; perhaps he harbored a hope that a change of administration at the USGS might restore him to an active role. There was no possibility of conflict of interest, though, between work for the USGS and work for the state of Minnesota. But while he was employed by the Minnesota Securities Commission, commercial work was permitted only in the later years. He was stuck, without a regular salary and, evidently hoping that the USGS would put him to work again, he stayed on the part-time job for the state.[3]

In the post-war years, then, Sardeson failed to gain a salaried position, let alone one that supported paleontologic work. The intermittent work for the state served rather well through the 1920s, but it yielded little during the Great Depression, and the "Blue Sky" function of the State Securities Commission ended in 1934. For his last nearly 30 years (from about age 60 on), therefore, he was dependent upon his daughter for a living and for field expenses.

Sardeson's work in paleontology, stratigraphy, and glacial geology continued for nearly 30 years after he lost his position at the University. Most of that work was published, perforce, in the *Pan-American Geologist*. With the exception of the one small grant to study bentonites, the work was done from his growing fossil collection, his accumulated field notes, and new field work made possible by his family. He had his own library, and he kept up with current literature as well at the University Library.

Other than his published papers, the documentary record of Sardeson's work outside academe is scanty, but there is some evidence that he tried to establish a sideline shortly before being dismissed. His plan was to go into real estate, which failed for reasons beyond his control, but it did lead to forensic geologic work and, ultimately, to his post with the Minnesota Securities Commission.

REAL ESTATE AND WETLAND DRAINAGE

Many years after the events, Sardeson told Bassler that he had joined Leverett in the mapping of glacial deposits with the intention of becoming a realtor.[4] In this and most other references to that work he called the mapping job a "soil survey," probably because he and Leverett often identified glacial materials by their soils and because the whole project had the enhancement of agriculture as a principal purpose. A major impetus for such statewide mapping was apparently lobbying by the All Minnesota Development Association, what Sardeson called the "Realtors Association."[5, 6]

Before the glacial mapping was done Sardeson had been dismissed from the University, and he did go into real estate with a partner. His partner soon died (1915 or 1916, as nearly as can be determined), and Sardeson, with no business experience, was adrift again.[4] Sardeson's daughter Marion had no recollection of his ventures into real estate and wetland drainage; although she was too young at the time to have been concerned, she learned no family "lore" about such businesses.

His brief foray into real-estate work may have brought Sardeson to the favorable attention of other Realtors, for on 1 September 1916 he was hired full-time by the Reclamation Drainage and Flood Committee of the All Minnesota Development Association, an association of developers.[7, 8] The poorly integrated drainage of the region of glacial drift

had led to extensive ditching and tiling of arable land, beginning in the late 1800s, and this was an enduring concern of farmers and politicians. Minnesota looked on the reclamation work with favor in order to enhance the sale of state wetlands for additional agricultural acreage. Drainage ditches existing in the early 1900s, perhaps not well engineered, were becoming increasingly subject to floods, and revision of the state drainage laws and reclassification of state lands were sought by the Association. Development of the state-owned wetlands of northern Minnesota for possible sale to farmers was a major goal. Sardeson made "dinner time speeches" to various audiences, answered questions, participated in discussions, and carried out some investigations for the committee.[6] He believed his role was effective because he was "the only person in the State who had seen all parts of it." Apparently he was introduced that way to his listeners. The broad scope of Sardeson's knowledge was touted in a flyer put out by the Drainage and Flood Committee of the Association.[9] As an expert witness, he lobbied on behalf of the committee before the 1917 Legislature, which passed a revised drainage bill.[6] In his letters Sardeson referred to that work as having "gone into politics," although he never was a politician of any sort.

As a co-author with Leverett of the "soils" maps, Sardeson was surely regarded by his listeners as an agent of the USGS,[10] whether he took pains to disabuse them of the notion or not. Someone (Emmons?) sent news clippings of the reports on Sardeson's educational and lobbying efforts to G. O. Smith, Director of the USGS, probably in the hope that Smith would take action against Sardeson. Smith did caution Sardeson against appearing to represent the USGS, on whose roll he was still listed,[7] and Sardeson responded (on 12 October) to Smith's satisfaction.[11]

Sardeson's only publication dealing with drainage (1902c) is his study of a region of north-central Iowa where glacial drift covered karsted limestone beds. Though he did not say so, the combination of surface and subsurface drainage may have been the reason for him to venture into that area where the bedrock is of Carboniferous age.

TESTIMONY IN LITIGATION

Sardeson's public action regarding drainage reform and reclamation must have brought him to the attention of the attorney general, for he was soon involved as an expert witness in suits between Minnesota and its neighbors. Minnesota had straightened a part of the course of the Mustinka River by ditching; the river drains into Lake Traverse, which divides Minnesota from both North and South Dakota. The state of North Dakota alleged that the ditches drained the river's basin too rapidly, causing Lake Traverse to rise too fast and too far, and that its outlet, the Bois de Sioux River, had therefore overflowed and damaged valuable cropland in North Dakota in both 1915 and 1916. North Dakota sued for $5,000 for itself

and $1,000,000 for its injured citizens, who had paid for the suit in addition to having suffered the flood damage.[12] Provision was also made for South Dakota to join the action against Minnesota, though it did not do so.[12]

Minnesota admitted the ditching, but claimed that the damage resulted from the succession of three unusually wet years, 1914–1916. Employed as expert witness by the attorney general of Minnesota, Sardeson testified before a special master for the U. S. Supreme Court in October of 1917.[8] He described the geology of the Mustinka watershed and the Pleistocene history of the region, which includes the area he studied for Folio 210 (1919). Part of Sardeson's testimony was surely helpful to Minnesota, for he pointed out that although the land surface in the region sloped northwestward toward Lake Traverse, the ditches all lay east-west or north-south, on the section lines. Instead of accelerating runoff and causing flooding, as North Dakota contended, the ditches delayed the delivery of storm water to Lake Traverse.

Sardeson's testimony turned out to be valuable. Minnesota won the suit and North Dakota had to pay the costs. The main reasons for the decision were that North Dakota had not shown that the floods were much different from those of previous wet cycles; it had built roads and a railroad across the Bois de Sioux River with culverts that were too small and that exacerbated the flooding; and the case for Minnesota was better supported by engineering and geological testimony.[12]

At about the same time, Minnesota sued Wisconsin over the trace of their common boundary along the St. Louis River, where it flows into Lake Superior between Duluth, Minnesota, and Superior, Wisconsin. Port facilities constructed on the Minnesota shore of Upper St. Louis Bay obtruded across what had for decades been mutually understood as the boundary. Wisconsin surely noticed this trespass and may have thought to tax the facilities. As Sardeson put it seven years later in his testimony on Teapot Dome,[13] Wisconsin wanted Minnesota's docks "down to the roots." Minnesota, of course, hoped to push the boundary southward, into the middle of that bay, so that the docks would lie wholly in Minnesota.[14]

Sardeson was employed as an expert witness because he had studied the region during preparation of the map of glacial deposits of northeastern Minnesota (Leverett and Sardeson, 1917a). He testified for Minnesota in September 1917, before a special master of the U. S. Supreme Court.[15] Sardeson's testimony addressed the nature of rivers and their channels and banks, as well as conditions in the area during higher (Pleistocene) levels of the lake. In short, he described where the thalweg—the trace of the deepest part—of the St. Louis River lay, and that line was the one adopted by the court. The thalweg and the historically understood boundary were similar and lay closer to the Minnesota shore than the center of the bay, which is what Wisconsin had noticed at the outset. Minnesota's

claim was almost doomed from the start, for it merely wanted to move the boundary beyond the ends of its docks—to a place where it had not been historically and where it did not quite coincide with the post-glacial course of the St. Louis River. Much later Sardeson told a college classmate that he had "saved Duluth Harbor for Minnesota,"[16] but the "roots of the docks" had not been in dispute. Minnesota had a poor case to start with. Sardeson's geologically appropriate testimony merely confirmed part of Wisconsin's opinion.

The Supreme Court directed that a monumented survey be made so that the boundary might accord as closely as possible to that specified in the legislation that established the State of Wisconsin.[17] The survey was done by a joint commission of the two states, and a report and maps were filed with the court. This first cadastral establishment of the boundary was adopted by the Supreme Court, which had directed that the two states share the cost of the preparation and publication of the survey. That boundary still stands. Although it differs somewhat from the unsurveyed pre-trial trace, it substantiated most of what Wisconsin had claimed over most of its length. In consequence, the outboard ends of some of the docks on the Minnesota shore are in Wisconsin today.

In the course of these two proceedings, Sardeson had served the attorney general of Minnesota effectively and at the same time had come to the attention of persons in the legislature and state capitol. That acquaintance with people in positions of power led directly to his next employment, as an examiner for the Securities Commission.[4]

EXAMINER OF OIL AND OTHER PROPERTIES

From 1917 through 1934, Sardeson was an examiner for the Minnesota Securities Commission, which was established in 1917.[18] The Commission sought to assure that only those corporations with accurately valued properties were licensed to sell stock in the state. The new policy was a response to numerous cases of shysters having sold stock in worthless corporate ventures of all sorts, often in oil fields, to gullible or uninformed citizens, particularly in the rural areas of the state. The law provided that the property for which shares were sold must have some prospective value and that the capitalization must not be unreasonably large compared to the value of the assets of the corporation. The law required that the corporation and its property be examined by a competent specialist and that a license to sell stock in the state would be granted only after a favorable report by the examiner and approval by the Commission. People investing in licensed stock were thus to have assurance that the venture was not crooked and the stock was not watered. The relief from blatant fraud or total uncertainty gave buyers of stock "clear sailing," or a "Blue Sky," the term often used to refer to both the law and the Commission. Rejected applicants could provide additional data and gain a license, or they could appeal denial of a license to the Minnesota Supreme Court.[6]

Sardeson became an examiner for the Securities Commission shortly after it was established; he was not the first expert employed, but his predecessor had quickly "became confused by Oil geologists and quit."[6] The attorney general of Minnesota, a member of the Securities Commission, summoned Sardeson to replace the earlier investigator because he remembered the service Sardeson had given in the suits with North Dakota and Wisconsin. "He called me in," Sardeson recalled, "Taken by surprise, I had no valid excuse for not serving, although I never consented to being called an Oil geologist."[6] Sardeson was employed by the Securities Commission beginning 19 December 1917, at $10 per day to start, with expenses reimbursed.[19] Summing up his methods in the new work, he commented, "I was never quarrelsome, revengeful nor open to bribery."[6] Apparently Sardeson steered a calm course with respect to interpersonal relations and served the state well for 17 years. The formerly free-standing Commission was folded into the Commerce Department as its Securities Division in 1925.[20] Sardeson's employment ceased in 1934; not many applications for licenses appeared in the depths of the Great Depression and the federal Securities and Exchange Commission was created then.

It was reasonable to appoint a scientist with academic experience to the Commission. The "experts" needed for validating oil properties were to be found only in the University, for Minnesota was not an oil state and therefore had no cadre of independent consultants or petroleum engineers. The state's mining engineers were involved mostly with the extraction of iron ore, and there was no great need for scrutiny of new securities there, for the iron-mining properties were already well exploited and largely settled. The Commission did employ mining engineers to evaluate metal or coal properties of companies—in whatever state.

Nature of the Work

As an investigator, Sardeson traveled to study the properties held by the applicant corporations and to estimate the value of "improvements" that may have been made to well sites. "My work is to examine the properties of corporations, - oil, cement, clay, road materials, etc.," he recalled.[21] He filed duplicate reports (none of which survive) on the properties and prospects of applicants with the Commission, which made the legal decisions. He also filed a "secret single report" on the "promoters" of the corporations. He believed that he could distinguish whether the "enthusiasm" of the promoters derived "from their knowledge and experience, or from ignorance, or from dishonesty, or from illusion."[22]

Sardeson ranged widely over all of the regions where petroleum exploration was active in his years as examiner for the Securities Commission. Numerous Minnesota corporations sought licenses for oil stocks, and many, perhaps most of them, owned property in other states. No official records exist of all of the locations of oil fields and other

properties that Sardeson examined, but his itinerary can be partially recovered from his own notes and other sources. To establish his credentials before the U. S. Senate Committee investigating the Teapot Dome affair he reported having made examinations in Arkansas, Colorado, Kansas, Kentucky, Montana, New Mexico, North Dakota, Ohio, Oklahoma, South Dakota, Texas, and Wyoming.[13] In letters to Schuchert he mentioned examinations in Louisiana and Alberta.[23, 24] There is no record of any inspection of properties in Minnesota except, by implication, in the confrontation with a Methodist bishop, described farther on. As Minnesota's main examiner for oil properties,[6,19] Sardeson visited the Gulf Coast, Midcontinent, and Rocky Mountain regions repeatedly in the course of his 17 years of service.

Success as an examiner depended on more than scientific expertise. It required the willingness and capacity to constrain other scientists, professional geologists, or wildcatters in the employ of promoters and pushing their schemes. Throughout his tenure in that post, Sardeson believed that "the intent of the 'blue sky law' is to prevent scientists by their 'favorable reports' from aiding in promotion's swindles or in coming to the rescue in courts of those who are swindlers." An unsuspecting public often gave undue credit to biased reports. "Science can be used unwisely within the law, particularly if the offender has no dishonest motive or if no person complains in the courts."[25] What he was up against is clear in the following:

> For example, an oil company finds itself crampped [*sic*] by low prices of oil and wants to sell three million dollars of stock in Minnesota to do business with. The case is turned over to me as examiner's "expert." The company claims $8,000,000 lease values; $1,500,000 of developments; and a going concern. I examine and find no lease value; $85,000 of developments or a salvage value of $30,000. They used to fight me in "re-hearings" but lately my "rep" of being the "best geologist in America" seems to be the worst punishment for me & salve for their geologist.[26]

Apparently applicants sometimes came face-to-face with the Commission and with Sardeson; whether this was routine for applications or only for appeals is not known. Only one documented example of Sardeson's presence at a hearing, the consideration and denial of an application, survives, though he once made mention of "many hearings."[27, 28] Drawing upon legal language then current that separated the worst evils of trust practice from ordinary (if often sharp and predatory) business methods, promoters' geologists wanted Sardeson to be "reasonable." To avoid the problem of defining reasonableness, the Secretary of the Commission "was instructed to tell the rascals confidentially that 'Sardeson

is just a little bit queer and the Commission lets him have his own way.' "[29] Complaints not directed at Sardeson certainly fell on the Commission, but it was a committee and thus much less sensitive to abuse and "leverage."

Views of Sardeson's Work

Sardeson's performance in the state's service was inevitably controversial, as all regulatory efforts necessarily are. "Of course I was not at all popular with the oil geologists, since I was so effective against them," Sardeson told Bassler, "and only as company officials began speaking of me as 'the best' oil geologist in America, the atmosphere cleared so that I felt easy in their company."[30] Indeed, Sardeson doubtless considered himself the best geologist in America, as he implied to Bassler, in the long quotation above.[26]

Few views held by the regulated survive. What associates in the oil patch said of Sardeson or his predecessor may sometimes have been flattery or sarcasm rather than true admiration. For example, a laudatory letter praising Sardeson's geologic practice and knowledge sent him by a company in Kansas may have been flattery in the hope of a favorable examiner's report.[31] R. D. George, State Geologist of Colorado, had quite another opinion of the examiner for Minnesota, regarding whom he complained to Emmons that he should know "the caliber of the man who is doing the Blue Sky work for the state."[32] Emmons thought that the man in question was Sardeson, but it probably was his predecessor, for George wrote on 10 December 1917 and Sardeson was not appointed examiner until 19 December. A Wyoming geologist undoubtedly did mean Sardeson when he asked Emmons:

> Who is Prof. Sargents [*sic*]? . . . I was reading . . . a prospectus of an oil company wherein Professor Sargents was said to have made a favorable report on certain territory. It was further stated that he was the head of the Department of Geology at the University of Minnesota, and chief of the Minnesota Survey. . . . the land in question is of extremely doubtful value.[33]

Tough-skinned and ornery, Sardeson obviously understood and perhaps relished the controversy aroused by his position, and was rather bemused by the job. "I must make my living by oil-field work as expert geologist . . . that is as crazy an occupation as can well be imagined."[34] He characteristically distinguished an "Oil geologist" as someone with limited knowledge of the science and, possibly, an easy virtue that let him recommend uncertain prospects as sure money-makers. Not an "Oil geologist," but "a geologist in oilfields," as he put it, Sardeson also had experience with "doodlebugs"—any of various devices supposed to have an affinity for petroleum that were used by the ignorant and dishonest

to promote dubious or phony prospects.[35] His position was rather like that of Peter at the dike; to hold back a flood of ill-founded investment schemes was certainly worthy, and probably satisfying, but it subjected him to much complaint and verbal abuse. Perhaps some was deserved. He wrote wearily of "this last few years [of] being savagely attacked by disappointed promoters and fakers."[36] He bragged that he spent 17 years "frustrating geologists and oil promoters as they tried systematically to plunder the public, with 'favorable' geologic reports as immunity from jail sentences. We did put some in jail too."[30] Although Sardeson was apparently not involved in it, a case involving gold notes and dummy brick companies arose in 1927 when a promoter bribed the Securities Commissioner for a license; both the promoter and the (by then) former commissioner were convicted of mail fraud and sentenced to 20 years in Leavenworth.[37]

Recall that Sardeson believed he could tell whether the basis and enthusiasm for a promotion derived from the promoter's experience and knowledge, or his ignorance, or his dishonesty, or his illusions. We might hope to be capable of like judgments, but hardly infallibly; if Sardeson had been that good at such analysis he would have been marvelously valuable to the people of Minnesota. At the practical level he was, for none of his recommendations from 1917 into 1924 had been overturned, as he testified before the U. S. Senate Committee investigating the Teapot Dome scandal.[13]

Oil in Minnesota

Three related aspects of Sardeson's regulatory work are discussed in this section: the general subject of exploration for oil and gas in Minnesota; the dangerous aspects of Sardeson's work, and the relations of the University's teaching department to the Blue Sky work. An enduring problem for Sardeson and the Commission was the fervent conviction of many Minnesotans that an oil field could be discovered in Minnesota. The belief persisted well past Sardeson's years as an examiner; in 1950 a lumber dealer in southern Minnesota who believed in doodlebugs demonstrated how well they worked over a puddle of crude oil on the floor! He scorned Emmons (then deceased) and displayed a "structure map"—of topographic contours on a drumlin—that he believed was an oil dome.[38] The occasional escape or discovery of flammable marsh gas may still excite the hopes of an oil field. Several prospects were drilled each year in the 1920s and test drilling was taken up again after WW II, even up to the 1980s, the date of a report on the history of petroleum exploration in Minnesota (Morey, 1984).

No one at the University ever believed that there was an oil field in the state (Morey, 1984). Emmons was known in the Department of Geology and throughout the state for having offered "to drink all the oil

that is found in Minnesota." Sardeson himself had years earlier stated under oath that he had no reason to believe there was any geologic possibility of commercial oil or gas in Minnesota.[39] When oil drillers found oil in Minnesota, "the oil was put into the well before it [was] pumped out," Sardeson remarked sardonically. "It is very inexpedient to smuggle . . . [crude oil] in commercial quantity, at least ten barrels a day [,] into a well here."[40]

Some speculators were hard to discourage, even by the experts. In the 1920s, some doctors at the Mayo Clinic wanted to try an oil well in the Rochester area. Emmons told them not to bother, but they had the money and wanted the "fling." So, cleverly, Emmons offered them a student or two to "sit" the well, which was drilled 1923–1925 to a total depth of 3225 feet, and report on the geology.[41] That dry hole helped the students stay in the University! On the watch as ever, Sardeson was not sure that Emmons had not helped promote the well!

Some measure of the exploration activity by Minnesota companies can be inferred from the number of applications—in the hundreds—during Sardeson's tenure as an examiner. About half of the applications from oil companies during the Commission's early years (1917–1924) were from companies incorporated in Minnesota, though, to be fair, many—perhaps most—intended to drill not in Minnesota but on their properties in other states. The majority of all applications in that interval were denied; some were licensed promptly, but others were licensed only after revised or additional evidence was submitted.[18]

Sardeson claimed years later that Emmons had submitted a favorable report on a well to be drilled at Dassel, Minnesota,[39] but whether that came before the Securities Commission is not known. In Sardeson's view, "The nearest we ever came to having a legal oilfield and natural gas in commercial quantity here was as a Methodist bishop said to the Securities Commission that it is none of the State's business what (stock) is sold by him to his own people. The Commission indicated politely that there would be an open season on Methodist Bishops unless he changed his mind."[40] Daughter Marion recalled hearing that bishop (although she had the wrong church), in her family's living room, threaten her father's life if he didn't approve the bishop's plan to sell stock in an "oil field" to his parishioners.[42,43]

Sardeson's daughter recalled two other threatening incidents as well. On a visit to the Kevin field in Montana, Sardeson was met at the train by the camp cook and wagon; en route to the field he was shot at. The bullet passed through the shoulder of his overcoat, but missed his body. En route to Mandan, North Dakota, Sardeson worked on a table in a whole Pullman berth, as was his wont. He carried two identical satchels, one for clothing and one for his maps and notes from his investigations. The map bag lay on the opposite seat under a newspaper, and the clothes bag lay under the table. Before getting off at a brief stop, Sardeson prudently

switched the bags; when he got back aboard, the satchel on the seat was gone, but it was the clothes bag! He completed the job without fresh clothing and came home angry; shortly afterward the railroad reported to him that his tattered clothing and bag had been found along the right of way

Sardeson believed that members of the University came to regret their earlier support for securities regulation.[6] His idea was that the Department of Geology and the School of Mines had suffered from the scrutiny of the Securities Commission. He was annoyed at some members of the Geology Department because he believed that they interfered with his "Blue Sky" work by being promoters themselves.[44] He boasted that through his regulatory work he had helped "kill them off."[45] Members of the Geology faculty did take part in forming the Greenwood [Oil] Company in 1917. No doubt Sardeson would have delighted in doing them damage, but there is no documentary evidence that he did. Whatever he thought he may have done to them, both the Department and the School survived Sardeson.

His irritation at the geologists aside, in his work as an examiner over those 17 years Sardeson participated in the development of institutions and practices that were absolutely essential for the performance of the regulatory functions expected of modern governments, both state and national. No fair assessment of the value of securities issues or the fairness of particular prices or profit levels could be made without a determination of the value of the physical assets of companies.

Although Sardeson was generally conservative in his political views, he believed the valuation work carried out by the state provided essential protection for citizens. No doubt promoters resented as "red tape" and unreasonable interference with corporate capitalism the inspection required before issuance of a license to sell stock. But when Minnesota's Blue Sky law was enacted, each of the states on its borders already had such laws, which had left Minnesota a happy hunting ground for promoters. Yet the examinations by Sardeson and mining engineers were surely a valuable public service on the whole, particularly in those years of the teens and 1920s, before the national Securities and Exchange Commission was established in 1934.

Teapot Dome

In the early years of the petroleum industry in the United States the "rule of capture" was operative. It permitted wells to be drilled near producing wells or fields, and to pump the same reservoir, to the disadvantage of the operator of the original well(s). It was rather like permitting someone else to stick a straw through the side of the glass into your milk shake and pump it out faster than you, or even before you had a straw of your own. Naval Petroleum Reserve No. 3 had been established

on the Teapot Dome structure in southeastern Wyoming in 1915. The dome is on the southern part of the Salt Creek anticline, which has a larger and more productive dome to the north—the Salt Creek Dome and oil field.

The Teapot Dome affair, a famous scandal of corruption in the federal government in the early 1920s, was made possible because of the rule of capture. Some concerned parties thought it prudent to draw off the oil in Teapot Dome with defensive wells, in order that the reserve not be drained off under the saddle between the two domes in the Salt Creek anticline by vigorous development of the Salt Creek field. The scandal was political rather than geological; it developed because the Navy released responsibility for the reserve to the Secretary of Interior, Albert Fall, who was bribed by Harry Sinclair, owner of Mammoth Oil, to grant leases to establish defensive wells in the Naval Reserve. The conspirators alleged that the threat of drainage of the Navy's oil northward under the structural saddle into wells of the Salt Creek field required preemptive drilling and pumping of Teapot Dome for the benefit of the country. Fall and Sinclair further justified the sale of oil from the Naval Reserve by pointing out that the government was to receive a royalty on the oil produced. But the fraud in their scam was that the royalty would return only a fraction of the value to the government; Sinclair was to profit abundantly, enough to pay the bribe to Fall and have a large profit for himself. Actually, such "defensive drilling" had been urged for some years before Sinclair commenced it in 1922, but as a wartime measure to save the oil, and not as a fraud upon the government.

Because of his experience with evaluation of oil field properties, including Teapot Dome itself, Sardeson was subpoenaed to testify before the U. S. Senate Committee investigating the Teapot Dome scandal. The scientific issue was whether drainage from the Teapot Dome field to wells in the Salt Creek field had occurred or could occur. For his appearance before the Senate Committee on Public Lands and Surveys, Sardeson was escorted to Washington by two U. S. Marshals; his daughter believed there were threats of bodily harm to Sardeson.[12] He testified before that committee on the morning of 28 March 1924, and he was questioned by Senators Walsh of Montana and Spencer of Missouri.[13]

In his testimony, Sardeson made much of his experiences in oil fields in a dozen states and of the fact that none of his recommendations to the Minnesota Securities Commission had been overturned to date. He also represented himself as one of only three geologists in the country who was not employed by "the interests,"[13] mostly oil companies, that is. His testimony was useful to the senators trying to demonstrate fraud, for he asserted strongly that drainage from Teapot Dome, beneath the saddle, and to wells on the south flank of the Salt Creek Dome, was not geologically possible except to a trivial degree. In other words, the drilling in the Naval Reserve was wholly unnecessary, and the decision to allow private interests

to draw off the government oil preemptively, at great profit to them, was merely to make the fraud possible.

Some of Sardeson's other testimony is interesting today for what it shows about the state of understanding of petroleum and gas reservoirs 75 years ago. Sardeson asserted that the blow-off of gas from a newwell diminished the *rate* of oil production to be expected from that well, but that such action did not decrease the total *volume* of oil that may be produced from that well. Our understanding of reservoir pressure and its critical role in the production of liquid petroleum urges that it be conserved and not wasted; it provides the energy that makes the oil flow, as we have known now for many years. Sardeson was adamant that a well not closer than about 400 feet to another could not drain oil from the first—gas pressure, yes, but not oil. He therefore recommended that the Naval Petroleum Reserves be protected by a requirement that no wells might be drilled closer than 500 feet from the reserve boundary. No geologist would be prepared to make such a recommendation today, for drainage or lack of it at a specific distance would depend upon the porosity and permeability of the reservoirs to either side of the boundary, as well as upon the relative reservoir pressures. Sardeson's opinions cannot fairly be faulted by comparing them to what can be predicted on the basis of technology existing 75 years after his testimony, of course. His view was good for its time and so considered by others; for example, K. C. Heald, later a distinguished petroleum geologist, had studied Teapot Dome in 1921 and reported that drainage was "no immediate danger" (Werner and Starr, 1959).

The question of the closeness of Salt Creek wells to the boundary of Naval Reserve No. 3 was of great moment to the Senate Committee, for the boundaries of the reserve had been set according to the structure mapped by C. H. Wegemann for the USGS (Wegemann, 1911). By Wegemann's map, the saddle between the two domes—the "threshold" beneath which "drainage" would have occurred—was at the north edge of the Naval Reserve. The frenzy of drilling on the Salt Creek anticline after World War I provided many more drill logs with which to determine the structure of the two domes. From those new data, geologists Clapp and Lewis[46] prepared a new structure map of the anticline for the Senate Committee and concluded that the saddle really lay nearly a mile farther south than Wegemann had mapped it.[13] This newer view of the matter is embodied in a map published much later (Thom and Spieker, 1931) and still regarded as definitive. This new finding meant that the threshold of drainage lay well within the Naval Reserve and enhanced the concerns about drainage. Senator Walsh pointed out the discrepancy between the two maps before the Committee, but Sardeson elected to depend upon the older Wegemann map. The Senators knew that Wegemann himself had endorsed the newer work, but Sardeson passed that off as the result

of Wegemann's employment. After leaving the USGS, he had gone to work for the Standard Oil Company!

Despite Sardeson's belief that the danger of drainage was slight, some did occur. At the time of the scandal, some geologists believed that drainage might total about 15 percent of the volume of oil recoverable from the Teapot Dome (Ravage, 1924), and the two geologists consulting for the Senate Committee were among them (Noggle, 1962). Present-day professional opinion is that some drainage occurred from Teapot Dome to very productive wells on the south flank of the Salt Creek Dome.[47]

Though the Teapot Dome scandal is still well known and raised from time to time as an example of corrupt politics, Teapot Dome field is regarded today as a small incident in the history of the oil industry in Wyoming. "It was and is a small time oil field," judged a savvy Wyoming oil man of today.[47] Whereas the Senate Committee's geologists had estimated a reserve of 26 million barrels, Sardeson contended in testimony that it was about 75 million barrels.[13] Its neighbor, the Salt Creek field, was the most prolific producer in the Rocky Mountain region at the time and "one of America's great fields" (Barton, 1969). Teapot Dome, by contrast, had thinner reservoir sands, more faults, and less permeability. From 1922 through 1927, Teapot produced only 3.55 million barrels of oil, while Salt Creek yielded 139.3 million barrels in the same time interval (Barton, 1969). In 1923 alone, Salt Creek out produced Teapot by more than 34 to 1. Curry (1977) described the Teapot Dome field today as "a lonely pasture." A 1977 rating of the 100 largest oil fields in the United States and Alaska put Teapot Dome as 99th, with 42.5 million barrels of proved reserve (Curry, 1977). Sardeson was clearly overly optimistic before the Senate Committee.

EARNINGS

Sardeson's loss of his University job consigned him to insecure, sporadic employment at a drastically reduced income level for the rest of his life. His real estate and drainage work apparently lasted less than two years, and he reported to Bassler that he was paid $200 per month while preparing "drainage legislation."[3] He worked only sporadically for the Securities Commission at a salary of $10 per day plus expenses—not a shabby sum if it had been full time. Having had no pay from the USGS after 8 December 1915,[8] Sardeson was glad to get the examiner's work in 1917.

He also tried, without success, to get work at $7 per day from the USGS; he inquired again about work on the Minnesota Cretaceous in late 1920.[2] Because the USGS had not assigned him work as of 1921, he appears to have asked the Securities Commission for some relief—more work or perhaps permission to do consulting. Whatever the case, he said in 1925 that the Commission "requested me to take up private oilfield work as consulting geologist so as to be still available for their work. This has

been done to date."[1] But there is no evidence, not even in Sardeson's letters, that he ever did such work or earned a dollar at it. His daughter recalled that he once worked for the Sun Oil Company, however.[42]

Despite the fact that he had the regulatory work, he delayed his resignation from the USGS until 30 June 1924, evidently hoping that he might receive an assignment. Besides proscribing work in conflict of the interest of the USGS, the Survey apparently also prohibited its staff geologists from accepting private consulting work for two years following their resignation, until 30 June 1926 in Sardeson's case. Even holding himself aloof from conflicts with the interests of the USGS, he evidently was sufficiently busy from 1917 to about 1925 with his Blue Sky work that he did not feel that he was failing his family obligations.

Between 1917 and 1934 Sardeson's income as an examiner probably fluctuated widely, often leaving him short of cash. Although his house, bought in 1906 for $3,000, was paid for, he could not afford to install electricity; so he claimed that he preferred gas light. About 1927, daughter Marion, recently out of college and working in a department store while living at home, threatened to take an apartment of her own if he didn't allow electric lights; Sardeson relented and Marion paid for the electrification. On the other hand, apparently in a relatively fat year, he bought Marion a new blue two-door Chevrolet [about $625 then] for her 25th birthday, in 1929.[42] Sardeson's mother had died early that year, so it may be that he had received some modest inheritance.

Sardeson evidently had a slim year in 1927, for he complained that he had had more than 300 days of no work.[48] In November, 1927 he offered to consult in the field in northeastern Iowa for $500 for the first five days or fewer and $50 per day thereafter; it seems that he did not get much work at those rates. [49] He reported that he had mostly done nothing in the summer of 1928,[50] but in early 1929 he was "financially better off outside the University."[51] Sardeson repeatedly told people that he could earn $100 a day preparing "favorable" reports on oil prospects, but that he would not indulge in such crookedness. Yet later that year, apparently bragging, he reported to his friend Charles Schuchert that he got $100 a day for oil work.[52] "It is true that my 'honesty' as they call it keeps me relatively poor," he told Schuchert. But he also reported that he had recently been on the Gulf Coast and in Montana.[23] In February 1930, he was engaged in "oilfield duties,"[53] and that March he claimed that it required less than half his time to earn a living and "attend to house holding here."[54]

When the East Texas field blew in during late 1930, the price of oil crashed countrywide and surely dampened the enthusiasm for promoting oil stocks in Minnesota. In March of 1931 Sardeson was out of humor because of the lack of work,[55] and again in the fall,[56] but he spent that Thanksgiving in an "oilfield shack" near Kevin, Montana, and hoped for more work at Christmas.[57] He had no work in Oklahoma that winter, so

he missed the petroleum geologists meetings in Tulsa.[58] Apparently he had no work at all in 1933–34,[59] which is hardly surprising for those depression years. A hint of the end of his work as a securities examiner, which did indeed come in 1934, was his 1934 report to Bassler that "I may get entirely out of business this winter."[60] Furthermore, the coming of the federal Securities and Exchange Commission in 1934 made the Minnesota Commission unnecessary, as was surely widely known in the government of Minnesota.

NOTES

[1]Frederick W. Sardeson (FWS) to George Otis Smith (GOS), 9 October 1925, with copy to William H. Emmons (WHE), [MGS Correspondence, Box 5].

[2]FWS to Frank Leverett, 31 December 1920, [JHZ].

[3]FWS to Ray S. Bassler (RSB), 17 [the day cited in his letter of 6 June] May 1950. "I had a reputation for [honesty] . . . . But I lacked experience and meanwhile hung onto the Survey," [RSB].

[4]FWS to RSB, 18 September 1951. "A demand by the State's Attorney General, however, diverted me towards commercial geology and politics. The World War and the accidental death of my business partner left me no alternative than to serve the State in law enforcement and the politicians in combatting Socialism. That is where I accomplished something worthwhile. Meanwhile I kept up the hope for a long time of again becoming paleontologist only." [RSB].

[5]FWS to RSB, 4 March 1956, [RSB].

[6]FWS to W. Charles Bell (WCB), at University of Texas-Austin, 7 October 1956, [WCB].

[7]GOS to FWS, 10 October 1916, [MGS].

[8]State of North Dakota, Complainant v. State of Minnesota, Transcript of Record [of testimony in October 1917] Number 17 Original, Vol. II, p. 851–910. Supreme Court of The United States, October Term, 1919, (Washington, D. C., Judd & Detweiler).

[9]Flyer "The Waste Lands of Minnesota," issued by the Reclamation Drainage and Flood Committee of the All Minnesota Development Association; sent by FWS to GOS, and filed with his reply to FWS of 13 December 1916, [NA, File 115].

[10]Sardeson was styled as "of the United States geological survey" by *The Minneapolis Journal*, 18 September 1916.

[11]GOS to FWS, 8 November 1916. Sardeson's letter of 12 October does not survive, [MGS].

[12]State of North Dakota v. State of Minnesota, 263 U.S. 365 (1923), a report of the case. An order of the Supreme Court allowing participation by South Dakota is contained in 256 U. S. 220 (1921).

[13]Congressional Record, 68th Congress, First Session, hearings [in March of 1924] before Senate Committee On Public Lands and Surveys, S-223-0, v. 3, p. 3016–3035.

[14]State of Minnesota v. State of Wisconsin, 252 U.S. 313 (1920) and S258 U. S. 149 (1922).

[15]State of Minnesota, Complainant v. State of Wisconsin, Transcript of Record [of testimony in September of 1917] Number 18 Original, Vol. II, p. 910–938. Supreme Court of The United States, October Term, 1918, (Washington, D. C., Judd & Detweiler).

[16]FWS to Victor Stearns, of Duluth, 18 August 1941, with a copy to F. B. Snyder (FBS), [FBS].

[17]Decree appointing a boundary commission to run a monumented boundary along the St. Louis River from the Falls to Lake Superior, 254 U. S. 14 (1920).

[18]Securities Commission Annual Reports, 1917–1924; case reports are not preserved. [Minnesota State Archives, Record Group: Securities Commission. Minnesota Historical Society (MSH)].

[19]Securities Division Minutes, 1917–1925, v. 1; none survive for the years 1926–1934, Minnesota State Archives, [MHS].

Midway through his 17 years with the Securities Commission, Sardeson withdrew from it, apparently deliberately. His successor promptly proved inadequate to the task, and Sardeson was asked to take up the work again; he carried on until the Commission was dissolved, in 1934. This episode is described in letters to G.F. Kay, 6 January 1925 [26] and 7 November 1926, [UIA, Kay Papers, Box K-7, Folder 13].

[20]Commerce Department, Agency Subgroup Administrative History, Minnesota State Archives, [MSH].

[21]FWS to RSB, 16 December 1921, [RSB].

[22]FWS to RSB, 8 September 1950, [RSB].

[23]FWS to Charles Schuchert (CS), 29 October 1929, [YMA].

[24]FWS to CS, 13 September 1929, [YMA].

[25]FWS to Fred B. Snyder (FBS), 30 March 1935, [FBS-32].

[26]FWS to RSB, 22 February 1930, [RSB].

[27]Securities Commission Minutes for 21 June 1918 for a hearing on the Basin Wyoming Oil Company of Duluth, Minn. Sardeson reported unfavorably, but action was deferred pending further information from the applicant, Minnesota State Archives, Record Group Securities Commission, Series Minutes, 1917–1925, [MHS].

[28]FWS to Kenneth E. Caster, 20 July 1940, [KEC].

[29]FWS to RSB, 15 September 1933, [RSB].

[30]FWS to RSB, 22 February 1936, [RSB].

[31]The National Silica & Pumice Co., of Meade, Kansas, to FWS, 8 December 1919, Minnesota Securities Commission Correspondence, Book 8, Minnesota State Archives, [MSH].

[32]Russell D. George, State Geologist of Colorado to WHE, 10 December 1917.

"I am sending you herewith an exact copy of a report made by the Blue Sky inspector for Minnesota. It seems unnecessary to me to comment, except to suggest the desirability of the state employing for such purposes, a man possessed of the ordinary sanity, together with the knowledge, if ever so elementary, of the thing on which he is supposed to report. It would also be desirable that he should not reach conclusions before hand and then search the proposition to find confirmation or substantiation of his prejudices. He

takes absolutely no account of structure or geological conditions, pays no attention to the report, and does not even test the well to see what was in it. I found 425 feet of oil in the casing and over-flowing the top of the casing. I found abundant evidence to show that it had run far down the valley of the Nueces River. I examined the outcrop of asphalt, shipped large samples to test the material as to content of hydrocarbons and the quality and uses to which he materials could be put and reported accordingly . . . . It would seem to me a proper matter for the consideration of the Association of State Geologists."

Emmons pencilled "Sardeson" on the letter beside the words "Blue Sky inspector," but did not keep the report George sent him. Thus we cannot know who signed it, and there is no reply to George in the file. George dated his letter "10-12-17," probably 10 December, and Sardeson was appointed examiner on the 19th of that month. If the date be construed as October 12th, the culprit was even less likely to have been Sardeson. Records from those early years at the Colorado Survey no longer exist, so George's letter is a dead end, [MGS].

[33]E. Russell Lloyd, of Casper, Wyoming, to WHE, 29 March 1920. Lloyd wrote regarding a recommendation for a job-seeking student, but asked about Sardeson parenthetically, [MGS].

[34]FWS to RSB, 15 January 1931, [RSB].

[35]FWS to RSB, 14 December 1953, [RSB].

[36]FWS to Edward O. Ulrich, 8 January 1925, [NA].

[37]*Minneapolis Journal,* June 1927 through December 1928. They appealed, but as no record of reversal by the Court of Appeals exists, the appeals must have been denied.

[38]This demonstration was performed for the present writer, who made a report at the time at the time to G. M. Schwartz, the Director of the MGS. See also Morey (1984, p. 26).

[39]FWS to FBS, 21 July 1939, [FBS-37].

[40]FWS to FBS, 19 August 1939, [FBS-38].

[41]See also the more detailed account of this operation in Morey (1984, p. 23).

[42]Record of interview with Marion Sardeson Buyken, 1984, supplemented by subsequent interviews and correspondence.

[43]Compare this account with that by Morey (1984, p. 24).

[44]FWS to President Marion L. Burton, 6 September 1919, [PP].

[45]FWS to WCB, University of Minnesota, 3 February 1949, [WCB]; FWS to Eva Jerome, 28 November 1947, [CWJ]; FWS to FBS, 15 November 1948, [FBS].

[46]Probably Frederick G. Clapp of the USGS; Mr. Lewis cannot be identified.

[47]Personal communication from W. A. Morton, Wyoming oil man and longtime student of the petroleum game in Wyoming, December, 1995.

[48]FWS to RSB, 4 November 1927, [RSB].

[49]FWS to Ellison Orr of Waukon, Iowa, 22 November 1927, [Archives of Effigy Mounds National Monument, McGregor, Iowa].

[50]FWS to RSB, 30 September 1928, [RSB].

[51]FWS's conversation with Emmons, reported in letter to President L. D. Coffman, 1 February 1929, [PP].

[52]FWS to CS, 28 September 1929, [YMA, Box 22].
[53]FWS to RSB, 15 February 1930, [RSB].
[54]FWS to RSB, 9 March 1930, [RSB].
[55]FWS to RSB, 20 March 1931, [RSB].
[56]FWS to RSB, 11 November 1931, [RSB].
[57]FWS to RSB, 7 December 1931, [RSB].
[58]FWS to RSB, 3 February 1932, [RSB].
[59]FWS to RSB, 6 January 1934, [RSB].
[60]FWS to RSB, 24 September 1934, [RSB].

7

THE MINNESOTA SURVEY AND THE DEPARTMENT

Nursing a bitter wound from having been expelled from the University, Sardeson had an unhappy and contentious relationship with both the Minnesota Geological Survey (MGS) and the Department of Geology after he left the University. The Survey and the Department were different entities, but embodied the same persons for the most part, and both were headed by W. H. Emmons, the man who fired Sardeson. His troubles with the MGS were concentrated in the few years following his dismissal from the University in 1913; his scorn for the academic program of the department continued for the remainder of his life.

The professional staff of the MGS consisted of members of the Geology Department at the University. They were on salary from the University for their teaching functions, and they were paid from the MGS appropriation in the University budget while actually employed for field and office work for the Survey. Emmons drew about one-third of his salary from the Survey budget each year. He was the only one regularly employed, but he was still only part-time on Survey work. The Survey also hired students from time to time as temporary assistants.

Sardeson lived only a few doors from the campus and after his dismissal he continued regular use of the libraries and visits to friends in various teaching departments. As we have seen, he and Leverett carried on their mapping of glacial deposits and editing of reports for about six years after 1913, and Sardeson was paid for that work partly by the MGS during part of that interval. Thus he was in intimate contact with the Survey and the Department, although not of them.

Two aspects of Sardeson's tenuous relationships with official Minnesota geology require special emphasis. The first was personal: Sardeson's attempt to punish Emmons for leaving his name off the first-completed part of the state map of glacial deposits (Leverett, 1914) and the accompanying MGS Bulletin 12 (Leverett, 1915). His attack extended from 1916 until 1919, when Sardeson gave up believing that he might unseat Emmons and become state geologist himself. The second was professional: over many years Sardeson was routinely scornful of the academic program in the department. He thought the department had "gone commercial" and no longer taught paleontology with the intensity that Sardeson had. In particular, he was amused by the fact that some members of the department joined in the Greenwood [Oil] Company, a poorly organized venture that never amounted to much.

SARDESON VERSUS THE MINNESOTA GEOLOGICAL SURVEY

Outraged at being excluded from an authorship he could rightly claim, Sardeson attempted to punish Emmons by attacking the Survey. Exaggerating his influence, he claimed many years later that he had managed to force the termination of the appropriations for the Survey about 1916.[1] There is no evidence that cooperative funds from the U. S. Geological Survey (USGS) were ever seriously threatened. In fact, Emmons had friends in Washington and Sardeson had a few enemies there. Nor was the state support interrupted, though Sardeson certainly succeeded in annoying Emmons and in getting his name included with Leverett's on their four subsequent publications (Leverett and Sardeson, 1916, 1917a, 1917b, 1919).

Sardeson opened his campaign against Emmons rather obliquely, with a request in early 1916 for a copy of the record of his "case" held in the President's office.[2] President Vincent replied that his own memoranda of meetings and interviews were a part of the University's official records, but he supposed that Sardeson had the right to see and copy them. He would seek advice from the President of the Board of Regents.[3] That person, F. B. Snyder, denied Vincent's request on the grounds that the records concerning Sardeson were the "private records of the Board."[4] Vincent dutifully passed a copy of Snyder's letter along to Sardeson, reaffirming the refusal of copies.[5] But Sardeson was just getting started. Replying soon, he taunted Vincent for being unwilling to copy personnel records concerning a former faculty member without permission from higher authority.[6]

He also opened the attack on another front. Seeking additional proof to demonstrate his authorship, he requested copies of the contracts between the USGS and the MGS made in March 1912 and for subsequent years.[6] Although Sardeson had been employed by the MGS "for the past three seasons" (Leverett, 1915, p. 2), he was not listed as a co-author on MGS Bulletin 12, which recently had been issued, no doubt prompting his complaint. Vincent bucked this new request to Emmons, and told Sardeson so.[7] Emmons tried to stonewall Sardeson, saying that the relevant parts of the contract were already known to Sardeson; although they were not confidential, he would not give them "promiscuous distribution." Emmons suggested that Sardeson might get them himself from the USGS.[8] Sardeson appealed once again to Vincent,[9] who urged Emmons to petition the USGS for whatever permission he deemed necessary.[10] This Emmons did on 28 February 1916.[11]

Having nothing to hide, the USGS proved quite cooperative. G. O. Smith, Director of the Survey, informed Emmons that the USGS-MGS cooperative agreements were informal, but might be shown to anyone.[12] Emmons stalled again and even consulted with President Vincent further,

for the promised copies of USGS-MGS agreements were not delivered until nearly two weeks later.[13] The two agreements furnished were for fiscal years 1912–1913 and 1914–1915. That for 1914–1915 provided that the state would "furnish and maintain . . . F. W. Sardeson" to assist Leverett with the work of mapping the state, irrefutable proof of his claim for coauthorship. Sardeson promptly thanked Vincent for the copies received, but also complained that they were not the ones he had requested.[14] He had not received the one made in March of 1912, which he realized might have been oral, nor the one for 1913–1914, both of which he had requested.[15]

In further support of his claim to equal authorship, Sardeson pointed out to Vincent that the 36th Annual Report of the Director of the USGS (Smith, 1915) had styled Sardeson as "of the State Geological Survey." Surely this language had not come from Emmons, who wished only to be rid of Sardeson! Furthermore, Sardeson also reminded Vincent that he had always been paid the USGS rate ($7 per day), rather than the lower ($5 per day) rate of the MGS.[14] He insinuated some form of double dipping (payment by both federal and state agencies for the same work) on the part of the MGS, a practice that Vincent had recently assured the Legislature did not occur in the University. Vincent surely regarded Sardeson's implied threat as empty, for the records plainly do not permit the conclusion that double-dipping occurred. USGS funds for cooperative work with the MGS were simply passed through to the MGS, via the University budget.

Having clearly enjoyed harassing the MGS, Sardeson conceded as much. "It is not my desire to question the legality of the 'State Geological Survey' " he told Vincent, "and further, this matter has not been carried to the Federal Survey."[14] When Emmons learned of Sardeson's more reasonable posture, he supplied a copy of the agreement for 1913–1914 to Sardeson.[16] It had provided $550 dollars for salary and expenses for an assistant to Mr. Leverett, no doubt Sardeson, although he was not identified by name or title. Sardeson's complaints thus fell flat, and the harassment of the MGS lapsed until later in the year, when he did attack the legality of the MGS, from a new direction.

In mid-August, 1916, F. L. Ransome, Acting Chief Geologist, reported to Emmons that his office "had recently been informed by one who claims that he knows the facts, that there is no State Geological Survey of Minnesota, and that the office of State Geologist does not legally exist." Because the USGS was careful to make cooperative agreements only with duly constituted surveys in each state, Ransome asked for Emmons' comments on the claim.[17] Emmons sent Ransome's query on to Vincent, who asked the University Comptroller, G. H. Hayes, to obtain an opinion from the Minnesota attorney general.

Assistant Attorney General C. L. Weeks assured Hayes the unknown accuser's claims were false. Legislation in 1872 that had created the Geological and Natural History Survey and assigned responsibility for it

to the Regents of the University had never been rescinded.[18] No publicly supported geological survey work had been done between October 1900, when Winchell retired, and 1911, when it was reestablished under Emmons as the Minnesota Geological Survey, still under the Regents of the University. During that 11-year interval the income from state swamp lands dedicated to the Survey had been directed to botany and zoology, which had received very little money during the Winchell years, 1872–1900 (Schwartz, 1964; Morey, 1988).

The attorney general's opinion further affirmed that the position of "State Geologist" had never been explicitly created, but that the term had often been used informally in legislation and in regulations. Emmons had been hired as and was formally *Director* of the MGS. Called "State Geologist" from time to time he may have been, but such usage was informal. Emmons forwarded the legal opinion to David White at the USGS,[19] and USGS Director Smith assured Sardeson that the MGS and the relations it had with the USGS were normal and legal in the opinion of the attorney general of Minnesota.[20] On receipt of the full text of the opinion, Smith wrote Sardeson and quoted from the opinion, showing that his claim of illegality of the MGS was unfounded.[21]

Still not mollified, Sardeson wrote again to Smith, claiming that a bill before the Legislature (apparently never passed) seemed to show that the "Survey" had never been a part of the University.[22] He too had talked with Assistant Attorney General Weeks, and sought to show from that conversation that the Winchell Survey had never been "an integral part of the University." He again raised the issue of double-dipping, next to which Emmons made the marginal note "not true." Smith responded that the USGS was satisfied with the arrangements, and he abjured Sardeson from speaking on those issues as though he were speaking for the USGS.[23] Through bad tactics (his erroneous conclusions from the various texts) and his failure to gauge the capacity of bureaucrats to fend off attack, Sardeson had clearly lost his fight to discredit the MGS and Emmons.

It has seemed so far that Sardeson's sole objective was harassing Emmons to punish him for denying him joint authorship with Leverett. Another motive, perhaps the main one, was revealed in a long letter that Leverett wrote to USGS Chief Geologist White. Sardeson "has . . . been trying to find a place to do geologic work in the State," Leverett reported," and is in hopes that the position of State Geologist will eventually be created and that he may receive the appointment. He feels as I do that he is qualified for this position."[24] So, Sardeson was not only angry at Emmons; he also hoped to pull his office from beneath him and become "State Geologist" himself! Unfortunately for his own cause, Sardeson succeeded merely in irritating officials in both Minneapolis and Washington.

Almost two years later Sardeson went on the attack again, with a complaint to University of Minnesota President Burton (who had succeeded

Vincent in 1917) that double-dipping had been arranged in USGS-MGS cooperative agreements "two years ago."[25] Comptroller Hayes promptly looked up the 1916 opinion of the attorney general and reassured Emmons of the sound status of the MGS.[26] Emmons concurrently gave the lie to this latest of Sardeson's claims. During the period of which Sardeson complained, he insisted, *no* cooperative agreements had been made between the USGS and the MGS, for each was independently pursuing "special problems in connection with the war."[27] This finally put a stop to Sardeson's attempts to unseat Emmons, efforts we can only conclude reflected the desperation of a dedicated, driven scholar who had been cut off from his most rewarding opportunities for scientific work by the powers that controlled the institution he felt driven to attack.

SARDESON, THE DEPARTMENT, AND THE GREENWOOD COMPANY

Sardeson was always contemptuous of the strong economic bent of the Department of Geology under Emmons's leadership. In particular, he was amused by what he called the "oil geologists" on the faculty. World War I stimulated a great burst of exploration for petroleum and natural gas in the teen years, and hundreds of companies, mostly small, were formed to seek and exploit the hoped-for bonanzas. This occurred at about the time that Sardeson began work for the state assessing the asset value of companies (mostly oil companies) that sought licenses to sell stock in Minnesota.

Sardeson's new responsibility to the state focussed his attention on the national craze for petroleum exploration and drilling at about the time that a small oil company was formed by businessmen in Minneapolis in 1917 (Owen, 1973).[28] The Greenwood Company ("Greenwood" hereafter) concentrated its exploration on the Midcontinent region. Some faculty members of the University were among its investors. C. R. Stauffer, Sardeson's successor as Professor of Paleontology, was an employee who did geologic field work for it, but also had a veto power over exploration recommendations. In fact, Stauffer was granted a leave without pay for the school year 1918–1919, which he spent working in several states for Greenwood.[29] For a brief period, the company employed as its treasurer former Professor W. A. Schaper, who needed work because he had just been fired by the Regents for alleged pro-German sympathy (as described in Chapter 2). Because Emmons was director of the MGS, he apparently did not work for the company, although he may have been a stockholder.[30]

The chief executive officer of Greenwood was Frank G. Jewett, a mining engineer and former Vice-President of the E. J. Longyear Company. Jewett's office was in Minneapolis, and reports from field geologists were directed to him.[31] The Chief Geologist of Greenwood was Roy S. Hazeltine, one

of the founders of the American Association of Petroleum Geologists. Hazeltine's office in Kansas City, Missouri, was the center of exploration operations for Greenwood (Owen, 1973). Through January 1917 at least, Hazeltine had been with the Empire Gas and Fuel Company; thus Greenwood may have been formed early in 1917.[32] When Hazeltine moved from Empire to Greenwood he brought with him several able young geologists; the first among them was E. W. Owen, later a famous oil-finder.[33]

One of Greenwood's earliest employees was A. I. Levorsen, later one of the world's most distinguished petroleum geologists. Levorsen graduated from the University of Minnesota 14 June 1917, a brilliant student with an E.M. (mining engineering) degree in Geology.[31, 34] As he approached graduation, someone apparently thought to put him to work discovering oil and making money, and put the Greenwood Company together to accomplish that aim! By that August, if not before, Levorsen was mapping in Kentucky for Greenwood.[31] He quickly became an important member of an exploration team that included Stauffer and Hazeltine. In 1917 they mapped domal structures by plane table in Kentucky and Kansas.[35] Levorsen later reviewed both oil and coal occurrences in the Indiana-Kentucky region.[31] He had a stint in the army, 1918–1919, but returned to work for Greenwood during the 1919 field season. He mapped structures in Kansas as late as September of that year, when Greenwood also had an office in Wichita. Marginal data on several maps show that Levorsen usually was the rodman (the traditional post for an investigating geologist) in those field parties. Levorsen submitted several reports to Greenwood on the Illinois-Indiana-Kentucky coal basin as late as December of 1920. In all, he was with the company for at least three years, including the army service.[35]

The Greenwood Company worked in regions where many other companies had success, but it apparently folded in 1921 or 1922.[36] Greenwood's failure was partly due to bad luck in exploration, but the company faced structural difficulties as well (Owen, 1973). If Chief Geologist Hazeltine had been allowed to make the key decisions, things might have gone better, but policy was made in Minneapolis, where knowledge of the petroleum industry was minimal. As Owen later recalled, there was "also a scientific adviser in Minneapolis who could second-guess the Chief Geologist in Kansas City" (Owen, 1973). That person was without doubt C. R. Stauffer. The historical importance of the company is that it employed two giants of petroleum exploration, A. I. Levorsen and E. W. Owen. Levorsen became a world-famous oil man, for companies and as an independent, and Dean of the School of Mineral Sciences at Stanford University. Owen was one of the most successful oil-finders in the Midcontinent and Gulf Coast regions and, later, an important historian of the petroleum industry.

Sardeson's oft-spoken contempt for the company was a classic example of the hard-edged orneriness that made him a pariah in his own beloved

profession. His contempt for "oil geologists" stemmed from the high priority he placed on what he saw as pure science. The oil people were mere technicians in his view. Yet this very intolerance, evident in some other instances as well, not only laid down a staunch defense of critically important professional values. Tragically, it also marginalized a man of great learning, dedication, scientific inventiveness, and originality—if not, and we can never know, so arduous were the constraints he faced—true genius. To successors, his life is both an example and a caution.

NOTES

[1]Frederick W. Sardeson (FWS) to W. Charles Bell (WCB), University of Minnesota, 3 February 1949, [WCB].

[2]FWS to President George E. Vincent (GEV), 11 February 1916, [PP].

In the letter Sardeson also alleged a slander from one of his former colleagues at the hearing held prior to his dismissal. Nothing of that sort is recorded in Vincent's memorandum (Appendix 2), and Vincent denied recalling such an allegation (Note 3). I have searched out the details of that red herring and found that it has no merit; furthermore, it has no relevance to the Minnesota Survey.

[3]GEV to FWS, 12 February 1916, [PP].

Vincent's reference to his own memoranda is the proof that he had written Appendix 2, as internal evidence has already suggested. Appendices 3 and 4 were probably also meant by Vincent in this letter.

[4]Fred B. Snyder (FBS) to GEV, 16 February 1916, [FBS & PP].

[5]GEV to FWS, 17 February 1916, [FBS & PP].

[6]FWS to GEV, 19 February 1916, [PP].

Sardeson accepted the fact that his records were confidential and expressed satisfaction that they would not be made public, but he also initiated the inquiry into the MGS.

[7]GEV to FWS, 23 February 1916, [PP].

[8]William H. Emmons (WHE) to FWS, 24 February 1916, [PP].

[9]FWS to GEV, 26 February 1916, [PP].

[10]GEV to WHE, 28 February 1916, [PP].

[11]WHE to George O. Smith (GOS), 28 February 1916, [MGS]. Also, WHE to FWS, 6 March 1916, [PP].

[12]GOS to WHE, 9 March 1916, [MGS].

[13]WHE to FWS, 23 March 1916, [PP].

Emmons was obviously miffed, for he sent the agreements with a curt, one-sentence cover, saying he did so "by order of President Vincent." The two agreements furnished were for fiscal years 1912–1913 and 1914–1915.

[14]FWS to GEV, 25 March 1916, [PP].

[15]The first of the two may have been just Emmons' editorial carelessness, for the agreement for 1912–1913, signed on 5 November 1912, may well have been made orally the previous March.

[16]WHE to FWS, 13 April 1916, [MGS].

[17]Frederick L. Ransome, Acting Chief Geologist, USGS, to WHE, 18 August 1916, [MGS].

WHE replied to Ransome, 24 August 1916, saying that the MGS had "a satisfactory legal status," and that the President would "define it accurately and send you a copy." He also asked for the name of the informant mentioned in Ransome's 18 August letter. Ransome noted in the margin of his copy, "This seems scarcely necessary." Emmons must have known in his own mind that Sardeson was the informant, [NA].

[18]C. Louis Weeks, Asst. Attorney General of Minnesota, to George H. Hayes (GHH), University Comptroller, 26 September 1916, [MGS].

[19]WHE to David White (DW), 30 September 1916, [MGS].

[20]GOS to FWS, undated. Date was 8 November, according to GOS's letter of 12 January 1917 to Sardeson (Note 23), [MGS].

[21]GOS to FWS, 13 December 1916, [MGS]. This is one of two letters he wrote to Sardeson on this date; the other pertained to quite different business.

[22]FWS to GOS, 7 January 1916 [sic, actually 1917], with a copy to WHE, [MGS].

[23]GOS to FWS, 12 January 1917, [MGS].

[24]Frank Leverett to DW, 2 June 1917, [NA].

[25]GHH to WHE, 30 December 1918. Sardeson's letter to President Burton does not survive, but is referred to in this letter from the Comptroller of the University, [MGS].

[26]GHH to WHE, 9 January 1919, [MGS].

[27]WHE to GHH, 10 January 1919, [MGS].

[28]There is no evidence that Sardeson ever had official dealings with this company, nor does he mention such in his letters.

[29]Minutes of the Minnesota Board of Regents for June 1918, [UOM].

[30]Emmons certainly was keenly interested in petroleum geology at the time, for he published the Geology of Petroleum, an early textbook on the subject, in 1921. The book contains no references to the Greenwood Company or its employees.

[31]The A. Irving Levorsen collection (# 10515) at the Petroleum History and Research Center of the American Heritage Center, University of Wyoming.

[32]Roy S. Hazeltine's letter, on Empire Gas and Fuel Company stationery, to Everette L. DeGolyer of the Mexican Eagle Oil Company, 3 January 1917, [DeGolyer Foundation Library, Southern Methodist University].

[33]Greenwood is mentioned in Owen's history of the petroleum industry (Owen, 1975, p. 322). Additional data on Greenwood's personnel and history are in the Edgar W. Owen Papers (# 6558) at the Petroleum History and Research Center of the American Heritage Center of the University of Wyoming. They contain a report by him to Greenwood on 21 counties in north-central Texas in March of 1918.

[34]Graduation records, [UOM].

[35]Duplicate copies of some of Greenwood Company's maps and reports, furnished to me (March, 1993) by John C. Freeman, petroleum geologist of Wichita Falls, Texas, and Levorsen's son-in-law.

[36]The Greenwood Company is listed in the 1923 Minneapolis city directory, but that entry may well have been purchased the year before.

CONCLUSION

Sardeson's higher education and years of professional practice spanned more than 50 years, and he continued to pour forth opinions and advice to friends for nearly another 20 years. What does the story of this man teach us about him, about the practice of geology nearly a century ago, or about the academic scene in that same period?

There is abundant evidence in his publications and his letters that the man was very bright and very able. He had an innovative imagination as well. In several corners of geology he was ahead of his time: the solely eolian origin of loess, a practical regional stratigraphy based on the reality of the rocks and fossils in the region, and a concept of species based on the variation in a population rather than on trivial differences in countless individuals. Full of self-assurance, he urged his views against established orthodoxy without thought of the consequences of his presumption. His repeated urging of the eolian origin of loess surely irritated the great T. C. Chamberlin and earned him Chamberlin's enduring disdain. There is sometimes a price to pay for being right, even though glacial geologists generally had adopted Sardeson's view before long before Chamberlin died.

With stratigraphy and species, Sardeson had no individual antagonist such as Chamberlin; he was working against older and less effective patterns of science that were entrenched in the geologic profession. Perhaps because of this, it took longer for new patterns to be widely used. Sardeson had urged the de novo description of the rocks of a region during the exploration of that region and the careful correlation of those new findings—using fossils—with the stratigraphy of regions already well known. This policy was almost forced on the geologists who were overspreading and exploring the country west of the Mississippi a century ago, not long after Sardeson had applied the principle in the Midwest. Although Sardeson urged the correct policy, contra the workers of the Midwest, he did not single-handedly establish it in the profession as a whole. Sardeson, on the other hand, was on the wrong track in the matter of definition of geologic formations; he always insisted on using faunal features, if available, rather than lithic characteristics for the purpose. Given that rocks having no fossils are more voluminous than those with fossils, the profession has developed the policy of identifying and naming formations for their lithic properties.

A populational or statistical approach to the distinction of species did not come into wide use in geology until about World War II, and then because of advances in genetics and evolutionary biology. Sardeson had been out in front of most of his contemporaries, but because of constraints on his publications we cannot say that Sardeson helped cause the change. He only signaled the need for it far earlier than most. Sardeson early on urged that fossil samples be referred closely to the rock layers from which

they were taken. Such control gives chronologic value to the specimens, for their exact position in the succession of rock layers is known. This ethic still is not observed universally, with the result that some paleontologic works are useful only for the anatomy described, and not for chronologic conclusions.

With so much to his credit, why did Sardeson not have a long and satisfying career at the University of Minnesota? As his publications and letters make plain, his was a prickly personality, something that he admitted himself to close friends. The colleagues who drove him from the University probably thought him a megalomaniac, and the record of their complaints shows that he was overbearing and highly condescending. Even so, it was devastating to be fired because your colleagues didn't like you, and that harrowing occurrence probably increased his natural tendency to paranoia. His personality assuredly got in the way of his development as an academic and even as a scientist, but other factors were at work at the same time.

Sardeson seems, in several respects, not to have made the turn from the ways prevailing in the 19th Century to the new goals that tempted geologists in the early 20th Century. Through much of the earlier period paleontology was the end-all of geology, and he got into geology about the time that other aspects of geological science were beginning to gain importance. But Sardeson never moderated his view that paleontology and stratigraphy were primary, and was therefore unable to abide the growing interest in economic geology and petroleum geology that characterized the first half of the 20th Century. Perhaps some Regents of the University did have conflicted interests, but their desire to turn the teaching of geology into newer and broader paths was not wrong of itself. Failing to adapt to those changes—that were truly nationwide—made Sardeson's already insecure relations with his colleagues worse.

Many 19th Century scientists, not just the rich or the famous, were able to function in a solitary fashion without the troubling need to accommodate to their peers in the profession or their colleagues in some institution. As professional organizations developed and as colleges and universities grew in size and complexity, individual freedom for the scientist decreased. One had to begin to relate to others in a profession, to bend some effort to be a part of a society, to respond to the requirements of the editor of a society's journal, to seek the support of others for a research project, or to cooperate in some degree with colleagues in a college or university. As time passed it became necessary to respond to the practices or rules of a group, such as a department of geology. The granting of academic tenure and the requirement of a period of apprenticeship prior to tenure came to many institutions (but not the University of Minnesota) about the time, or very soon after, Sardeson was dismissed. Perhaps he even may not have been able to adapt to such a program, had it been in force at Minnesota before he was hired.

Besides failing to "make the turn from the 19th to the 20th Century," Sardeson, for all his skill and imagination, could persist in a cherished opinion that did not stand scientific scrutiny. The best example is his refusal to recognize early Paleozoic bryozoans as such and his persistent claim that they were corals, against the rest of the world's paleontologists. In the history of geology—even recently—such determination to be wrong is hardly unique.

There are several things about Sardeson's career that are very much to be regretted. That he was philosophically so rigid in some ways at the same time that he was imaginative and innovative in others is unfortunate, but only human. That he was so rudely handled by the University of Minnesota was both a professional and personal tragedy. One can barely imagine the pain he endured—at the time and for the next 45 years—because of that episode. For geologists in the early and middle years of this century, the thing to be most regretted is that they ignored Sardeson and his teachings—for the wrong reasons—and thereby diminished themselves and delayed adoption of some good ideas that he had pioneered.

APPENDICES

APPENDIX 1—
EMMONS RECOMMENDS DISMISSAL

Letter from Dean John F. Downey of College of Science, Literature and the Arts to President George E. Vincent 26 April 1913 [PP]. The Dean enclosed two letters from Professor Emmons: in the first he recommended "that the services of Assistant Professor Frederick W. Sardeson be discontinued at the end of the present University year, but that his full pay be continued through the year 1913–14." In the other letter he recommended dismissal of the geographer at half pay. Respecting Sardeson, Downey supported Emmons as follows:

> In case of Mr. Sardeson the University has taken on even greater responsibility [as compared to the geographer]. He was appointed Scholar in 1892, at $350. Since then his salary has been raised successively as follows: $400, $500, $750, $800, $850, $1000, $1100, $1200, $1500, $1700. Professor Hall, the former Head of the department, very much wished that Mr. Sardeson would receive a call elsewhere, but he could never bring himself to the point of recommending that Mr. Sardeson be discharged. [This opinion is not supported by any other record, but Downey knew Hall personally.] He has been kept on and on until he has now been in the department for twenty-one years [of which six were as a student or on *ad hoc* non-faculty appointments; he was appointed Instructor in 1898]. As he would probably have difficulty in securing a position elsewhere or in taking up some work that would furnish support, the University should, in my judgment, continue his full pay through the next year.
>
> I recommend that Mr. Sardeson have leave of absence next year on full pay . . . and that [his] connection with the University terminate at the end of the year 1913–14.
>
> This recommendation is approved by the Advisory Committee.

APPENDIX 2—
MEMORANDUM OF THE UNIVERSITY PRESIDENT

Five-page, undated "Memorandum Concerning the Case of Mr. Sardeson," that was prepared by President G. E. Vincent, probably on Monday 7 July 1913. Although unsigned, its contents and President Vincent's letter of 9 July (Appendix 4) assure that he wrote it, probably

at home, as a reading aid for the Board of Regents meeting the following Wednesday. In a letter to Sardeson of 12 February 1916 Vincent referred to "a memorandum prepared by me concerning various group meetings and interviews"; that demonstrates his authorship beyond question. The original memorandum and the letter are filed in the "Presidential Papers" at the University Archives. All of it is printed here.

> "To propose the dismissal from the University of a man of ability, scholarship and teaching capacity is unusual enough to require special explanation and justification.
>
> The Department of Geology, through its head, and with the approval of all members of the Department, made the recommendation sometime ago 'that the services of Assistant Professor F. W. Sardeson be discontinued at the end of the present University year, but that his full pay be continued throughout the year 1913–14.'
>
> This recommendation was unanimously endorsed by the eight members of the Advisory Board of the College of Science, Literature and the Arts, and with the approval of the Dean was transmitted to the President.
>
> On Saturday morning July 5th the Head of the Department, the Dean of the College and the President held a conference with Mr. Sardeson. As a result of this conference it seemed wise to hold a departmental meeting, which was called for Sunday evening July 6th. All members of the Department with the exception of Mr. Soper were present, as was Dean Downey of the College of Science, Literature and the Arts.
>
> After a three-hour discussion of Mr. Sardeson's relation to the present colleagues and to the Department in the past, Mr. Sardeson said that while he felt that the charges against him had been greatly exaggerated, it was obvious that he had failed to secure the confidence and good will of his colleagues, some of whom had been his students and at one time close friends.
>
> One member, Mr. Lehnerts [the geographer], explained that while he had concurred in the original recommendation that Mr. Sardeson be dismissed, he a [sic] later felt that Mr. Sardeson might be able to control himself as to maintain cooperative relations with the other members of the Department.
>
> After this full discussion, the members of the Department were requested once more to vote as to Mr. Sardeson's retention. All but Mr. Lehnerts voted in favor of carrying out the original recommendation, namely, that Mr. Sardeson's relation to the Department be terminated. Mr. Lehnerts again expressed the belief that it would be impossible to go on if Mr. Sardeson were not

radically to change his attitude, but that he (Lehnerts) thought that he (Sardeson) could control himself.

Mr. Sardeson said that he would not voluntarily withdraw and would have to be dismissed if it were decided that he could no longer continue as a member of the Department.

The President indicated his intention to recommend to the Board that Mr. Sardeson be given leave of absence for one year on full pay with the understanding that at the end of the year his appointment would terminate, and that he would not be reappointed.

Mr. Sardeson at first said that he would accept no salary in these circumstances, but he seemed later to modify his position on this point.

The President asked Mr. Sardeson if he desired to appear before the Board and speak in his own behalf, assuring him that this opportunity would be afforded. Mr. Sardeson replied, however, that he preferred not to appear personally, but asked the President to say that he hoped the Board would give him an opportunity to show that he could control himself and get into the right relations with his colleagues.

Mr. Sardeson also expressed the hope that the President would have interviews with three members of the faculty whom he (Sardeson) mentioned as his close friends.

After Mr. Sardeson had left the room there was further discussion of the situation. The President urged upon the members of the Department that every point should be strained in the interests of Mr. Sardeson if there was any reasonable hope of his being able to readjust himself to the situation.

Again all the members present except Mr. Lehnerts reaffirmed their belief that it would be impossible for Mr. Sardeson to modify his personal characteristics, which have become a second nature and are to a large degree unconscious with him.

It should be pointed out that Mr. Lehnerts has been transferred to the Extension staff and after August 1st will no longer be connected with the Department of Geology.

On Monday the President held a conference with the three friends of Mr. Sardeson whom he had designated. To these three men the whole story was told in detail. Two of the men expressed the deepest regret that matters had come to a climax, but said that they clearly recognized the characteristics which made Mr. Sardeson so serious a problem. They concurred in the conclusion that the only solution was for Mr. Sardeson to leave the institution. One declared that he and others had repeatedly warned Mr. Sardeson that he could not safely indulge his habits of detraction and sarcasm.

The third friend demurred and at first was rather inclined that it might be well to let all the members of the Department go and to retain Mr. Sardeson because of his abilities, his long connection with the University and his knowledge of the geology of Minnesota. On being pressed, however, he did not maintain this view. While he could not be said to concur in the plan of dismissal he was unable to offer any other solution for the problem.

On Tuesday morning the President had a conference with former Regent Hovland [a mining engineer from Duluth who had long been interested in geologic education], who has been familiar with the geological situation for a number of years. Mr. Hovland expressed emphatically the opinion that unfortunate as it was in many respects to sever Mr. Sardeson's connection there was no hope of harmony and team-play in the Department of Geology so long as Mr. Sardeson was connected with it. He expressed the belief that Mr. Sardeson could not change the characteristics which had given rise to the discord in the Department.

The President also interviewed a member of the Advisory Committee of the College of Science, Literature and the Arts, and learned from him that the action of the Committee was not a mere perfunctory endorsement of the departmental recommendation, but represented a strong conviction on the part of the Committee that Mr. Sardeson ought not to continue a member of the Faculty.

To make specific charges against Mr. Sardeson is difficult. It is a question of temperament and disposition. His egotism is extraordinary. It leads him to assume an air of patronage which is galling. Again he has a bitter and sarcastic tongue which he uses freely and constantly. It is his nature to belittle other people and to speak of them contemptuously. He has talked to every member of the Department about other members and has by innuendo and implication, as well as by direct statement, sowed constantly the seeds of suspicion and discord.

It seems that his early experiences in the Department were unfortunate. He has had the feeling apparently that he must fight his way, that the world was against him, and that he must be constantly on his guard to protect himself. The result is that he has acquired certain characteristics that are so ingrained that those who know him best have no confidence that he could change them. When in the conference of the Department he said that things had been going more smoothly of late, at least three of his colleagues declared that they had been for the last few months consciously avoiding him.

Mr. Sardeson has for years talked freely about the Department troubles to other members of the Faculty and has in this way

contributed to the demoralization which has been characteristic of the Department of Geology for a long time.

Among geologists throughout the country Mr. Sardeson's reputation for sarcasm, bitterness, and exaggerated egotism is well-known and widespread.

In all the circumstances, after the most careful consideration, I have reached the conclusion that in the best interest of the University I must recommend that Mr. Sardeson be given leave of absence for one year from August 1st, 1913, on full pay and that he be notified that his appointment as Assistant Professor will terminate on July 31st, 1914, and that he will not be re-appointed."

APPENDIX 3—MEMORANDUM FOR THE REGENTS

President Vincent also prepared a brief memorandum for the Regents, which was filed at their meeting and became a part of their minutes. It is transcribed here from the copy in the Presidential Papers of the University Archives.

Special Memorandum concerning Assistant Professor F. W. Sardeson

The Head of the Department of Geology with the concurrence of all but one member of that Department recommends that Mr. Sardeson not be continued as a member of the Department of Geology.

The Advisory Committee of the College of Science, Literature and the Arts unanimously approves this recommendation and the Dean of the College transmits it with his approval. After careful consideration I recommend that Mr. Sardeson be given leave of absence on full pay for one year from August 1st, 1913, that his appointment expire on July 31st, 1914, and that he not be reappointed after that date. The Executive Committee unanimously approved this recommendation on July 9th, 1913.

APPENDIX 4—NOTIFICATION OF DISMISSAL

Before the close of July 9th, President Vincent wrote to Sardeson to tell him the action of the Board. From Presidential Papers.

Mr. F. W. Sardeson,
Brainerd, Minnesota.

My Dear Mr. Sardeson:

At the meeting to-day, which for lack of a quorum was an Executive Committee meeting rather than a meeting of the full Board, the following action was taken with reference to you:-

'Voted to grant leave of absence on full pay for one year from August 1st, 1913, to Assistant Professor Sardeson in accordance with the special memorandum made a part of the supplementary minutes.'

This memorandum recites all the circumstances and concludes with the recommendation that you be granted leave of absence for one year with full pay, that your appointment be terminated with July 31st, 1914, and that you not be re-appointed after that date.

I sincerely hope that you will allow us to be of service to you in re-adjusting yourself to new work. We shall do all in our power to protect you.

It is not for me to give you advice, but I strongly urge you to keep this situation to yourself until you have an opportunity to take counsel with such friends of yours as Messrs. John and Anthony Zeleny and Mr. Erickson [Erikson, actually], with whom, as you requested, I had a conference.

Regretting keenly the necessity of making this communication to you, I am,

Yours sincerely,

GEV-B

A pencilled note at the bottom reads "Copy to Dean Downey and Mr. Emmons."

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FAMILY DATA FROM VARIOUS SOURCES

(Notes in brackets are interpretations of M. P. Weiss)

WILLIAM SARDESON

b. 5 August 1793[a] in Willesford [sic], Lincolnshire[a]; (3 August[o,p]); (5 August 1794[g]).

[Willsford (1610 map) was a village at road junction and bridge, 5 miles WSW of Sleaford, in central Kesteven, Lincolnshire; it is not on 1985 map of Lincolnshire.]

m. Ann Moody, 2 January 1817.[a]

d. 3 December 1854 in Naperville, Illinois[a]; (25 April 1854[o,p]).

ANN MOODY[a] (also Moodey[a] or Moodie[o])

She didn't know her exact birth year; thought she was "about twenty" at marriage.[a]

b. 5 April 1794 at Farnesfield [sic], near Southwell, Nottinghamshire, England[a]; (5 April 1796 at Farnsfield[g]); (17 August 1797[o,p]; this latter date is probably correct).

Probably orphaned; was a parlor maid at an inn when she married.[a]

m. William Sardeson, 2 January1817.[a]

d. 5 April 1887 in Argyle, Wisconsin.[a] [Is named Anna in the cemetery record.]

[Farnsfield is still a village, 3.5 miles NW of Southwell and halfway between Mansfield and Newark-on-Trent, in central Nottinghamshire.]

[Records prepared by Charles Waldron Jerome,[a] husband of Fred Sardeson's sister Eva, show a variety of spellings and some variance in dates. The spelling of place names is best taken from maps, as in the brackets above.]

[It is clear from the context of dates and places that William, although born in Lincolnshire, milled in central Nottinghamshire as a young man,

met and married Ann Moody, and subsequently moved back to south Lincolnshire to live and work.]

Ann and William had 12 children,[a] most born at Dyke, of whom Joseph was the 11th in order.[a]

JOSEPH SARDESON

b. 4 June 1839 at Dyke, near Bourne, in southern Lincolnshire, England.[a]

m. Petra Rossing, 8 May 1860,[o] probably in her family church, the Lutheran Society [the term used at that time, apparently] of Wiota, Wisconsin.

d. 19 June 1914 in Minneapolis, Minnesota.[e, o]

[Dyke was a village 1.5 miles NE of Bourne, in southern Lincolnshire, spelled Dike on the 1610 map, and no longer on the modern maps.]

[The Sardesons operated windmill(s) in Lincolnshire. The two oldest children never left England. Four of the oldest six children came to Chicago in the 1840s.[p] Parents William and Ann and their six youngest came to Chicago later and "scattered to find work."[p] The dates differ in different records.

1) when Joseph was 10 (i.e., 1849[e]);
2) when Joseph was 15 (i.e., 1854[l]);
3) in 1850 or 1851.[a,o,p]

The 1850 or 1851 dates come from Joseph's conversation with C. W. Jerome "a few years" before Joseph's death, and are probably the most accurate estimates.]

[The second wave of Sardesons came first to Kendall County and then Du Page County, both in Illinois, and some moved subsequently to Lafayette County in southwestern Wisconsin.[a] That part of Wisconsin, including the valley of the East Branch of the Pecatonica River, was populated mostly by English Methodists and Lutheran Norwegians.[m]]

JAMES SARDESON—Only one of the 11 siblings of Joseph is important to our story, his older brother James.

b. 30 November 1830, also at Dyke; [a,g,o] (14 November[p]).

It's "almost certain" that he came to the U.S. with his parents in 1850 or 1851, and soon thereafter to Argyle, Wisconsin. They were Wesley Methodists avoiding the Church of England.[g]

d. Argyle, Wisconsin, 1919?[p]

The importance of James to the story of Joseph and his children is through the Owego Mills. The excerpts below are from Catherine Barry.[h] Where cross-checking has been possible, these notes are supported by other records.

The Owego mill, the first in the valley of the East Branch of the Pecatonica River, was built with one run of stones in 1843 on Whiteside Creek [formerly Whitesides], on an 80-acre land patent, by Zophar Williams. He had moved to Wiota [formerly Wyota] Township from Monroe Township, Illinois, but was originally from Owego, in Tioga County, New York. The mill was rebuilt with two runs of stones in the following year, after a flood destroyed the first structure.[h] [The 1881 history of Lafayette County gives somewhat different details of the early days of the mill.[n]]

Williams died in 1850, and the mill was sold to Robert Thredgold [also Threadgold] who ran it for four years and died. His young widow, Marilla Sowl [later Sowls] Thredgold married James Sardeson in 1855.[g,h] The Owego "mills" included the mill and two houses.[n]

James operated the mill for several years, obviously with the assistance of and, later, partnership with his younger brother Joseph. James, and later Joseph, removed to Argyle, where James and Joseph invested in a saw- and grist-mill and James opened a drygoods store.[h]

Owego Mills operated under other owners until 1890, when the Dodgeville Branch of the Illinois Central Railroad came up the Pecatonica Valley and put small manufacturers out of business.[h]

JOSEPH SARDESON worked and attended school in Chicago during his earliest years in the U. S. and lived there from 1853 to 1855, when he moved to Owego Mills. He worked for James for 5 years and rented the mill from him for 5 years.[a] As he moved to Argyle in 1870, there is an interval of 5 years at Owego Mills not accounted for in this part of the family history.

PETRA ROSSING

b. 11 January 1839, on a farm in Lands Parish, Norway. She was a daughter of Andrew (formerly Andrease) Rossing and Bertha Välden (Wolden in the U. S.)[a,i] She immigrated to the U. S. with her parents in 1857, and they took a claim for a farm in the bend of the Pecatonica River south of Argyle, in Wiota (formerly Wyota) Township.[d] The family was German, having fled earlier to Norway.[l]

m. Joseph Sardeson, 8 May 1860.[o]

d. 9 January 1929, in Minneapolis, Minnesota.[d]

Before her marriage, Petra "went into service" with two families in Warren, Illinois, while her parents ran the farm.[a] [Although the family record says Warren, Wisconsin, Warren is in Illinois, just across the line.]

PETRA and JOSEPH had five children:

1) BERTHA ANN

b. 27 November 1861, at Owego Mills.[a,i]

m. David Duff Dryden, of Sioux City, Iowa, January, ? .

d. 1943 or 44.[q]

2) AMOS CORNELIUS

b. 2 November 1863, at Owego Mills.[a,i]

m. Minnie Eriksen Oas, 6 November 1889, in Minneapolis, Minnesota.[l,o]

d. 1929 or 30, also in Minneapolis.[l]

3) FREDERICK WILLIAM

b. 22 February 1866, at Owego Mills, Wisconsin.[a,i,l]

m. Edna Agnes Mitchell, of Seattle, 16 June 1903, in Minneapolis, Minnesota.[a,l]

d. 28 August 1958, in Seattle, Washington.[l]

He was baptized "Frederik," 31 May 1866 in the Wiota Lutheran Church, but never used that spelling.[c]

4) OLIVE JOSEPHINE

b. 1 or 2 December 1868, in Argyle.[a,i,o]

m. Dr. Sven S. Reimstad, of Madelia, Minnesota, 13 December 1898. He died 28 March 1902.[a,i,o]

m. Dr. William Henry Shaver, of Madelia, 25 October 1905.[a,l]

d. 1943 or 44.[q]

5) EVA ROSSING

b. 13 September 1875, in Argyle.[a,f,i,l]

m. Charles Waldron Jerome, 7 August 1907, in Minneapolis.[a]

d. 3 September 1966, in Minneapolis.[a,b]

JOSEPH and PETRA must have lived at Owego Mills until 1870, when they moved to Argyle.[a] At that time James sold the Owego Mills, and with Joseph bought a saw- and grist-mill in Argyle. Shortly thereafter, Joseph purchased a partnership in a drugstore in Argyle, with Dr. C. A. Hansen.[a] James and Joseph sold the mill soon thereafter. About 1874, Joseph rented it back from the buyer and ran it until 1884.[a] From about 1884 to 1886, when he removed to Minneapolis, Joseph was not involved in business.[a] During the early years of the decade, his older son, Amos, attended and graduated from the Augsburg Seminary in Minneapolis, and Fred started there in 1883.[n]

While living in Argyle, both parents were active in community affairs. Joseph was installed as Worthy Master of the Argyle Masonic Lodge in December of 1884.[j] In 1885 he was elected president of the Argonaut Society, a poetry and literary club.[k] Petra helped organize the Argyle Lutheran Church and Ladies' Aid Society.[d]

In Minneapolis, Joseph earned his living in various ways: in the mercantile business, real estate, the Sheriff's Office, and the lumber and wrecking businesses.[a,l]

[Not part of the genealogy, but interesting in its own right, is the fact that both Frederick and his sister Eva graduated from the University of Minnesota as members of Phi Beta Kappa.]

EDNA AGNES MITCHELL

b. 12 April 1878, in Anoka, Minnesota.[l,o]

m. Frederick W. Sardeson, 16 June 1903, in Minneapolis.[l] [1902, according to Eva Jerome,[a] but in error]

d. 6 August 1963, in Seattle, Washington.[l]

The name Mitchell had originally been Michelet [or Michelé]; the family in St. Paul, Minnesota, was descended from French Huguenots who had fled to Sweden. Edna was a Baptist, but she went to the Congregational Church with her husband. Edna Mitchell Sardeson's father was Eduard Mitchell; apparently he moved to Seattle when Edna was a child. Some data concerning him are relevant.

b. 25 August or 25 September 1853.

m. Maryon ("Mary") Johnson, November 1877.

d. 28 September 1939, in Seattle, Washington.

His parents, ? and Anne, were married in 1851 or 1852, in St. Paul, Minnesota, and died on about the same day in 1863, during a cholera epidemic there.

EDNA and FREDERICK had one daughter:

MARION PETRA SARDESON

b. 9 August 1904, in Minneapolis, Minnesota.[1]

m. Carl William Baruth, 9 February 1944, in Minneapolis. He died 13 July 1956, in Seattle, Washington, aged 56.[1,o]

m. George H. Buyken, 14 February 1977, in Seattle. He died 21 September 1988, at age of 91, in Bothell, Washington.[1]

d. 13 April 1998, in Kirkland, Washington.

Marion was named for her mother's mother, "Maryon," who was always called "Mary."[1] Marion graduated from Hamline University in St. Paul in 1926 and was a businesswoman most of her career. She also worked in government service, both federal and Kitsap County and the City of Everett, Washington.[1]

Rising in the business world and wanting to entertain associates, Marion built her own house in St. Louis Park, a suburb of Minneapolis in 1938. Her mother was to live with her and be both companion and housekeeper. Dr. Sardeson objected and said he would not move, but did--complete with fossils--before the ladies moved in! His house on Harvard Street was sold to the University.

When Marion married in 1944, Mr. Baruth joined the household. Marion and her husband tired of Minnesota winters and wanted to move to Seattle to work. Her mother was delighted, for she had been brought up there. The whole family, including a cat, left Minneapolis in the early fall of 1947. Thus, Dr. Sardeson, somewhat reluctantly, spent the last 11 years of his life in Seattle.

SOURCES

a) Sardeson and Rossing families records:
   i) told to Charles Jerome, husband of Joseph's daughter Eva, a "few years" prior to Joseph's death (1914), but not typed by Charles until 1920, 6 pages.
   ii) 4-page genealogical table of the Sardesons, handwritten by Charles Jerome [in the early 1930s, by context] and to which is attached a list of the 12 children of William and Ann Sardeson "copied from family Bible by Ann before leaving England." The list is a transcription made about 1900, and it is not clear whether the Ann referred to is William's third child, Ann, or his wife Ann. Both items are in the C. W. Jerome Family Papers, File P854, Box 2, Minnesota Historical Society, St. Paul.

b) *Minneapolis Tribune*, 4 September 1966.

c) Files of Wiota Lutheran Congregation, South Wayne, Wisconsin.

d) *Argyle Atlas*, 1929; clipping in Jerome Papers (see a).

e) 1914 death notice from a Minneapolis paper; clipping in Jerome Papers (see a).

f) 8 September 1966 death notice from a Minneapolis paper; clipping in Jerome Papers (see a).

g) Story by Mrs. Orville Sardeson in the *Argyle Atlas* (in two successive weeks) of September, 1956. Mrs. Orville was a second cousin of Frederick Sardeson and the granddaughter-in-law of his Uncle James.

h) "Memoirs of the old gristmills in the Pecatonica River Valley: Owego, Argyle, Blanchardville, Moscow, and Puddle Dock." Catherine Barry, 1951, Argyle, Wisconsin, 10 p.

i) Rossing family genealogical notes and tables, given me (1984, 1994) by Prof. Thomas D. Rossing of the Physics Department of Northern Illinois University. His father, Torstein, was a second cousin of Fred Sardeson. Tom recalls meeting Fred when, as a lad, he visited his father's cousin Olive in Madelia, Minnesota.

j) *Argyle Atlas*, 3 December 1884.

k) *Argyle Atlas*, 21 April 1885.

l) Author's interviews with Marion Petra Sardeson, 1984 and subsequently, including letters and telephone conversations. Data supplemented by entries in Who's Who, obituaries from newspapers, and F. W. Sardeson's memorial in *The Proceedings of The Geological Society of America for 1959*, p. 143-146 [1960]. The date of his death in the title of the memorial is incorrect.

m) Lafayette County Historical Society, 1976, The Lafayette County Bicentennial Book. Darlington, Wisconsin, 225 p.

n) F. W. Sardeson letter to W. Charles Bell, of The University of Texas-Austin, 7 October 1956.

o) Numerous notes by Edna Mitchell Sardeson, including some from a 28 September 1914 letter to her from "Aunt Mary Knapp." Mary Knapp does not appear in any other record I have found.

p) Notes by Frederick Sardeson added to those from the letter from Mary Knapp. Most data in both "o" and "p" agree with those already posted to this record from other sources. References to these two sources are made only for additional or different items.

q) In a letter to R. S. Bassler, 10 July 1944, Sardeson wrote that he had lost "two of three sisters in the past year." [Smithsonian Institution Archives, R. S. Bassler Papers, Record Group 7234.]

BIBLIOGRAPHY OF FREDERICK W. SARDESON

All titles are by F. W. S. unless otherwise indicated. Prepared by Malcolm P. Weiss, November, 1997.

Oft-repeated publishers are indicated by initials, as follows:

AJS	American Journal of Science
AG	American Geologist
GSAB	Geological Society of America Bulletin
JG	Journal of Geology
MANSB	Minnesota Academy of Natural Sciences Bulletin
MGS	Minnesota Geological Survey
PAG	Pan-American Geologist
S	Science
UMQB	University of Minnesota Quarterly Bulletin

Citations are given in the order Volume: Number (if known): Pages. The month of publication, if determinable, is also cited.

No.	Year	Title	Published
1.	1892a	Paleozoic formations of southeastern Minnesota. C. W. Hall and F. W. Sardeson.	UMQB 1: 30 (May)
2.	1892b	Paleozoic formations of southeastern Minnesota. C. W. Hall and F. W. Sardeson [Read by CWH at the December, 1891 Geological Society Meeting] [Discussion on p. 464–465] [Precis in Am. Geol. v. 10, p. 182–183.]	GSAB 3: 331–368 (June)

No.	Year	Title	Published
3.*	1892a	Paleozoic fossils in the drift. [Read before MANS 3 February 1891]	MANSB 3: 317–318 (April)
4.*	1892b	Fossils in the St. Peter Sandstone. [Read before MANS 3 February 1891]	MANSB 3: 318–319 (April)
5.*	1892c	The Lower Silurian formations of Wisconsin and Minnesota compared. [Read before MANS 6 October 1891] [cf. No. 7]	MANSB 3: 319–326 (April)
6.*	1892d	The range and distribution of the Lower Silurian fauna of Minnesota with descriptions of some new species. [Read before MANS 8 December 1891] [cf. No. 8]	MANSB 3: 326–343 (April)
7.	1892e	The Lower Silurian formations of Wisconsin and Minnesota compared (Abstract). [Read 6 October 1891 before MANS][cf. No. 5]	UMQB 1: 29 (May)
8.	1892f	The range and distribution of the Lower Silurian fauna of Minnesota with descriptions of some new species (Abstract). [Read 8 December 1891 before the MANS] [cf. No. 6]	UMQB 1: 29 (May)
9.	1892g	The St. Peter Sandstone (Abstract).	UMQB 1: 29–30 (May)
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*These four papers of v. 3, no. 3, were printed, and covered, and about 24 were mailed out 6 April 1892 as "Palaeontological Papers." The U.S. Geological Survey Bibliography gives the date as 1901, perhaps the year v. 3 was completed.

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17.	1896a	The Saint Peter Sandstone.	MANSB 4: 1: 64–88 (February)
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23.	1897b	Review of Gerhard Holm's "On the apical end of Endoceras."	AG 19: 60–62 (January)
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70.	1923b	Portland cement materials in Minnesota.	PAG 39: 2: 121–124 (March)
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73.	1923e	Minnesota eskers and sundry kames.	PAG 40: 2: 95–101 (September)
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78.	1924c	Volcanic ash in Ordovicic rocks of Minnesota.	PAG 42: 1: 45–52 (August)
79.	1924d	Selective teaching in geology.	PAG 42: 4: 263–272 (November)
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82.	1925b	Primitive cephalopods from Minnesota.	PAG 43: 3: 185–204 (April)
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87.	1926c	Beloit Formation and bentonite. [Part I]	PAG 45: 5: 383–392 (June)

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88.	1926d	Beloit Formation and bentonite. [Part 2]	PAG 46: 1: 11–24 (August)
89.	1926e	Four-stage glacial epoch.	PAG 46: 3: 175–188 (October)
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No.	Year	Title	Published
98.	1928a	Derivation of the Calceocrinidae.	PAG 49: 1: 35–46 (February)
99.	1928b	Most primitive of star-fishes.	PAG 49: 1: 58–59 (February)
100.	1928c	Star-fish beginnings and Protopalaeaster.	PAG 49: 2: 99–110 (M.arch)
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102.	1928e	Heliokleptic Earth.	PAG 50: 1: 29–38 (August)
103.	1928f	Bentonite seams in stratigraphic correlation.	PAG 50: 2: 107–116 (September)
104.	1929a	Ordovicic brachiopod habit.	PAG 51: 1: 23–40 (February)
105.	1929b	What are Iowan loess and Iowan till?	PAG 51: 2: 97–108 (March)
106.	1929c	Pleistocene glacial stages in North America.	PAG 51: 3: 193–206 (April)
107.	1929d	Poesy in paleontology.	PAG 51: 4: 281–286 (May)

No.	Year	Title	Published
108.	1929e	Oil from cone-domes in Texas.	PAG 52: 2: 118–124 (September)
109.	1929f	Keweenawan rocks in southern Minnesota.	PAG 52: 5: 355–364 (December)
110.	1930a	Actinoceras in Minnesota.	PAG 53: 2: 91–104 (March)
111.	1930b	Cameroceras and its allies.	PAG 53: 3: 175–182 (April)
112.	1930c	Known glaciations of North America (Abstract).	PAG 53: 4: 315 (May)
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116.	1931b	Accident and variation in Orthoceras.	PAG 55: 4: 257–268 (May)
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118.	1932a	Traditional errors in glaciology.	PAG 57: 3: 186–194 (April)
119.	1932b	Fauna of the Jordan Sandstone.	PAG 58: 2: 103–106 (September)
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121.	1932d	Grantsburg sublobe of Wisconsin gray drift, *in* Frank Leverett, Quaternary geology of Minnesota and parts of adjacent states.	U.S. Geological Survey, Professional Paper: 161: 78–90.
122.	1933a	Glacial diversion of Mississippi River in Minnesota.	PAG 59: 3: 177–189 (April)
123.	1933b	Glacial diversion of Cannon River in Minnesota.	PAG 59: 4: 259–268 (May)
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126.	1934a	Ordovicic bentonite zones.	PAG 61: 1: 19–28 (February)
127.	1934b	Ordovicic complete Gonioceras.	PAG 61: 4: 251–263 (May)

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128.	1934c	Shakopee Formation.	PAG 62: 1: 29–34 (August)
129.	1935a	Patrician glaciation in Minnesota, *in* C. R. Keyes *et al.*, Patrician center of glaciation: A symposium.	PAG: 63: 1: 19–24 (February)
130.	1935b	Glacial Minnesota Man a damsel.	PAG 63: 2: 115–118 (March)
131.	1935c	Behavior of the bryozoan Prasopora simulatrix.	PAG 63: 3: 173–188 (April)
132.	1935d	Behavior of Monticulipora.	PAG 64: 1: 43–54 (August)
133.	1935e	Defense of Shakopee title.	PAG 64: 4: 279–285 (November)
134.	1935f	Behavior of Homotrypa of Decorah shales.	PAG 64: 5: 343–354 (December)
135.	1936a	Behavior of Dekayella of Decorah shales.	PAG 65: 1: 19–30 (February)
136.	1936b	Bryozoan Hallopora behavior.	PAG 65: 2: 97–112 (March)
137.	1936c	Pleistocene Saint Croix River.	PAG 65: 3: 189–208 (April)

No.	Year	Title	Published
138.	1936d	Cambric of upper Mississippi region.	PAG 65: 5: 339–347 (June)
139.	1936e	Early Batostoma behavior and Hemiphragma.	PAG 66: 2: 95–111 (September)
140.	1936f	Early bryozoans: Monotrypa to Eridotrypa.	PAG 66: 3: 179–190 (October)
141.	1936g	Fossil bryozoans; Leptotrypa to Fistulipora.	PAG 66: 4: 251–263 (November)
142.	1936h	Early bryozoans; Batostoma to Fenestella.	PAG 66: 5: 329–346 (December)
143.	1937a	Stromatotrypa to Pachydictya and allies [Part 1].	PAG 67: 1:19–30 (February)
144.	1937b	Stromatotrypa to Pachydictya and allies [Part 2].	PAG 67: 2: 99–107 (March)
145.	1937c	Stictoporella to Arthropora.	PAG 67: 3: 175–191 (April)
146.	1937d	Monticuliporoidea as early bryozoans.	PAG 67: 4: 253–262 (May)
147.	1937e	Glacial outwash and pitted plains in Minnesota.	PAG 67: 5: 325–332 (June)

No.	Year	Title	Published
148.	1937f	Galena Formation limestone in Minnesota, *in* Keyes, C. R., ed., Taxonomy of Galena dolomites of upper Mississippi region: A symposium.	PAG 68: 1: 24–34 (August)
149.	1937g	Evolutionary trends in Ordovicic bryozoans.	PAG 68: 3: 226–230 (October)
150.	1938	Saint Anthony Falls and Minnesota Man.	PAG 69: 2: 92–100 (March)
151.	1939a	Carabocrinus and species-making.	PAG 71: 2: 27–38 (February)
152.	1939b	Old Blue River, and upper Mississippi drainage in Tertic times.	PAG 71: 3: 183–193 (April)
153.	1939c	Early pelecypod Vanuxemia in Minnesota.	PAG 71: 4: 283–293 (May)
154.	1939d	Early pelecypod Cyrtodonta in Minnesota.	PAG 71: 5: 337–346 (June)
155.	1939e	Cambric relations in upper Mississippi province.	PAG 72: 1: 15–28 (August)
156.	1939f	Four glacial stages, or three?	PAG 72: 3: 193–206 (October)
157.	1940	Dolomitization and ore genesis of Galena Limestone.	PAG 73: 3: 193–202 (April)

From 1892 to 1940 Sardeson kept detailed records of reprints (separates) of his own work sent out to others and also of reprints received. The records are in two handwritten volumes. He mailed his works, according to subject matter, to about 220 different persons. Most went to workers in North America, but some went to Australia, Austria, England and Scotland, Germany, Japan, Sweden, and Switzerland.

In that same period he received about 2200 papers from workers both here and abroad. The names in the two lists include nearly everyone who was "somebody" in the geologic profession—except in mineralogy, basement rocks, and economic geology—during the first half of this century.

SOURCES FOR NOTES

This is a list, alphabetically by symbol, of the principal archival and other sources referred to in the notes of the several chapters. The few infrequently used sources are not included in this list, but are identified within each note.

Permission has been given by each of the institutions in the list to quote from materials they hold.

SYMBOL	DEPOSITORY OR ARCHIVE
CKL	Charles K. Leith Papers, University of Wisconsin Archives, Madison.
CWJ	Charles W. Jerome and Family Papers. Minnesota Historical Society. Catalog P 854, Box 2. Jerome was husband of Sardeson's younger sister, Eva.
FBS	Correspondence of Fred B. Snyder, Late President of the Board of Regents of the University of Minnesota. University of Minnesota Archives.
FWS	Personal articles, photographs, memorandum book, and news clippings saved by F. W. Sardeson, and given to me by his daughter.
HEW	Correspondence between Sardeson and Herbert E. Wright, Jr., Professor Emeritus of Geology, University of Minnesota. Materials that Wright copied for me from his files.
JHZ	Correspondence of various geologists with Frank Leverett, copied circa 1966 by the late James H. Zumberge, President [at that time] of Grand Valley State University, from originals then at the University of Michigan. The file includes also a few letters copied by H. E. Wright, Jr., and given to Zumberge in 1966 and copied by him for me in 1984. Zumberge's file is now at the Michigan Historical Society, or such was his intent shortly before his death.
KEC	Correspondence between Sardeson and Kenneth E. Caster, Late Professor of Paleontology, University of Cincinnati. Caster Archive, Department of Geology, University of Cincinnati.

MGS	Minnesota Geological Survey, correspondence and other files in the Archives of the University of Minnesota. Duplicates of some of these items have also been obtained from files of the U. S. Geological Survey at the National Archives (NA).
NA	National Archives, Record Group 57, Entry 81. Correspondence of the U. S. Geological Survey. The relevant file is included as part of the note.
PP	Papers of the Presidents of the University of Minnesota are filed sequentially by president and by date, as Presidential Papers in the University of Minnesota Archives.
RSB	Correspondence between Sardeson and Ray S. Bassler, Late Paleontologist of the U. S. National Museum [of Natural History]. Ray S. Bassler Papers in the Smithsonian Institution Archives, Record Unit 7234, Box 7.
SIA	Smithsonian Institution Archives: used for others of Sardeson's correspondents whose papers are filed there.
UC, TCC	Correspondence and letterpress books of Thomas C. Chamberlin, Late Head of Geology at the University of Chicago and Editor of *The Journal of Geology*. Department of Special Collections, the University of Chicago Library [in the Joseph Regenstein Library].
UIA	University of Iowa Archives, Iowa City.
UOM	University of Minnesota Archives.
WCB	Correspondence between Sardeson and W. C. Bell, Late Professor of Paleontology, University of Minnesota 1946-1953 and of Geology at the University of Texas-Austin, 1953-1978. Gift to the author from the W. C. Bell Estate, 1979. The gift included letters and other materials.
WSC	William S. Cooper, Late Professor of Botany at the University of Minnesota. Cooper gave his file of correspondence on the glacial geology of Minnesota to Prof. H. E. Wright, Jr. in 1966; Wright provided me with copies of items relevant to Sardeson. These and other papers of Cooper's are now in the University of Minnesota Archives.

YMA Correspondence of Charles Schuchert, Late Professor of Paleontology, Yale University, from the Charles Schuchert Papers (MSS 435). Manuscripts and Archives, Yale University Library.