FREDERICK WILLIAM SARDESON

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INTRODUCTION

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

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

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

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

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

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

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

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

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

TEACHING AT THE UNIVERSITY OF MINNESOTA

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

Sardeson continued as "scholar," with some increase in pay, until June of 1898, when he was appointed instructor in the department with a salary of $500. What teaching he may have done for Hall between November of 1895 and June of 1898 is not clear from the record. Because Hall always put Sardeson forward, it is likely that the latter did some teaching in addition to his own research and caring for the collections. He was promoted to assistant professor in July of 1905, the grade at which he remained until leaving the university. At that time a common

academic table of organization had a professor at the top, and several lesser types to fill out the pyramid and do much of the work (the rank of associate professor was not known). The regents of the university had little interest in paleontology because they considered that the effort and money the Winchell Survey (a part of the university) had spent on fossils was largely wasted. Despite that bias, Sardeson received several raises in pay while in the department. His rank of assistant professor was honorable and satisfactory for one with a Ph.D. degree.

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

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

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

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

Sardeson was himself very proud of his teaching, and said that the president once recognized the excellence of his teaching. Over many later years he needled the university administration about the dete-

rioration of its geology program. He scorned especially the bachelors of geology who were "trained" to be oil geologists, and the General College that the University of Minnesota developed in the 1920s for students with weak preparation and no clear goals.

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

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

DISMISSAL FROM THE UNIVERSITY

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

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

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

Emmons got his chance to act when the dean invited him in early April of 1913 to name any persons in the department who "should be discontinued." Emmons named Sardeson and one other, and the proposal started its way through channels. The dean hoped for action by the Board of Regents that spring, but the president didn't respond until mid-June. The slowness of the process was unfortunate, but it is clear from the record that the dean and his advisory committee joined Emmons in wishing Sardeson to be gone. Realizing that it was a serious matter to fire an

established professor with a good record of teaching and research, President Vincent called a meeting with Sardeson, the dean, and Emmons on Saturday, July 5th. There is no record of what transpired at that meeting, but Vincent decided to call another for Sunday evening with the whole geology faculty and the dean.

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

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

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

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

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

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

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

projects. The release from censorship in 1922 came through C.R. Keyes' *Pan-American Geologist*.

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

PALEONTOLOGY

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

Collecting and collections

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

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

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

Taxonomy

There are two ways to look at Sardeson and taxonomy: the principles that he urged and used in the

naming of species and assignments to genera, and the new names that he gave to fossils. The first is important because of the modernity of his policies; the second is of less interest because—compared to his contemporaries—he did not create a lot of names.

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

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

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

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

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

(Sardeson, 1933)

Sardeson himself named species of sponges, brachiopods, gastropods, bivalves, cephalopods, and crinoids. Many of the molluscs were from the St. Peter sandstone, from which few specimens have been discovered since. As these groups have yet to be revised in monographs, it is difficult to say whether Sardeson was "right" or not. Some species have survived; others have fallen into obscurity. Probably most would not be named today. Sardeson had better "luck" with the brachiopods, despite the fact that a number of his names were either suppressed by a phony publica-

tion (prepared by Ulrich for Winchell and Schuchert) prior to release of his own names or put into synonymy soon after that (Weiss, 1997). Sardeson did distinguished work on cephalopods and crinoids, but that had little to do with nomenclature. He worked also on corals and intensively on bryozoans, but never named species of either. Except for his bryozoan lineages and his work on the synonymy of several groups, Sardeson's taxonomic work was neither abundant nor remarkable.

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

Paleobiology

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

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

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

STRATIGRAPHY

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

Sardeson's most important contribution was the early rationalization of the stratigraphy of the Middle Ordovician beds of Minnesota, Wisconsin, and Iowa. Geologic work in that region began in the mid-1800s and gained intensity in the late 1800s. In those years some rock units were given names from distant regions—New York, Ohio, Tennessee—but without direct comparisons of rocks or fossils having been carried out. "Trenton," for instance, meant different things to different workers in different states and at different times. If a Trenton fauna was recognized ac-

curately in both Minnesota and Wisconsin, but in different rocks, what should be recognized as "Trenton" in Iowa? Work and maps were provincial rather than regional, simply because more years were required to develop a thorough regional view.

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

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

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

Sedimentary petrology

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

A number of peculiar surfaces or thin zones that are widespread in the Middle Ordovician carbonate rocks of the eastern United States and in Balto–Scandia show an interlocking (dovetailed) profile. The lower beds show evidence of solution and burrowing and are often coated with metal sulfides, and cavities in them contain material derived from the upper bed. The upper bed may contain chunks of the crust of the lower bed, as sand- or pebble-sized clasts. Sardeson recognized that submarine solution had been going on and named these features "corrosion zones." When he

discovered that some had chunks of the lower rock floating in the upper bed he suggested "corrosion conglomerates." He quickly realized that such surfaces were time lines and demonstrated their utility in correlation over the Upper Mississippi Valley. Later work has improved our understanding of them and shown abrasion to be important locally on some surfaces. Thus they are preferably called discontinuity surfaces today.

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

GLACIAL DEPOSITS AND PLEISTOCENE HISTORY

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

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

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

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

Sardeson was involved in three mapping projects for the folios that the USGS published in those days; one was a bust, but the other two were fine pieces of

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

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

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

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

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

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

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

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

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

THE INDEPENDENT GEOLOGIST

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

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

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

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

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

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

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

SUMMARY

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

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

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

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