SEM History

THE EARLY DAYS OF OUR SOCIETY, *Experimental Techniques* Nov/Dec 2002, Vol. 26, No. 6

Forward:
For SEM History articles we try to find authors who knew personally the people or events about which they write. The author, J. Hans Meier, not only was present when the first strain gages were made, he actually mounted and tested them. No one is better qualified to write about strain gages or early SESA history than is Hans. That is why we are absolutely delighted to receive the following article.

Hans himself was the subject of a SEM History article, "An Appreciation of J. H. (Hans) Meier SEM (SESA) President 1950-1951" by Robert Plunkett. (See Experimental Techniques, Nov/Dec, 1998, p. 34.) Hans is an active and much appreciated member of SEM, and currently is the senior Past President and senior Honorary Member of the Society.
CET

THE EARLY DAYS OF OUR SOCIETY

By J. Hans Meier

By a fortunate series of events I became witness and was privileged to contribute a little to the formation of the forerunner of our society.

In the fall of 1937 I, a new immigrant and the proverbial greenhorn, entered the graduate school of MIT. There I was welcomed by several friendly and helpful people. One was Dr. William M. Murray, who accepted me into his course on photoelasticity. Another was Professor Arthur C. Ruge, whose course on Vibrations was recommended by a friend from my native Switzerland.

Dr. Murray invited his students to attend the semi-annual meeting of the Eastern Photoelasticity Conference. The conference was comprised of some very knowledgeable academics in the field, notably Dr. Max M. Frocht, whose excellent book made him the father of photoelasticity, Professor Mindlin of Columbia, Dr. Hetenyi of Westinghouse and Professor Dolan of the University of Illinois. I felt that most of the papers presented were far over my head, and my master's thesis on photoelasticity was much more on the beginner's level.

Professor Ruge's course on vibrations was complemented by the hydraulic shake table he had constructed to simulate earthquakes. He had received funds from insurance companies because the elevated water tanks had collapsed in the San Francisco earthquake of 1906. The lack of water resulted in fires that caused enormous damage.

They were seeking means to make water tanks earthquake-proof. Professor Ruge invited me to pursue this project for my doctor's dissertation.

We built a model of the traditional water tank and placed it on the shake table, but were confronted with the problem of measuring the stresses. One night Professor Ruge had what he called the Eureka solution: glue a piece of cigarette paper on the tank and glue a small wire with end connections to the paper. Preliminary experiments with a one mil wire glued to a piece of plastic were encouraging and I began investigating small wires various composition for their strain sensitivity. Among others we tried Elinvar, Advance and nickel. We glued "strain gages" and even some strain rosettes of Advance wire to the tank, but at that time amplifiers did not yield sufficient output for our small gage lengths and my dissertation lacked measured data from the tank model. However, it included a theoretical analysis.

A few months before my graduation, Mr. Knox, vice president of Bucyrus-Erie Company came to MIT looking for somebody who could measure forces in excavating machines. The placement officer introduced Mr. Knox to Professor Ruge, who felt that I would meet Bucyrus-Erie's requirements and I was hired.

My first job at Bucyrus was to design and build an apparatus that could simultaneously record two stress and ten timing events. Various commercial equipment was used, such as a Dumont 208 Oscillograph, a Hewlett-Packard wave generator and an old-fashioned professional movie Camera.

A motor was mounted on the movie camera so that the continuously moving film would serve as time axis. A portable generator furnished the power and the entire rig was complicated and cumbersome by today's standards. But it worked and yielded useful results.

Meanwhile I kept in touch with Professor Ruge, who had started a strain-gage business together with Professor Deforest, and with Professor Murray, who was well aware of the new strain gages. Professor Murray felt that because of the war the Eastern Photoelasticity Conference should expand its field to include strain gages. He invited me to present a paper to the Conference in the fall of 1942. My entire apparatus, minus the generator, was shown and operated. Instead of an excavating machine, a one-inch steel bar with strain-gages attached was used for the demonstration.

Dr. Murray invited Dr. Arthur Anderson, whom he knew at MIT, and others to present papers to the Eastern Photoelasticity Conference in Detroit in April 1943. The Conference subsequently changed its name to the Society for Experimental Stress Analysis (called SESA) and opened it to workers in all fields of stress analysis. Professor Murray became the first President and Secretary and worked very hard towards the success of SESA. Notably, he untiringly edited the Proceedings which came out in bound-book form.

Various committees were formed and I served on the Papers Committee that was to make certain that the papers were on a suitable subject and functionally correct. Many good ideas, but also some bizarre were submitted. One prospective author drew an analogy between a rotating shaft going though the critical speed and a column subjected to buckling. His conclusion: when the bar is held steady until the gradually increasing compressive force exceeds Euler's load, the bar can be let loose and buckling does not occur.

The meetings of the new Society were lively and led to interesting discussions. Repeatedly Professor Frocht brought up the point that strain gages had a finite gage length and therefore cannot yield the maximum stress in a sharp notch, whereas photoelasticity can.

Two members of the Society were particularly noteworthy. One was Frank Tatnall, a master storyteller. He was a strain-gage salesman and told embellished stories of the invention of the strain gage to an amused audience. And the more he told them, the more embellished the stories became. In the end he told that he was very instrumental in the creation of the gage and came to believe the tale himself!

The other was Bill Bean, a master showman. He was a consultant to industry on dynamic strain measurement. As a showpiece he used an arrow with gages attached and trailing cables to his instruments. An eight-foot bow shot the arrow at a five-foot target ten feet away and invariably hit it. Then he proudly showed the impact-stress history recorded by his instruments.

Early on an Executive Committee was formed to guide the Society, but Professor Murray still carried the major burden for many years. Eventually, a secretary had to be hired and an office was rented. I joined the Executive Committee and was elected President for the year 1950-51.

1968 marked the twenty-fifth anniversary of the Society. The secretary arranged for a get-together of the early stalwarts. It was a most meaningful, almost stirring, gathering because we knew it was for the last time that we all would come together.