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A Conversation with Donald Marquardt

Gerald J. Hahn

Abstract. Donald W. Marquardt was born in New York, New York on March 13, 1929. Marquardt received a Bachelors degree in physics and mathematics from Columbia University in 1950 and a Masters degree in mathematics and statistics from the University of Delaware in 1956. From 1953 to 1991 he was employed by the DuPont Company starting as a research engineer/mathematician and ultimately organizing and managing the corporate quality management and technology center. He is now President of Donald W. Marquardt and Associates. In 1986 he was President of the American Statistical Association (ASA). He has received various awards, including the Shewhart Medal from the American Society for Quality Control (ASQC) in 1986 and the Meritorious Service Award from the American National Standards Institute in 1992. Marquardt was elected a fellow of the ASA in 1975, a fellow of the American Association for the Advancement of Science in 1983, a fellow of the ASQC in 1986 and a member of the International Statistical Institute in 1989. Two of his publications have been named Citation Classics by the Institute for Scientific Information. As Chairman of the U.S. Technical Advisory Group to ISO/TC176, he is the Leader of the U.S. delegation to the international committee that prepares the ISO 9000 standards on quality management and quality assurance. Among several leadership roles in this international committee, he is convener of the Strategic Planning Advisory Group. He has held adjunct professorships at several universities and has been Associate Editor of three statistics research journals.

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THE THEORY OF KNOWLEDGE, STATISTICS AND COMPUTERS— A WINNING COMBINATION

Gerry Hahn: It is, indeed, a great pleasure to talk with you this afternoon and to have the opportunity to review your many accomplishments and activities. Perhaps a good starting point is to ask how you got interested in statistics in the first place.

Don Marquardt: My entry into the field of statistics was, as is so often the case, more or less accidental. When I was at Columbia University, I was focusing my undergraduate work on physics and mathematics, and took many courses in the physics department. The Second World War had just finished. On the faculty and in the group of graduate students in the physics department at Columbia there were several people who were either then or were later to become Nobel Laureates. I, of course, had a lot of laboratory work at the physics department and from that I got interested in statistics. In retrospect, I realize that I first learned the notion of one variable at a time experimentation there.

Hahn: Picked up some bad habits I surmise.

Marquardt: Yes, from today's perspective it seems like a bad habit, but it was state-of-the-art in the physical sciences at that time. For me the importance was that I suddenly realized that a deliberate strategy of experimentation is possible.

I looked around the university for a course in statistics. I did find one in the School of Business, and sat in on the first two lectures, but it was at such a cookbook level that I decided that I really didn't want to continue. Perhaps I didn't under-

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stand some of the realities of statistics, but, nevertheless, I dropped out and instead took a course on philosophy of science with Professor Ernest Nagel. Professor Nagel turned out to be one of the most influential of my teachers at Columbia. I took several courses from him just in order to continue to benefit from his wisdom. I learned a lot about the philosophy of science and epistemology—the theory of knowledge—in those courses. They were very influential in my philosophic thinking in future years.

Later on, in the U.S. Army during the Korean Conflict, I was in the Chemical Corps at Camp Detrick in Frederick, Maryland. On the basis of my physics and mathematics work, I was assigned to the statistics branch. This seemed to me a strange turn of events, but it was an experience with much learning and accomplishment. The work was related to biological research. We did a lot of machine processing of experimental data. In most cases it was designed data, but sometimes just nondesigned, accumulated data. The data were analyzed with mechanical punch-card machines that were then state-of-the-art for commercial data processing. Toward the end of my stay we obtained some of the very earliest electronic computing equipment. We also used a lot of electrical desktop calculators. We did small multiple regressions, but mostly we did analysis of variance.

The statistics branch had some very good people, and, in retrospect, I am impressed with the planned experiments that they designed and carefully analyzed in quite a production mode. There were tests for curvature of response curves and careful analysis of variance tests. Dr. Clifford Maloney was the head of the group. Not many of the people were well known, but the work was very good. As a result of my work at Camp Detrick, which later became known as Fort Detrick, I became convinced that this new field of statistics was important and challenging and would be closely tied to the new computers with which I had a little bit of experience while I was there. That looked to me like a winning combination, so I decided to stake my career in that direction.

Hahn: I find that especially interesting because actually my career was somewhat similar. I went to Columbia for a while in the early 1950s and then also was drafted during the Korean Conflict into the U.S. Army Chemical Corps at the Dugway Proving Grounds in Utah, and that's where I really learned what statistics is all about.

Marquardt: Yes, and some of the experiments we analyzed at Camp Detrick were at least in part conducted at Dugway.

There was another interesting coincidence also. Among the other army personnel who were stationed at Camp Detrick at that time was Arnold Zellner. He was in the physics branch, which was right next to the statistics branch, though neither of us ever saw the environment in which the other worked. We became close friends and that has continued through the subsequent years. Of course, neither of us then had any notion that we would both become Presidents of the American Statistical Association.

Hahn: The U.S. Army Chemical Corps served as a good breeding ground for ASA Presidents. Maybe if we look we can find a few more!

Marquardt: With respect to my experience in trying the statistics course at the Business School of Columbia, it wasn't until a number of years later that I learned that during the very period that I was at Columbia, 1946 through 1949, one of the first statistics departments in the United States was formed there. Presumably there were some statisticians under whom I could have taken instruction, but I was not aware of that fact until quite a few years later.

THE DUPONT YEARS—THE GROUP THAT SET AN EXAMPLE FOR INDUSTRY

Hahn: Did you join DuPont right after the Army?

Marquardt: Yes. When I was about to be released from Camp Detrick, there were 30 or 40 of us coming out at the same time, so we got together and advertised that fact in a number of the technical trade journals and other places. A number of large companies who were recruiting technical personnel sent recruiters to Camp Detrick to interview us. DuPont was one of the companies. I was hired, I think, on the strength of my practical experience with statistics and computers. That was, of course, extremely limited in the theoretical sense, because I had no formal statistics training, but my real life experience in those combined fields was a rare commodity in those days.

Hahn: Yes indeed!

Marquardt: DuPont was one of the first industrial companies to get digital computers. The first one was an IBM card-programmed calculator. When it arrived a month or two after I did, I was immediately put in charge of it. Shortly thereafter DuPont acquired a Univac I computer as well. You probably know that General Electric also installed a Univac I computer at just about the same time. Those were the days when one worked strictly in machine language. The first compilers were not yet developed.

FIG. 1. *Donald Marquardt, shown with several units of the room full of equipment that comprised the IBM card-programmed calculator at the DuPont Engineering Research Laboratory. This computer was installed early in 1953; the photo was taken in January 1957.*

Fortran was off in the future also. So we were working in virgin territory.

Hahn: Did you program the IBM computer by plugging different wires into different holes in a wiring board, as they did in those days?

Marquardt: I absolutely did, and one of my big triumphs was that I was able to get an increase in machine speed from $2\frac{1}{2}$ floating point operations per second to over $3\frac{1}{2}$! I was a member at that time of the Engineering Research Laboratory at DuPont, which had a large group of chemical engineers and physicists and metallurgists. Many of these researchers had national or even international reputations. It was exciting to apply these new computers, along with numerical analysis and statistical concepts, to solving differential equations for chemical kinetics and heat transfer problems. We also did pioneering studies using computers for nonlinear algebraic models for chemical equilibrium and related subjects and molecular structure studies on various experimental data. The rate of learning itself was very exciting in those days.

Hahn: At this point you really had not taken very much in terms of formal statistics courses, and you were pretty much self-taught.

Marquardt: That's correct. However, during that period I did graduate study evenings and Saturdays at the University of Delaware, receiving a Masters in math and statistics in 1956. I also spent long hours in the libraries, including a very complete library at the DuPont Experimental Station. They had quite a good collection of the very few books that existed in statistics and numerical analysis then, but there wasn't a lot to go on.

Hahn: Of course, one of your many accomplishments has been the leadership of the statistical group at DuPont. Please tell us how the group evolved because it set an example for many of us in industry.

Marquardt: Well, I told you how I got into DuPont and that I was there at the Engineering Research Laboratory. There already were a couple of mathematical-type people at the laboratory, notably Arthur Hoerl, with whom I did several studies early on. However, I worked pretty much independently with one or several support personnel during most of the first 10 years that I was with the company, doing consulting in the combined fields of math and numerical analysis and statistics throughout the research community of DuPont. This community tended to be focused in a campus-like arrangement called the Experimental Station. There were several thousand researchers in a wide range of subject-matter fields.

During that same period another group of mathematicians and statisticians was put together elsewhere in the Engineering Department at DuPont. Dr. Robert Kennard was a key player in that organization. During that period I had initiated and been funded to do a study of what the long range approach for mathematics in DuPont might be. In part because of that study, and in part because of other administrative changes that were being made in DuPont, I was invited to become the supervisor of a statistics consulting group; it was about half a dozen consultants. I reported to Bob Kennard.

Hahn: Timewise, when was this?

Marquardt: This would have been in early 1964. I continued to manage that group and its successor groups until the end of my career at DuPont—39 years after I had joined the company. The group grew steadily over that whole period.

Hahn: How large was it?

Marquardt: Well, it's sometimes hard to know just how to count the various people. We had a number of part-time people and we had various support personnel. We had computer personnel and others. If you counted everybody at the peak, it would have been in the 90s; and if you counted full-time equivalents, it might have been around 65 or 70. For most of those years we had at least two dozen Ph.D.s.

Hahn: What were some of the challenges you had early on when you set up the group? How did things work then? It was apparently a pretty novel idea then—a statistics group in industry.

Marquardt: Yes, it was. There were many challenges, but also a number of preexisting things that were helpful. Before I joined DuPont, a consulting division of the engineering department, called the Engineering Service Division, had been set up. In that division there were groups of experts in fields like chemical engineering, mechanical engineering and metallurgy who provided consulting services to the entire company. Much of the mechanics of setting up an organization for purposes of internal corporate consulting were already in place before I moved into that organization.

However, in a subject-matter sense, consulting in the field of statistics had not really been developed into an organizational approach. Among the things that we had to concern ourselves with was "How do you sell the idea of statistical consulting to organizations and people who don't know what statistics is all about and don't know what it might contribute?"

Hahn: That hasn't changed too much.

Marquardt: That hasn't changed in some ways, but it has changed dramatically in other ways if you look back over the years.

There was very little in the way of well established statistical methodology applicable to industry at that time. Both experimental design and analysis of variance were still heavily focused toward agricultural field trial environments. The whole concept of response surfaces and response surface methodology was not yet in place. The use of computers was just at the very beginning. For example, people were still learning how to invert matrices by computer.

Hahn: I remember the "good old days" of using orthogonal polynomials to do regression, using electric desktop calculators like those made by Friden, Marchant and Monroe—names I haven't heard too much of late!

Marquardt: Yes, I used orthogonal polynomials in my work at Detrick and carried that work over to some of the early studies at DuPont. Calculating with the early computers was extremely limited. For example, the first version of the card-programmed calculator that we got had a total of 23 words of memory available for variable storage, so one had to be very parsimonious in what one did. Nevertheless, we solved some extremely important problems. For example, in collaborative work with Dr. Bernard Coleman of the Textile Fibers Department we solved numerically an asymptotic probability model in the form of an unstable differential equation; results that even today are considered fundamental [Coleman and Marquardt (1957, 1958)]. We learned as we went: for example, the importance of having a numerically stable computing algorithm such as the Runge–Kutta method for ordinary differential equations, the use of variable interval size and the importance of extrapolating to zero interval size, based on the pioneering numerical analysis studies of J. W. Carr (1958) to converge to the true solution.

Hahn: And even did so without computer-intensive methods!

Marquardt: Well, we thought they were computer-intensive at the time. One of the numerical cases that we reported in the 1958 paper required more than five hours continuous calculation on the Univac I computer.

Hahn: Turning back to DuPont, could I ask you to comment on how the statistical group evolved over time, and how things changed over the many years that you were at the helm of that group?

Marquardt: One of the first things we had to do was to decide how we were going to deploy our people among the various businesses and the more than 200 geographical sites of the DuPont Company. What we decided was that it was appropriate to provide long-term assignments of statisticians to consult at specific sites or with specific businesses. They would have total responsibility, in their territories, for recognizing the problems that needed to be solved and getting to know the people and the products that were involved. The consultants were expected to understand the business issues as well as the technical problems. They would be concerned, long-term team members, and their results would have extra value because of working on a succession of problems. This also meant that a consultant would have to satisfy repeatedly the clients in that organization. This created a built-in discipline in a consultant's work. Another major factor in the success of this approach was the practice of charging the full cost, with all the overhead allocations, to the business for which the consulting was done.

Hahn: The concept of full-cost charging for internal consulting is an idea that is much in vogue today, but might have been a little ahead of its time.

Marquardt: I think its value was just as high then. I believe, in fact, that the benefit of that approach is even stronger for the consultant than it is for the client because it is a very powerful feedback mechanism to determine what is seen as valuable by the client and what is not. Full-cost charging ultimately prevents the consultant from becoming excessively involved in a technical issue that is interesting from a statistical perspective but doesn't contribute very much to the results the clients are interested in.

Hahn: I absolutely agree! How, however, would you respond to the criticism that a downside of such an arrangement is that it tends to emphasize short-range payoffs, rather than focusing on longer-range goals, as may be the case for a group whose costs are supported on corporate overhead?

Marquardt: Well, I think focus on longer-range goals is a responsibility of the management of the statistical consulting organization. For example, often I was able to see that at various sites we were encountering classes of similar problems. Often, different consultants would take different approaches. Thus, we could combine the best features of these approaches into something that would be better for use at multiple sites. We periodically brought the statistical consultants together and planned both the technical and organizational methods to interact with the businesses as a team, to institutionalize our activities to identify and develop longer-range approaches to problems that we encountered repeatedly, either corporately or within a business unit. It is imperative that the consultant organization be tuned in to the business strategy at the corporate level, and to maintain a dialog with senior management to identify the places where statistics can contribute most to business goals and develop programs to capitalize on the opportunities. In managing our consultants we emphasized this role for the consultants as well as the managers. In fact we made it a theme to recognize the broad range of problems that our different businesses were seeing. Then, we would make proposals to the client management for studies that we could do—if they would fund us for some development work—so that we could solve generally a class of problems with a new approach.

Hahn: So even in those days you were concerned with proactive contributions, as opposed to purely reactive ones and just fighting fires.

Marquardt: Oh, absolutely. And another factor of being in the DuPont environment that helped was that we did have a modest level of funding from corporate sources for our development efforts. Our development funding was from a combination of sources—some from our clients and some from the company as a whole.

Hahn: So, in that fashion it seems you were able to combine the advantages of having a corporate group that shared a common discipline and had the ability to hire top-notch people, with on-site competence and keen understanding of local problems and concerns.

Marquardt: As the group developed, we created regional subgroups in various places around the United States and Europe where we had concentrations of DuPont plants and sites, and assigned statisticians to these regional offices. It was easier for them to stay in close touch with their clients. At the same time, however, the career development concerns and personal and professional advancement responsibilities were under the direction of the central group.

We found this to be a good mechanism for getting the benefits of having a central group together that could grow within the discipline of statistics and at the same time make the consultants readily available to the clients.

Hahn: And the record speaks for itself. Some of the best known people in industrial statistics have come out of your group at DuPont.

Marquardt: We've been very fortunate in that regard.

NONLINEAR ESTIMATION

Hahn: Let's now talk about some of your many technical contributions. I, and many other statisticians, heard of you first in association with the method for nonlinear estimation, which became widely known as the Marquardt algorithm—certainly a major development of its time. Could you tell us about how you came to this?

Marquardt: Well, I had to deal with nonlinear problems from the very earliest days of my DuPont career because all the chemical engineering, physics and chemical structure research problems that I worked on, even from the beginning, were inherently nonlinear in their parameters. My clients had very little interest in the purely empirical linear models that one could read about in the statistical literature.

With such a driving force in my client community and plenty of good problems to work on, we made rapid progress in getting answers to our clients' problems, and on research in nonlinear estimation methodology itself. Fortunately, in the first problems we worked on with the very early computers there were only two or three parameters. The data sets also were small, perhaps a dozen or two dozen data points. The computers were so slow that one could watch in excruciating detail every little step and nuance in the behavior of the calculations. For example in doing the work that led to my 1959 paper [Marquardt (1959)], using a Van Laar model for vapor–liquid equilibrium data, I constructed a two-dimensional plot on graph paper. My client gave me the first guess as to what the Van Laar parameters a and b might be for a particular set of data. Then knowing some of the chemistry and thermodynamics I did some empirical grid searching, picking places on the graph paper to calculate the sum of squares of residuals and then drawing by hand the contours of constant sums of squares.

In going through many exercises of that type with problems of that sort I discovered that invariably the sum-of-squares surface is not elliptical, as it is in the case of linear models, but is a long curving trough. Not only that, but often the minimum of a trough was right in a bend. The elongated shape of the trough implied high correlations among the model parameter estimates. It became pretty clear that the simple procedure of steepest descent would take you immediately across the bottom of the trough, wherever you happened to be, and would zigzag back and forth across this bottom with only a very slow progression along the trough toward the lowest point, which might be a long distance away. The method of steepest descent was not very effective even though its initial progress was very fast.

On the other hand, you could expand the model in a Taylor series and assume that the contours were elliptical and predict where the bottom of the elliptical trough was, based on the local information. We found that invariably the nonlinear trough would bend away and go in some other direction far short of the vector distance to the calculated minimum of the elliptical surface. From those geometric insights, obtained through absolute necessity working with highly primitive equipment, came the most important algebraic understanding leading ultimately to the theory that led to the algorithm.

Hahn: And your results, as I recall, were published in a nonstatistical journal!

Marquardt: Yes, it was the *Journal of the Society for Industrial and Applied Mathematics* [Marquardt (1963)].

Hahn: In 1979, the paper was selected as a classic as a consequence of the number of citations in the literature. You made the algorithm available also as a Fortran program. That was a significant contribution to those of us working out in the trenches.

Marquardt: The paper was heavily cited in the succeeding years and I did give talks about it at many different places ranging from university departments to government laboratories and industrial companies. Of course, not everybody used our Fortran code [Marquardt (1966)]. Many workers programmed the algorithm directly from the 1963 paper. In the paper an important small feature of the algorithm was described in a lengthy footnote. This feature monitors at each stage of the calculations, the angle between the steepest descent vector and the algorithm's candidate correction vector to improve the parameter estimates. The feature prevents that angle from becoming too small, which would cause steepest-descent-like behavior. That is, there would be fast early progress, but slow progress in later iterations. Unfortunately, some of the workers who programmed directly from the paper ignored the footnote. In the 1979 Citation Classic write-up I emphasized the importance of that footnote [Marquardt (1979a)]. One lesson I learned was never to use footnotes!

In my research, one of the really important practical criteria in developing the algorithm was robustness in getting to the minimum from a variety of initial guesses, even though the minimum will typically lie at the bottom of a long, curving trough in parameter space.

Hahn: So you were concerned with robustness before it became in vogue. How do you feel nonlinear estimation has evolved since and what do you think are some of the key challenges and issues today?

Marquardt: Well, today there are a number of researchers who specialize in nonlinear problems and there are many methods for design, estimation and inference for nonlinear situations. Like many fields, the initial work in nonlinear estimation was done mostly by newcomers to the field of statistics and by people in other subject matter fields. Today, despite all the progress that has been made, that is still largely true, and nonlinear estimation, although it is big business today, is not routinely taught to those who will become practicing statisticians. Work in statistical theory in general, I think, is still disproportionately focused on linear models, so I think one of the big issues for the field of statistics is to find ways of getting nonlinear statistical methodology more widely taught and practiced.

MULTICOLLINEARITY: RIDGE AND GENERALIZED INVERSE REGRESSION

Hahn: Another area to which you have contributed significantly has been generalized inverse regression, and ridge regression. As a matter of fact, I understand that you played a role in the early development of ridge regression with Hoerl and Kennard, whom you already mentioned as associates at DuPont. So you were involved in biased estimation right from the start.

Marquardt: Yes. As we've discussed, my early research had been in nonlinear estimation, in which I developed an algorithm whose key feature is the adding of a constant to the diagonal of the linearized least-squares matrix at each iteration. Naturally, I was keenly interested when Art Hoerl, who was then in another part of the engineering department, and with whom I had contact only two or three times a year, told me of work he was doing in data analysis by linear regression where he was adding a constant to the diagonal. He called it ridge regression, and was getting interesting results. In the following years, Bob Kennard teamed up with Art Hoerl to develop the theory of ridge regression which ultimately appeared in their joint *Technometrics* [Hoerl and Kennard (1970a, b)] papers. I was privileged to read a long memorandum which Kennard produced in which the essential content of their later *Technometrics* articles was presented, including the theoretical framework. I was struck by the similarity of their mathematics to the math that I had used in my algorithm for nonlinear estimation.

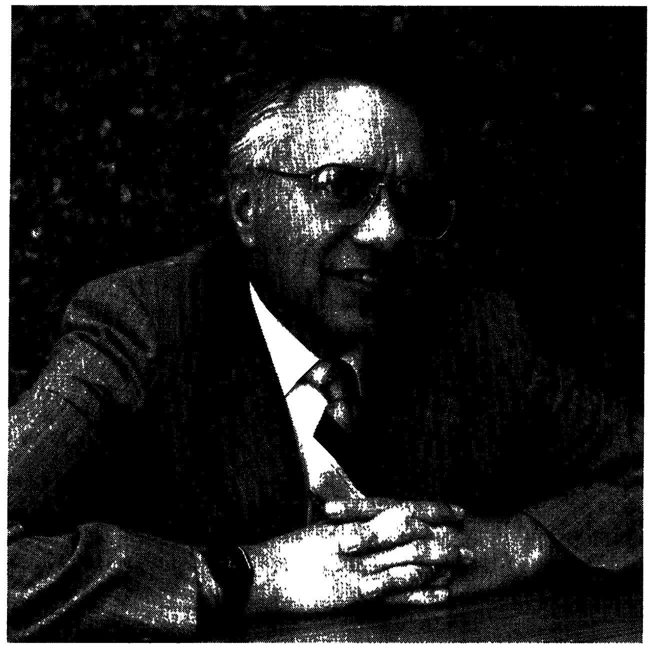

FIG. 2. *Donald Marquardt in 1986 in a photo for a DuPont publication when he was named a Fellow of the American Society for Quality Control.*

Their work, of course, involved a totally different sort of application. I was adding to the diagonal of the matrix as a mechanism to enhance the speed of convergence of the algorithm to a least-squares solution of a nonlinear problem. They were adding to the diagonal of the matrix in order to move away from the least-squares solution of the linear problem to another solution which was better in a mean-square sense. There were critical differences in the motivation and the end result, but the mechanics of the two methods has a great deal in common, and so it was natural that I should be very curious about the relationship between these two approaches. At the same time, in other studies on some subject matter problems I had used generalized inverses in numerical analysis situations as a means for handling cases where matrices are deficient in rank.

Ridge regression in the multicollinear situation was clearly a case where the data were deficient in rank, compared to the size of the matrix that you were working with. The deficiency was not a nice clean break between exactly zero eigenvalues and nice large eigenvalues. Instead, when we calculated the eigenvalues we observed a spectrum of values. Some were zero to as many figures as we could compute; others were just a little bigger and some clearly larger than zero. The primitive notion of a generalized inverse of a certain rank is not quite appropriate for such cases. At the same time, people were using principal components regression as another method for getting around the deficient rank of multicollinear problems. I set out to explore the interrelationships of all these various linear and nonlinear methodologies and, in particular, to explore the use of generalized inverses. I had used generalized inverses in some work in discriminant analysis which I never published, but that work had been very fruitful in some applications at DuPont.

Hahn: Multidimensional discriminant analysis?

Marquardt: Yes. Sure. I began to explore the various theorems that Bob Kennard had developed around ridge regression and to consider what kinds of relationships to the generalized inverses might exist. Over a period of time I wound up stating and proving a series of theorems. This parallel series of theorems demonstrated the very close parallelism between the characteristics of generalized inverse regression and those of ridge regression. One or two theorems added to the Kennard and Hoerl theory in the ridge regression context. I found myself with a paper that was essentially written reporting this parallelism. Kennard and Hoerl had not yet submitted their paper for publication. I wanted to wait until theirs appeared before publishing mine. So I put considerable pressure on Bob Kennard to get their paper submitted and published. As a result, their paper probably was published at least a year earlier than it might otherwise have been—and my paper appeared just shortly after that [Marquardt (1970)].

Hahn: That is very interesting indeed. Would you like to comment on how this whole field has since evolved, and exploded in so many different directions, and where we stand today?

Marquardt: Well, it's very remarkable how resistant to change the theoreticians in any technical field tend to be. I think it's now universally understood that least-squares estimates are extremely unstable, that is, they have extremely large variance whenever there is strong multicollinearity in the predictor variables. Yet many theoreticians still stubbornly hang on to the notion that the unbiasedness of least squares in a linear unconstrained model is somehow sacred. Things are changing gradually. In the *Technometrics* paper on generalized inverses [Marquardt (1970)] I defined quantities called variance inflation factors. The value of what variance inflation factors tell us about the quality of the data in relation to the model being used is becoming more widely appreciated. Variance inflation factors have turned out to be a very useful, practical, tool for displaying the degree of multicollinearity. They have become used relatively routinely even among those who are still resistant to biased estimators, and the notion of biased regression—the willingness to allow a little numerical bias in return for a dramatic decrease in the variance of the parameter estimates and of the predictions—is becoming accepted.

The practical inadmissibility of least squares in multicollinear situations itself is also becoming accepted. The fact that ridge regression and generalized inverses really convey the same message about a data set, as you will find any time you analyze the same data set by both methods, is a powerful concept. The theorems and examples of my *Technometrics* paper are being gradually internalized in the statistical community, especially by the younger generation of statisticians.

There is also important work that is beginning to sort out the effect of several separate, but interrelated, defects in regression data. Multicollinearity was the specific focal point of work on ridge and generalized inverse regression. Three other related defects in data are isolated points in regard to the predictor variables, isolated points in regard to the response variables, and errors in the predictor variable. Beyond these sources of multicollinearity I think there has been too little appreciation for what I have called in our papers the nonessential variance inflation due to lack of standardizing the predictor variables [Marquardt and Snee (1975); Marquardt (1980); Snee and Marquardt (1984)].

Hahn: I agree.

Marquardt: At the same time I think there has come to be a healthy recognition that the analysis of accumulated historical data (which typically have the various kinds of defects that I've just been mentioning) has a low batting average for getting useful, reliable results for clients. Whenever possible, designed experiments are the way to go. Bold, factorial-style experiments stand a much better chance of providing useful results. Although multicollinearity may be of less concern today because more people are using designed experiments, it's still a fundamental problem for data analysis because even in many designed experiments the predictor variable space you must explore is not symmetric. The nonsymmetry implies constraints that force multicollinearity, if not in the original data, then in the functions of those data that appear in the predictive model. So, that whole theoretical framework is important.

Hahn: Of course another approach that may cause multicollinearity is to include covariates in your analysis.

Marquardt: Yes. This applies when you don't design the experiment to include those covariates, but you take the precaution of measuring them during the experiment and then trying them out as candidate predictor variables, along with those that were designed. If the multicollinearity caused by the covariates is strong, then ridge or generalized inverse regression can be used for the analysis. I had great success in adding covariables to a designed experiment in a number of studies. In fact, I had several breakthroughs in instances where the experimenters I worked with were ready to design an experiment on only the predictor variables that they thought were important, but I persuaded them to take data also on some others that were measurable. When we analyzed the results it turned out that one of the covariates was more important than any of the design variables, even though the covariates weren't boldly and deliberately changed during the experiment. This finding was confirmed in subsequent experiments with the previous covariable included as a design variable.

Hahn: As extra insurance you collected data on the potential covariates.

Marquardt: Yes, and the insurance paid off in many instances. In one case the finding had major impact on improving the uniformity of the key property of one of DuPont's biggest products. It's a technique that even today is much too little used.

Hahn: And to the subsequent regret of the experimenters. It certainly is something on which we have to rely on the subject matter expert and the scientist to guide us.

MIXTURES: MODELS, DESIGNS, TEST STATISTICS

Hahn: Talking about experimental design, another area in which you did research is mixture designs. Would you tell us a little about that?

Marquardt: Yes, most of this work was done collaboratively with Dr. Ronald Snee, who was a big contributor within my organization and supervised a substantial portion of the organization for a number of years. Ron and I found we worked together well and synergistically. We both were involved in mixture problems from several different sources. Also, we both had stints consulting with the DuPont petroleum chemicals laboratory where blending of gasolines and other petroleum products was a key problem from both technical and business viewpoints. This led inevitably to mixture problems. So as a by-product of that consulting effort, we undertook a whole series of joint studies of mixture designs, mixture models and test statistics for mixture models [Marquardt and Snee (1974); Snee and Marquardt (1974, 1976)].

Hahn: Yes, and it seems to have led to much other important work since then.

UNEQUALLY SPACED TIME SERIES

Hahn: Also, you have been interested in the estimation of the spectra of unequally spaced time series.

Marquardt: Among the areas of my work that I feel are the most important that's the one that has been least recognized in the statistical community at large, but I do consider my work in unequally spaced time series to be just as important as my work in the other three areas that we've talked about. One reason I think that this work has received less attention may be that both of the key papers were published as book chapters rather than in the usual journals [Marquardt and Acuff (1982, 1984)]. Another reason is that due to factors unrelated to the technical work, I never was able to make computer software available for others. Nevertheless, my direct quadratic spectrum estimation, or DQSE method, as I call it, is a versatile, robust and practical method and has worked reliably for any form of unequal spacing as long as the spacing is not itself correlated with the values of the time series. Moreover, the DQSE method is straightforward to implement in a computer program.

Hahn: Undoubtedly, you run into unequally spaced time series quite often in problems in which observations are taken over time, but—unlike some economic data—people just did not end up with equally spaced observations. I know that this is often the case for production line data. I'm sure that was an important practical consideration.

Marquardt: Yes. Whenever observations are taken manually, they are inevitably spaced unequally because people can't do things at precisely the same time intervals. This happens when you have quality control measurements on a production line for some physical product or a nurse taking temperatures of patients in a hospital or in a very wide variety of other circumstances. I think time series practitioners do not appreciate the importance of accuracy of the time points at which the measurements are taken.

When you consider that a spectrum is computed over 1 or 2 orders of magnitude of frequency, and therefore wavelength, what may seem like a small difference, or error, in the point of sampling may mean a major shift in where that sample is within some of the shorter cycle lengths that are included in the frequency range of the estimated spectrum. You can quickly illustrate this if you take continuous data with known time series structure, then jitter the sample times about equally spaced target times and, finally, run the sampled data through a spectrum analysis program that assumes equal spacing. You'll then find the resulting spectrum is extremely poor, especially at the high frequency end, so it is important to have the data times recorded accurately and then to deal with the data in whatever the actual spacing may be. For example, you can have equally spaced target points, but when the data are actually taken, find, for a variety of reasons, that the observations are actually taken at unequal intervals. You may also have gaps in the data for various reasons. The literature has contained some methods for dealing with data when the unequal spacing is due to a known cause and follows a known law. That special situation really is not applicable in the big majority of instances.

It seems very important to me to have good methods for handling time series data with arbitrary unequal spacing. For analyzing unequally spaced time series data in the time domain, a variety of good methods now have been developed. However, for analysis in the frequency domain there have been relatively few publications on practical methods to calculate the power spectrum.

Hahn: That seems to provide a future opportunity for researchers.

Marquardt: I think that's absolutely right. Time series analysis, like some other areas in statistics such as nonlinear estimation, is seen as a special subject. There are people who make a career out of being specialists in time series, but the typical general practitioner statistician doesn't routinely use time series analysis methods. I think that's a mistake. In my own consulting I worked with equally spaced time series for many years and solved a large number of problems. After getting started on the unequally spaced problem, I realized I'd had a mental blind spot, never having recognized that unequally spaced data are widespread and also deserve careful attention.

In fact, as I look at my career in retrospect, I often found it necessary and interesting to tread in areas where well-established practice breaks down. A case in point is unequally spaced time series, but that's where the biggest contributions can be made. The important results were there for the picking in nonlinear estimation, in mixture problems, in biased estimation and in spectral estimation for unequally spaced time series data. There are, of course, a lot of theoretical fine points that go beyond where my work, and that of other industrial statisticians who tackle new areas, can go. That's where academic statisticians have contributed very heavily, but the important thing is to develop methodologies that are versatile and practical and that give reliable subject matter results, and that's what I think we've achieved in these four areas.

THE FUTURE: COMMUNICATIONS, VIRTUAL ORGANIZATIONS, THE GLOBAL ECONOMY AND THE ROLES OF STATISTICIANS

Hahn: Yes, you make some very important points. One of the things that makes your research so exciting is that it is all motivated by problems that you had to solve. Rather than the need to write a dissertation, you had to test the results via the real problems that motivated them in the first place. The obvious relevance of your work and the high acceptance of your publications certainly attest to your success. You've said some mighty interesting things about what you've done in the past. I'd now like to ask you to jump ahead and comment on where you think statistics is going and what you think statistics in general, and industrial statistics in particular, will be like in the year 1993 plus x, where you can choose x as you see appropriate, or even choose multiple x's. Where do you think we're heading?

Marquardt: Well, if we were to look forward 10 or 20 years, obviously there will be a lot of change. The rate of change of almost anything you can point to over the last 10 or 20 years has been extremely fast and I expect that will happen again. Certainly, for example, we'll see much more sophisticated computer software than today and more sophisticated data acquisition, much easier interfaces with computing equipment, large use of graphic presentations and built-in guidance to the practitioner on using these computerized software and graphics interfaces. When these better interfaces and more sophisticated software are available, many of the concerns that we have today about people misusing statistical methodologies will be ameliorated considerably. However, I think there are some other changes that are going on that are not as well recognized and appreciated, and which will dramatically affect the practice of statistics and the careers of statisticians in the future.

One of these is the impact of electronic communications, such as fax and E-mail and other forms of networking. These are now happening on a multinational and even global, as well as a national, basis. People can communicate now almost as readily across a continent as they used to be able to communicate across a table. We're not quite there yet, but the technology is moving in that direction and the economics of such communication is rapidly becoming better. Then add in the global availability of economical air transport of people and things. All of these are creating a single global market. I think this, in turn, is going to lead to what one might call "virtual organizations," a networking of people interested in various subjects who, despite their physical distance, can readily become functional colleagues and participants in the global marketplace. That, I think, is going to affect dramatically organizational structures, not only in universities, but also in industrial organizations. How this will all play out is not absolutely clear yet, but I believe that the practice of statistics will be very heavily affected by this technological revolution. (Note: This 1993 interview took place before the popularization of the World Wide Web.—G. Hahn)

Another thing which I believe will affect statistics and many other fields is the emergence of what today are the third world countries. Their populations are large. For many years most of these countries have been focusing on training and educating their most capable people deeply in the sciences and in mathematics. I believe we will encounter a lot of competition in commerce and in technology development from these third world countries over a period of time. This will have a dramatic effect on the development of statistics as a discipline and the U.S. as a country.

A fourth area that will be very different, I believe, will be the greater constraints on energy and on geographic space and on natural resources that are available per person due to world population growth, and this will affect both the subject matter of statisticians' attention and the manner in which they can carry out their work. When you put all these major global changes together with other things that I can't predict at this point in time, I'm sure we will have tremendously challenging opportunities, but I'm also very sure these will be amid severely constrained resources and under very severe competition.

So organizational structures will be both virtual and real, and leadership will have to be in the hands of those who can make useful results happen through others in these network situations. That's a different type of a role than statisticians today are comfortable with playing, and we're going to have to reckon with that.

Hahn: You certainly have some far-reaching ideas and I can see the impact of communications to furthering technical work, but do you also see this as impacting the mode of operation for a statistical consultant? Do you think that maybe in the future the statistical group at DuPont may not have to be in the field as much and will do "its thing" via teleconferencing and videoconferencing?

Marquardt: Oh, I think that will happen for sure, but I don't think that's the whole story. Given that so many software and hardware options will be available to users of statistics everywhere, the whole practice of statistics and the role of the consultant will change as well; not only the way that the consultant supplies services, whether by remote transmission of images and data or face-to-face. The mode will change, but also the role of the statistician will change in various ways.

Hahn: With that vision and background, are there any words of advice you'd want to offer young people contemplating statistics as a career today?

Marquardt: Well, I would think that it will be at least as important in the future as it is today to understand the existing theory base at the time one gets one's basic education, but also I think it's going to be ever more important not to be enslaved by that existing theory base because things will change, and change rapidly. The thing that I hope has been clear from this conversation is that it is important to focus on developing and implementing practical, generic approaches to solving big classes of real problems and to take the social and the psychological and the business impact of your work just as seriously as the technical part.

Hahn: Do you have any thoughts on what might be an ideal preparation for somebody who is planning to go into statistics? Basically your background was an undergraduate degree in physics, getting your hands dirty at Camp Detrick and then getting involved at DuPont, and, at the same time, going back to school to do graduate work. Do you feel that this is still an appropriate mode today or do you think it might not work any more?

Marquardt: I think it's one feasible mode. There used to be the belief that it was important to have subject matter knowledge in an applications field before you ever began to study statistics. That's the way it happened with most of the people who've been in the field for a long period of time and it certainly was true for me. In fact, this really used to be essential because statistics as a university discipline didn't exist more than 40 to 50 years ago, so just about everybody who became a statistician went to graduate school in statistics after having done undergraduate work in something else. The motivation for that to happen is still strong because there are very few schools that teach undergraduate statistics. In my ASA presidential address one of the main themes was the necessity for statistics departments to emphasize undergraduate degrees and to integrate the entire curriculum to reflect this change [Marquardt (1987)]. It is still true that having a background in some subject matter discipline is extremely helpful in understanding what data are all about. That is beginning to happen in U.S. secondary schools under the National Council of Teachers of Mathematics initiative.

For a number of years we've seen a trend toward many of the students in graduate departments of statistics having come up through the mathematics route, rather than through one of the subject matter areas. This is accelerated in many schools because of the very heavy mathematics requirements that the graduate level statistics courses are demanding. I don't feel that this is acceptable as a universal feature. If it becomes heavily predominant, it will tend to make the discipline ingrown and will hurt it in the long run. There are things that are happening in the outside world which are working against that being too serious a problem. Certainly electronic computers and the graphics and simulation approaches with those computers is a very healthy trend which I think will be a counterbalance.

ASA — RESTRUCTURING TO RELEASE PENT-UP INITIATIVES

Hahn: I absolutely agree. Now turning yet to another topic: among your accomplishments was your stint as President of the American Statistical Association in 1986. This puts you in a good place to comment on where you think the profession stands today, although of course you have already said many things about that. What do you feel is the health of our profession and what might we be doing that we are not?

Marquardt: My years as President-Elect, President and Past President of ASA were, of course, an honor and a challenge and a time of great accom-

FIG. 3. *Barbara Bailar, ASA President-Elect, Donald Marquardt, ASA President, Richard Heckert, CEO of the DuPont Company, and Janet Norwood, ASA President in 1989. Photo taken at the ASA Annual Meeting in 1986. Dr. Heckert was the President's Invited Speaker.*

plishments both for me and for the ASA. It really was a turning point during which the ASA began to emerge from many years of ingrown scholarly focus and to expand into many new professional arenas. My work in leading a broad-based, dynamic strategic planning process for the ASA was, I believe, the vehicle that made the turning point happen in a timely way [Marquardt, Neter and Bailar (1986); Bailar, Fienberg, Marquardt, Neter and Norwood (1988)]. The result was a new constitution and a new organizational structure whose major characteristic was that it decentralized control. By doing so, we unleashed what I think were pent-up initiatives in many areas of statistics.

Hahn: I'm sure your background in industry was very helpful in making this happen.

Marquardt: Yes, I believe so. Walter Shewhart, who was President in 1945, had the dual features of coming from industry and having a career and background principally in the physical sciences. I was the first ASA President after him to have that combination.

A WORK ETHIC COUPLED TO A PERSONAL VISION

Hahn: With all these accomplishments, the next question is probably superfluous but I'll ask it anyhow. Do you regard yourself as a workaholic, and what things beyond the world of statistics turn you on?

Marquardt: I guess, yes, I've always been a workaholic. I think work should be a pleasure even when the going gets difficult, as it always will. It's important to have a vision of who you are and where you want to make your contributions over the next 5 or 10 years, no matter where you are in your career. I think it's important to keep moving the goal you have for yourself so that you never are quite there. You have to make it more ambitious, but never make it so that it's totally out of sight. I do have interests other than statistics. I had some recreational sports interests earlier in my career, but now my interests are primarily people-oriented and intellectual.

Hahn: What kinds of sports were you involved in?

Marquardt: Oh, I did sailing, bicycling and swimming, but never at a competitive level. I still walk a lot; also I've been keenly interested in photography for many years.

Hahn: Would you say now that you're more people-oriented?

Marquardt: I think so, yes. Looking back on my career, even from my early years, I've wanted to be influential in multiple arenas.

Hahn: You certainly have been that.

Marquardt: At the core I am dedicated to research and innovation. It's fundamental to my nature to want to influence people and ideas and organizations in ways that really count. The only way to do all these is to wear many people-oriented hats, so I have been researcher, teacher, businessman, leader, administrator, writer, lecturer, publisher and mentor. In fact, since I left DuPont, the one thing I miss the most is the opportunity to be a mentor of people in an organization.

One of my principal role models has been Crawford Greenewalt, who was chief executive of the DuPont Company when I first joined in 1953. The company prospered during his long tenure. I had the privilege of working with him in some of his personal research efforts. He was an outstanding expert on all the various things that relate to hummingbirds.

Hahn: My goodness!

Marquardt: In regard to their methodology of flight and how it compares to other flying objects, whether birds or airplanes. He has done substantial research on the coloration effects of hummingbird feathers. When you change the angle of looking at them, the color of light reflected back always changes. I worked with him on some of the optical math modeling and data analysis of these various studies, and did computer work with him for a number of years. He continued his personal research and important publications in that and other areas all through his career and long after his retirement. He has been described as a true Renaissance man. In recent years that term has been applied to me a couple of times. I take that as indication that I have achieved some of my goals.

ISO 9000 AND AN INTERNATIONAL NETWORK OF COLLEAGUES

Hahn: Yes, and, of course, that suggests yet another area in which you've been involved over recent years, and that has been in the development and practical application of the key international quality standards, ISO 9000. Could you say a little about what ISO 9000 is, and then tell us about your contributions and your current interest in that area?

Marquardt: Let me put it in perspective in the sense of career evolution since we've just been talking about that. There's a career model described by Dalton and Thompson (1986) in a book entitled *Novations*. Their analysis, based on many people in many kinds of organizations, is that a career can go through one or more of four successive stages. In the beginning, one is essentially an apprentice and dependent upon guidance from others; later one graduates into a stage of independence, developing possibly a broad reputation for excellence of work in a particular field; then perhaps one moves on to a third stage, to be a mentor or supervisor where one is influencing the work and development of other individuals; finally, some people move along to a fourth stage where they may be described as an organization change agent, where their focus is on influencing whole organizations rather than simply individual people or individual technical results. Not everybody moves through all of these stages, but they are four different kinds of roles one can have and they tend to follow one after another in a career. In moving from one stage to the next you must go through a personal change process in your approach. This change, or novation, requires deliberate effort. In my case I've deliberately continued to operate in all of stages two, three and four, so I've continued to do teaching, lecturing, consulting, writing and research in statistical methodology. The second, third and fourth career stages are all satisfying from my perspective.

Nevertheless, my emphasis in the last two decades has been on influencing beneficial changes in organizations. This certainly has included DuPont. It also has included the American Statistical Association. Since 1980 I've taken this role in connection with the ISO 9000 work, in international approaches to management systems and quality standards.

I began my work on ISO's Technical Committee 69 which deals with applications of statistical methods. During my work with that international committee, I was for several years the chairman of the subcommittee on statistical process control. In 1980 the ISO Technical Committee 176 had its first meeting on quality standards. I attended that meeting and have been heavily involved ever since. The initial series of ISO 9000 standards was published in 1987 and they have had very rapid implementation throughout the world. In fact, the ISO 9000 series as of mid-1993 has been adopted by over 73 nations, including the United States, so it has had a profound impact. What's becoming realized now, as ISO 9000 standards are in wide use in compa-

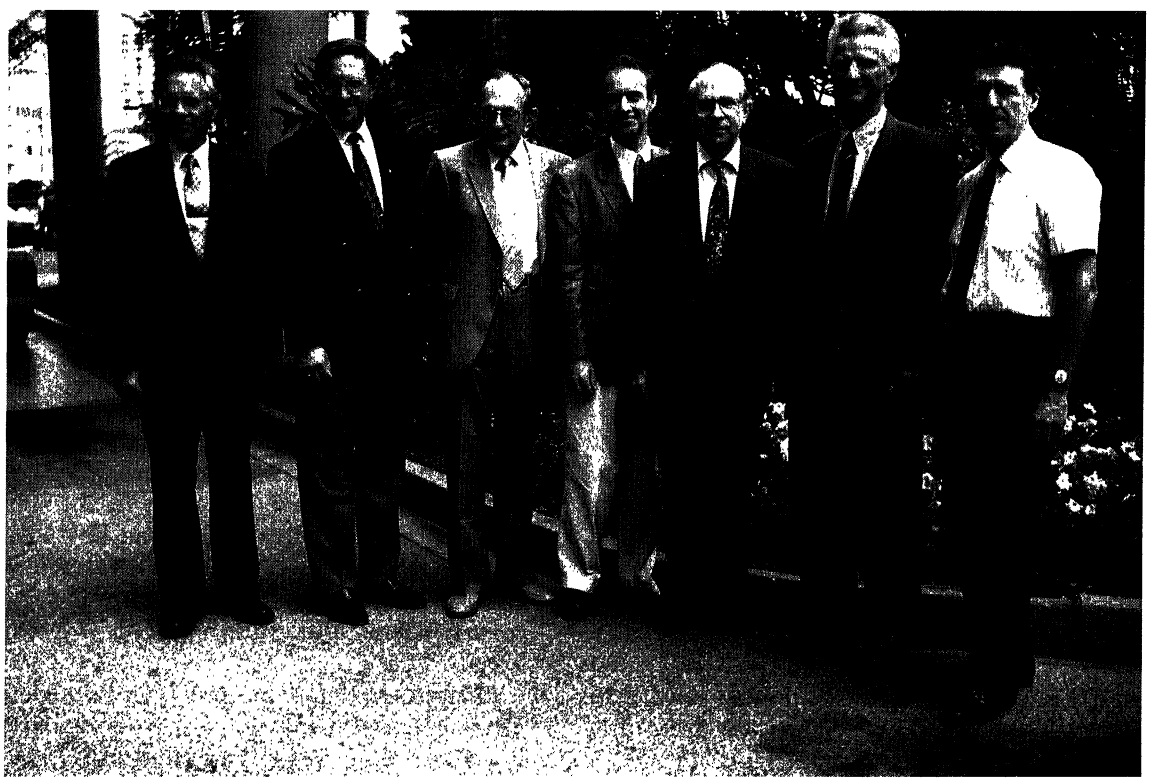

FIG. 4. *Seven of the eight people worldwide who participated in the initial meeting of ISO/TC176 in 1980, and who were still active in the international committee in 1992. Left to right: D. Marquardt (USA), T. Bøhler (Norway), A. Stanenas (Canada), J. Enger (Sweden), R. Peach (USA), K. Petrick (Germany) and K. Ford (Canada). In September 1993 Marquardt was appointed convener of the international strategic planning advisory group; Bøhler, Petrick and Ford are among the members. Photo taken November 1992 in Brisbane, Australia.*

nies and other organizations worldwide, is that they are in fact generic management standards, applicable not only for the management of product quality, but for other management areas such as the environment, safety and health and even finance. They are being viewed now as a prototype for application in those other areas. Just recently, Technical Committee 207 of ISO has been formed in the field of the environment.

Statisticians are going to have to reckon with the fact that their discipline is becoming intertwined with all of these other areas; not just in the technology, but also, and most importantly, in regard to the management principles that are involved in quality and in the environment and other areas. Statistical practitioners will have to understand these managing approaches and be able to purvey their statistical expertise in terms of these globally accepted managing approaches. If you try to fight it, you're not going to get very far.

Hahn: Yes, indeed. Are you still involved with work with ISO 9000?

Marquardt: Yes, I've been the leader of the U.S. delegation to ISO TC176 since 1989 and I hold a number of leadership positions in the international committee itself. I've been very much involved in developing the long-term strategy for TC176 [Marquardt, Chové, Jensen, Petrick, Pyle and Strahle (1991); Durand, Marquardt, Peach and Pyle (1993)].

Hahn: Your influence in all this work certainly seems to have transcended national boundaries.

Marquardt: It's been a real delight to get to know outstanding people in a large number of countries and to develop a network of close friends and colleagues worldwide.

Hahn: Has this resulted in a lot of travel?

Marquardt: A great deal.

Hahn: You've also been a senior examiner for the Malcolm Baldrige National Quality Award. How do you view the relationship between the ISO 9000 standards and the Baldrige award?

Marquardt: I think the relationship can be, and should be, one of great harmony. Both deal with generic managing processes and criteria applicable to any organization. Both are pathways to improved product quality, organizational performance and business success.

The ISO 9000 requirements standards provide, in my view, a practical, proven, no-nonsense framework to understand what managing processes are necessary in any organization and the basic criteria for adequacy of those processes. The guidance standards in the ISO 9000 family go a step beyond the requirements standards and show the way to superior performance and results. These ISO 9000 approaches are attainable by almost any organization. Nevertheless, most organizations today require a year or more of dedicated effort to achieve ISO 9000 compliance. Implementation of the standards involves periodic internal and external audits as a key provision. Follow-up from the audits has proven to be a powerful source of continued improvements. On the other hand, I think the Baldrige criteria are the most comprehensive, succinct and useful statement available to describe the processes and practices that characterize an organization performing at the best-of-the-best level in all functions, including product design, development, production, delivery as well as interactions with customers, society, employees and other stakeholders. As you know, the Baldrige criteria are intended to be a way of selecting a tiny fraction of organizations as award winners. The Baldrige criteria also are intended as a long-range goal for any organization.

In my view, satisfying the ISO 9000 standards should be seen as a necessary prerequisite before attempting an across-the-board Baldrige effort. Having worked in both ISO 9000 and Baldrige, I see them as very compatible and synergistic. We should encourage statisticians to learn about them and to operate within their conceptual framework.

CURRENT ACTIVITIES

Hahn: Although I know you formally retired a couple of years ago from DuPont, I also know that you have far from retired in any real sense. In addition to the ISO 9000 work that you've described, I know that you are now actively consulting under the banner of Donald W. Marquardt and Associates. Could you tell us a little about what you are doing now and what you look forward to doing in the years to come?

Marquardt: I am devoting about half of my time to volunteer work for ISO international standards, for the Registrar Accreditation Board and for professional associations. The other half of my time I'm devoting to consulting with companies and trade associations and to graduate-level university teaching. My work for companies is focused on improving their management approaches, helping with strategic planning and, of course, in doing statistical design and data analyses and teaching short courses. A significant part of my business is in the developing countries, such as Brazil and India.

Hahn: Do you have any technical work underway that we can look forward to?

Marquardt: Yes, I gave a paper at the ASA/ASQC Fall Technical Conference, in 1984, on report card issues in quality management. I've given many lectures on that topic, and have taught it in

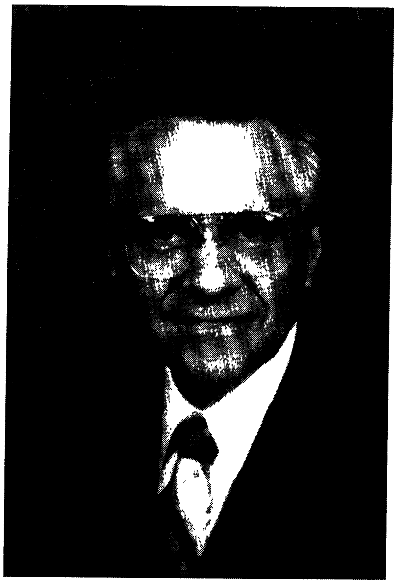

FIG. 5. *Donald Marquardt, 1992, taken during the first year of establishing Donald W. Marquardt and Associates.*

graduate courses, but I had never submitted it for publication. (Editorial note: Now published in ASQC's new Quality Management Journal [Marquardt (1994)].) I've also completed a paper Estimating the Standard Deviation for Statistical Process Control [Marquardt (1993)]. This paper exhibits several important properties of the mean-square successive difference estimator and generalizes the approach to a whole class of estimators. Another direction of my research is the development of Twin Metric statistical process control, derived from CUSUM control, but adapted to have the simplicity, look and feel of Shewhart charts together with the threefold or more performance improvement over Shewhart charts [Marquardt and Ulery (1992)]. Later this week I'll be discussing Twin Metric control during my presentation at this Gordon Research Conference. [Editorial note: A comprehensive paper is accepted for publication [Marquardt (1997).]

Hahn: I think it shows a very interesting example of applying methods which are a lot more efficient than the standard Shewhart methodology, but which in the past may not have been used as heavily as they should be.

REFLECTIONS ON CONSULTING: PAST AND FUTURE

Hahn: One area that I neglected so far has been your frequently quoted writings on statistical consulting, in such publications as *The American Statistician*. Perhaps you would like to update your comments in these articles by talking about the role of the industrial statistician in consulting in these difficult economic times.

Marquardt: Yes, I think we are in a period of transition, as I've indicated before. That will affect the practice of consulting. I believe all of the concepts and paradigms that are articulated in my two papers [Marquardt (1979b, 1981)] are still valid, but they have to be supplemented by other paradigms, responding to things that have happened in the United States in the last 15 years. The mathematics and science skills of the typical employee are not as good today as they were. Our people are just as capable, but our educational system has not given them these skills. This has many ramifications regarding the feasible approaches in companies everywhere.

In addition to the less thorough mathematics and science training of typical employees in companies, there has been a dramatic thinning down of organizational structures. The combined effect of these two demographic changes has made it impractical today to try to use technology that is as complex as the technology we once used successfully. There is a need today to work with less complex technology, except where the complexity can be buried inside computer software. It has always been important to match technology complexity to the skill levels of employees, but the balance point has changed.

Some important new educational initiatives are beginning to happen in the United States, but it will take a decade or more for the effect to be seen in our work force. This will be a difficult period for U.S. industry in competition with developing countries that have educated large numbers of people to a high level of skill in mathematics and science. We will have to focus on technology that gives high performance with modest complexity.

Hahn: Your work on Twin Metric control is a great example of that.

Marquardt: It was specifically motivated by an understanding of the change that I just described.

Hahn: Well, it's been a great pleasure to spend this time with you. Thank you for sharing your thoughts and experiences.

REFERENCES

BAILAR, B. A., FIENBERG, S. E., MARQUARDT, D. W., NETER, J. and NORWOOD, J. L. (1988). Strategic planning for the American Statistical Association, 1984–1987. *Amer. Statist.* **42** 1–9.

CARR, J. W. (1958). *J. Assoc. Comput. Mach.* **5** 39–44.

COLEMAN, B. D. and MARQUARDT, D. W. (1957). Time dependence of mechanical breakdown in bundles of fibers. II. Linearly increasing load. *J. Appl. Phys.* **28** 1065.

COLEMAN, B. D. and MARQUARDT, D. W. (1958). Time dependence of mechanical breakdown in bundles of fibers. IV. Infinite ideal bundle under oscillating loads. *J. Appl. Phys.* **29** 1091–1099.

DALTON, G. W. and THOMPSON, P. H. (1986). *Novations: Strategies for Career Management.* Scott, Foresman and Company, Glenview, IL.

DURAND, I. G., MARQUARDT, D. W., PEACH, R. W. and PYLE, J. C. (1993). Updating the ISO 9000 quality standards: responding to the marketplace needs. *Quality Progress* **26** 23–28.

HOERL, A. E. and KENNARD, R. W. (1970a). Ridge regression: applications to nonorthogonal problems. *Technometrics* **12** 69–82.

HOERL, A. E. and KENNARD, R. W. (1970b). Ridge regression: biased estimation for nonorthogonal problems. *Technometrics* **12** 55–67.

MARQUARDT, D. W. (1959). Solution of nonlinear chemical engineering models. *Chemical Engineering Progress* **55** 65–70.

MARQUARDT, D. W. (1963). An algorithm for least-squares estimation of nonlinear parameters. *SIAM J. Appl. Math.* **11** 431–441.

MARQUARDT, D. W. (1966). NLIN2—Least squares estimation of nonlinear parameters. IBM share Library 3094.01.

MARQUARDT, D. W. (1970). Generalized inverses, ridge regression, biased linear estimation, and nonlinear estimation. *Technometrics* **12** 591–612.

MARQUARDT, D W. (1979a). Citation classic. *Current Contents* **10** 14.

MARQUARDT, D. W. (1979b). Statistical consulting in industry. *Amer. Statist.* **33** 102–107.

MARQUARDT, D. W. (1980). You should standardize the predictor variables in your regression models. *J. Amer. Statist. Assoc.* **75** 87–91.

MARQUARDT, D. W. (1981). Criteria for evaluating the performance of statistical consultants in industry. *Amer. Statist.* **35** 216–219.

MARQUARDT, D. W. (1987). The importance of statisticians. *J. Amer. Statist. Assoc.* **82** 1–7.

MARQUARDT, D. W. (1993). Estimating the standard deviation for statistical process control. *International Journal of Quality and Reliability Management* **10** 57–64.

MARQUARDT, D. W. (1994). Report card issues in quality management. *Quality Management Journal* **1** 16–25.

MARQUARDT, D. W. (1997). Twin Metric control: CUSUM simplified in a Shewhart framework. *International Journal of Quality and Reliability Management* **14**. To appear.

MARQUARDT, D. W. and ACUFF, S. K. (1982). Direct quadratic spectrum estimation from unequally spaced data. In *Applied Time Series Analysis* (O. D. Anderson and M. R. Perryman, eds.) 199–227. North-Holland, Amsterdam.

MARQUARDT, D. W. and ACUFF, S. K. (1984). Direct quadratic spectrum estimation with irregularly spaced data. *Proceedings of the Symposium on Time Series Analysis of Irregularly Observed Data. Lecture Notes in Statist.* **25** 211–223. Springer, New York.

MARQUARDT, D. W., CHOVÉ, J., JENSEN, K., PETRICK, K., PYLE, J. and STRAHLE, D. (1991). Vision 2000: the strategy for the ISO series standards in the '90s. *Quality Progress* **24** 25–31.

MARQUARDT, D. W., NETER, J. and BAILAR, B. A. (1986). ASA strategic planning, part 1: diagnosis of needs and developments of goals. *AMSTAT News* June 5–9.

MARQUARDT, D. W. and SNEE, R. D. (1974). Test statistics for mixture models. *Technometrics* **16** 533–537.

MARQUARDT, D. W. and SNEE, R. D. (1975). Ridge regression in practice. *Amer. Statist.* **29** 3–20.

MARQUARDT, D. W. and ULERY, D. L. (1992). Twin Metric control: improving SPC to meet supply chain realities of the 1990s. In *Transactions of the 46th Annual Quality Congress* 367–373. American Society for Quality Control, Milwaukee.

SNEE, R. D. and MARQUARDT, D. W. (1974). Extreme vertices designs for linear mixture models. *Technometrics* **16** 399–408.

SNEE, R. D. and MARQUARDT, D. W. (1976). Screening concepts and designs for experiments with mixtures. *Technometrics* **18** 19–29.

SNEE, R. D. and MARQUARDT, D. W. (1984). Collinearity diagnostics depend on the domain of prediction, the model, and the data. *Amer. Statist.* **38** 83–87.