GEORG RASCH

The Man Behind The Model

The Mathematician

Georg Rasch

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Georg Rasch, Doctor of Philosophy in mathematics (1930), member of the International Statistical Institute (1941), charter member of the Biometrics Society (1947), Professor of Statistics at the University of Copenhagen (1962), and Danish Knight of the Order of Dannebrog (1967), was born in Odense, Denmark, on 21 September 1901, the youngest and "least practical" of three brothers.[1]

His mother was ill throughout his childhood and Rasch had few recollections of her. But his fiercely religious father left deep and lasting impressions. Wilhelm Rasch, sailor, ship's officer, mathematics teacher and self-anointed missionary, was, "the most hard-boiled evangelist I have ever known."

Wilhelm dragged his family to Svendborg in 1906 to open a mission high school for prospective seamen. In 1914 Georg became fascinated by the trigonometry texts in his father's library and fell in with a school teacher who made mathematics "something with which a wonderful world was opened."

The teacher realized that Georg was a born mathematician and persuaded his frugal father to invest in sending Georg to the cathedral school in Odense where there was a good mathematics curriculum. Georg made the most of it and went on to the University of Copenhagen in 1919.

> *I entered the Faculty of Science, to which mathematics belonged, and got into immediate contact with my teachers. I had, of course, to learn the elements of function theory and even geometry, but I concentrated upon the analytic part. What caught my interest was the theory of Lagrange equations. This resulted in my first publication (Neilsen & Rasch 1923).*
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> *I got a stipend for my studies and became a member of college Regensen where we received free room and board. Since I did not see any further reason for doing arithmetical work for my living, I left Professor Neilsen and got another teacher, Professor Nørlund, who had written an extremely good book on difference equations.*
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> *Nørlund was my professor for the rest of my time as a student, and I was his assistant teacher from 1925, when I graduated, until 1940. The topics in function theory that Nørlund lectured about together with the other topics I had to study in order to lecture as his assistant built up my mathematical background.*
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> *Nørlund was also director of the Geodetic Institute to which I became attached to provide mathematical and computational assistance. This added to my income and in 1928, I married my sweetheart, Elna Nielsen, with the charming nickname "Nille". Two daughters were added to the family in 1931 and 1933.*
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> *My thesis, defended in 1930, was the fruit of my cooperation with Nørlund, but in a field which he himself did not cultivate. It dealt with matrix algebra and its applications to linear systems of differential equations. I have always loved to think, but I have never*

been inclined to do much reading. So I had never seen anything about matrices. Nørlund gave lectures on difference equations in which he wrote out every equation in detail every time. When working through my notes I discovered, to my surprise, that these long equations could be condensed in a simple way. I did not know anything about matrices at that time, but just invented them for myself and discovered what their rules must be. Only later did I find out that others had formalized the same idea.

I invented my own theory of matrices, especially as they applied to linear systems of differential equations. The part of my thesis on the theory and application of product integrals which developed a linear system of differential equations as a generalization of the ordinary elementary integral was published in German (Rasch 1934). Years later I learned that the techniques developed in this paper played a part in solving problems in atomic theory and were also used to prove some difficult theorems in group theory.

The early 1930s were difficult. Aside from teaching as Nørlund's assistant and small jobs for the Geodetic Institute, there was no work in mathematics. So Rasch helped two medical acquaintances studying the reabsorption of cerebrospinal fluid to understand their data. This gave him his first experience with the exponential distribution and material for his first experimental paper (Fog, Rasch & Stürup 1934).

The success of this collaboration motivated Fog and Stürup to engage Rasch to teach mathematics and statistics to a small group of psychiatrists and neurologists. Word of this got to the head of the Hygienic Institute, who was also interested in statistics. The outcome was that Rasch served the Hygienic Institute as statistical consultant from 1934 to 1948 and also become attached to the State Serum Institute, a relationship which continued until 1956.

About the same time Nørlund, for whom Rasch still taught mathematics, and Madsen, Director of the Serum Institute, got into a conversation about Rasch's work and decided that to do his job at the Serum Institute, he needed to learn the latest developments in statistics. They applied to the Rockefeller Foundation for Rasch to study with R.A. Fisher.

The Rockefeller fellowship was granted, but, while it was brewing, Rasch went to Oslo on a Carlsberg grant to study Ragnar Frisch's confluence analysis, a technique developed for economics, but similar to factor analysis. Then in September 1934 Rasch joined Fisher at the Galton Laboratory in London.

I went through Fisher's statistical methods and soon got hold of his 1922 paper where he developed his theory of maximum likelihood. What caught my interest was his idea that this is a form of generalization of the same kind as Gauss attempted when he invented least squares.

The meaning of least squares is not, in Fisher's interpretation, however, just a minimization of a sum of squares. It is a maximization of the probability of the observations. There is an essential difference between this and the simple idea of minimizing sums of squares.

This philosophy went further when Fisher got to his concept of sufficiency. To mathematical minds sufficiency may appeal as nothing more than a surprising nice property, extremely handy when accessible, but, if not, then you just do without it. But to me sufficiency means much more than that. When a sufficient estimate exists, it extracts every bit of knowledge about a specified feature of the situation made available by the data as formalized by the chosen model. 'Sufficient' stands for 'exhaustive' as regards the feature in question.

What is left over when a sufficient estimate has been extracted from data is independent of the trait in question and may therefore be used for a control of the model that does not depend on how the actual estimates happen to reproduce the original data. This is the cornerstone of the probabilistic models that generate specific objectivity.

The realization of the concept of sufficiency, I think, is a substantial contribution to the theory of knowledge and the high mark of what Fisher did. His formalization of sufficiency nails down the conditions that a model must fulfill in order to yield an objective basis for inference.

During his year in London, Rasch also discussed the problem of relative growth with Julian Huxley. Using data on crab shell structure, Rasch discovered that it was possible to measure the growth of individual crabs as well as populations.

It meant a lot to me to realize the meaning and importance of dealing with individuals and not with demography. Later I realized that test psychologists were not dealing with the testing of individuals, but were studying how traits, such as intelligence, were distributed in populations. They were making demographic studies and not studies of individuals.

Rasch began teaching statistics to biologists in the fall of 1936. Then in 1938 the director of the University of Copenhagen Psychological Laboratory learned of Rasch's interest in statistics. The director asked Rasch to give some lectures to his psychologists. The result was a connection lasting thirty years.

Rasch began his work on psychological measurement in 1945 when he helped standardize an intelligence test for the Danish Department of Defense (Rasch 1947).

In carrying out the item analysis I became aware of the problem of defining the difficulty of an item independently of the population and the ability of an individual independently of which items he had actually solved.

A friendship with Chester Bliss formed in London in 1935

brought Rasch to the United States in 1947 to participate in the founding of the Biometrics Society (Rasch 1947a) and the postwar reorganization of the International Statistical Institute. Tjalling Koopmans, a fellow student of Ragnar Frisch's confluence analysis and Fisher's sufficient statistics, invited Rasch to spend two months with the Cowles Commission for Research in Economics at the University of Chicago, where Rasch met Jimmie Savage.

> *In 1951 I was faced with a task the solution of which added a new tool to my arsenal. The Danish Ministry of Social Affairs wanted an investigation of the development of reading ability in 125 former students of public schools in Copenhagen, who in their school years had suffered from serious reading difficulties and therefore had received supplementary education in that discipline.*
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> *For each of these students were recorded the results of repeated oral reading tests during his school years. It would be a simple task to follow the development of a student's reading ability over a number of years if the same part of the same test were used every time, but at each testing it was necessary to choose a test which corresponded to the student's standpoint, so each student was followed up with a series of tests of increasing "degrees of difficulty."*
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> *In a concrete formulation of this problem I imagined — in good statistical tradition — the possibility that the reading ability of a student at each stage could be characterized in a quantitative way — not through a more or less arbitrary grading scale, but by a positive real number defined as regularly as the measurement of length.*
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> *Whether this would be possible with the tests in question could not be known in advance. It had to be tried out through a separate experiment which was carried out in January 1952. In this experiment 500 students in the 3rd – 7th school year were tested with 2 or 3 of the texts used in the earlier investigation. (Rasch 1977, 58-59)*
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> *I chose the multiplicative Poisson for the reading tests because it seemed a good idea mathematically, if it would work. It turned out that it did and so I wanted to have some motivation for using it. In order to do so, I imitated the proof of a theorem concerning a large number of independent dichotomous events, each of which had a small probability. Under these conditions the number of events becomes Poisson distributed. I took care that my imitation ended up with the multiplicative Poisson model, that is, I made sure that there was a personal factor entering into each of the small probabilities for the dichotomous outcome and that each item would have its own parameter and then I had my new model.*
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> *I had taken a great interest in intelligence tests while working with them in 1945. It struck me that I might analyze the test we had constructed then, and which had been taken over by the Military Psychology Group.*
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> *The first thing I did was to analyze the Raven tests. They worked almost perfectly according to the multiplicative model for dichotomous items. That was my first example using the newly discovered model. Now I compared the results of the Raven's test and the results of my analysis of the military intelligence test. The intelligence test did not conform.*
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> *When I showed this to the head of the military psychologists he saw the point. I had talked to him about my attempts to make sense of intelligence tests by means of the model I had discovered in connection with the multiplicative Poisson. I had also told him about the Raven's tests. Now I presented the examination of the test he actually had in current use from the Psychology Laboratory. I pointed out that it seemed to consist of different groups of items with quite different kinds of subject matter.*
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> *His immediate reaction was to call on Borge Prien who was working for the military psychologists and to give him the order that, within the next six months, before the next testing session in November 1953, to have ready a new intelligence test consisting of four different subtests, each of these to be built in such a way that they followed the requirements that Rasch demanded.*
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> *It was remarkable. Prien actually did that in six months. He invented tests, which, when you see them, are rather surprising. He really did invent items of the same sort, from very easy to very difficult, and spaced in a sensible way. We did do some checking in the process and omitted or modified items that did not seem to be working. It was a masterpiece. Prien had been told, 'All you have to construct is four different kinds of tests, with very different subject matters and each of them should be just as good as Georg tells us that Raven's tests are.' And so he did. That was when I really began to believe in the applicability of that elementary model.*

THE BOOK

The establishment in 1955 of the Danish Institute for Educational Research brought Rasch a wealth of problems requiring clarifications, elaborations, and extensions of the principles already laid down.

> *In 1957 I gave some lectures on the researches I had done since Prien's construction of the new intelligence tests. I told about the multiplicative Poisson and about the nice little model which sorts items out from each other. My lectures were tape-recorded, and my daughter Lotte got the task of deciphering them and writing them down. She made a proper work out of it, and what she did was taken over by the Educational*

Institute, and they had it mimeographed.

At that time the institute consisted of five different departments, each with its own head. Every Friday morning the company of them, together with the director, Erik Thomsen, and I had a meeting where we discussed current matters. Thomsen organized it so that on a number of these Fridays we went through my manuscript. That clarified many points that I had been vague about. I was forced by the young fellows there to make clear what I meant.

A preliminary Danish edition of the manuscript was carefully scrutinized by the staff members of the Institute. The Danish text was transformed into English by G. Leunbach, who has also revised later additions in English. Finally, in 1960, L.J.Savage of the University of Chicago reviewed the final manuscript critically.

The outcome of the reading test experiment was beyond expectation: a statistically satisfactory analysis on the basis of a new model which represented a genuine innovation in statistical techniques!

But the understanding of what the model entails tarried several years. Then at the 1959 anniversary of the University of Copenhagen the highly esteemed Norwegian economist Ragnar Frisch — later Nobel Prize winner — came to Copenhagen to receive an honorary doctorate. I visited him the next day, and he asked me what I had been doing in the 25 years since I stayed at his institute in Oslo for a couple of months to study his new techniques of statistical analysis. I soon concentrated on the comparison of reading speeds which I proceeded to explain.

Applying my measurement model to reading speeds states that the probability that person n in a given time reads a_{ni}, words of text i is determined by the Poisson distribution.

The Poisson distribution has the important property that the sum of the two Poisson distributed variables is also Poisson distributed with a parameter which is the sum of the two parameter values.

In a class of possible outcomes of this kind where the total number of words read, a_{n+}, has a fixed value, the probability of the outcomes a_{ni} and a_{nj} conditional on the total a_{n+}, is given by dividing the two Poisson variables.

Until now Frisch had only listened politely, but now I presented a crucial point which demands a careful inspection.

When one Poisson distribution is divided into another, factors cancel, and the resulting conditional probability does not contain the person parameter. The probability that the given number of words read, a_{n+}, is composed of a_{ni} and a_{nj} words of the two tests is therefore expressed by

$$p\{a_{ni}, a_{nj} \mid a_{n+}\} = \binom{a_{n+}}{a_{ni}} \left(\frac{E_i}{E_i+E_j}\right)^{a_{ni}} \left(\frac{E_j}{E_i+E_j}\right)^{a_{nj}}$$

which is determined by the observed numbers a_{ni} and a_{nj} and by the ratio between the difficulty parameters of the two tests E_i and E_j, while it is not influenced by which person is involved. On seeing this Frisch opened his eyes widely and exclaimed: "It (the person parameter) was eliminated, that is most interesting!" And this he repeated several times during our further conversation. To which I of course agreed every time — while I continued reporting the main results of the investigation and some of my other work.

Only some days later did I all of a sudden realize what in my exposition had caused this reaction from Ragnar Frisch. And immediately I saw the importance of finding an answer to the following question: "Which class of probability models has the property in common with the Multiplicative Poisson Model, that one set of parameters can be eliminated by means of conditional probabilities while attention is concentrated on the other set, and vice versa?"

What Frisch's astonishment had done was to point out to me that the possibility of separating two sets of parameters must be a fundamental property of a very important class of models.(Rasch 1977, 63-66)

By 1953 Rasch had used a Poisson model to analyze a family of oral reading tests and with Borge Prien had designed and built a four-test intelligence battery each test of which fit the requirements of his logistic model for item analysis. Rasch discussed his concern about sample dependent estimates in his article on simultaneous factor analysis in several populations (Rasch 1953). However, his work on item analysis remained unknown outside Denmark until 1960, when he lectured in Chicago, gave a paper at the Berkeley Symposium on Mathematical Statistics (Rasch 1961), and published *Probabilistic Models*.

PREFACE to *Probabilistic Models*

For several years statistical methods have been a favorite instrument within various branches of psychology. Warnings have, however, not always been wanting. Two instances from recent literature may serve as examples.

Skinner[1] vigorously attacks the application of statistics in psychological research, maintaining that the order to be found in human and animal behavior should be extracted from investigations into individuals, and that psychometric methods are inadequate for such purposes since they deal with groups of individuals.

As far as abnormal psychology is concerned Zubin[2] expresses a similar view in stating: "Recourse must be had to individual statistics, treating each patient as a separate universe. Unfortunately, present day statistical methods are entirely group-centered so that there is a real need for developing individual-centered statistics."

Individual-centered statistical techniques require models in which each individual is characterized separately and from which, given adequate data, the individual parameters can be estimated. It is further essential that comparisons between individuals become independent of which particular instruments tests or items or other stimuli — within the class considered have been used. Symmetrically, it ought to be possible to compare stimuli belonging to the same class — "measuring the same thing" — independent of which particular individuals within a class considered were instrumental for the comparison.

This is a huge challenge, but once the problem has been formulated it does seem possible to meet it. The present work demonstrates, by way of three examples from test psychology, certain possibilities for building up models meeting these demands. And it would seem quite possible to modify and extend the methods used here to cover much larger areas, but in order to investigate how far the principles go — and what should be done outside possible limits — much research is needed. It is hoped, however, that planned continuations of the present work and contributions from others will gradually enlarge the field where fruitful models can be established. (Rasch 1960, xx-xxi)

In her 1965 review Jane Loevinger wrote,

Rasch (1960) has devised a truly new approach to psychometric problems.... He makes use of none of the classical psychometrics, but rather applies algebra anew to a probabilistic model. The probability that a person will answer an item correctly is assumed to be the product of an ability parameter pertaining only to the person and a difficulty parameter pertaining only to the item. Beyond specifying one person as the standard of ability or one item as the standard of difficulty, the ability assigned to an individual is independent of that of other members of the group and of the particular items with which he is tested; similarly for the item difficulty.... Indeed, these two properties were once suggested as criteria for absolute scaling (Loevinger, 1947); at that time proposed schemes for absolute scaling had not been shown to satisfy the criteria, nor does Guttman scaling do so. Thus, Rasch must be credited with an outstanding contribution to one of the two central psychometric problems, the achievement of non-arbitrary measures. Rasch is concerned with a different and more rigorous kind of generalization than Cronbach, Rajaratnam, and Gleser. When his model fits, the results are independent of the sample of persons and of the particular items within some broad limits. Within these limits, generality is, one might say, complete. (Loevinger 1965, 151).

In the 60's I introduced a more definite version of an old epistemological concept. I preserved the name of objectivity, but since the meaning of that word has undergone many changes since its Hellenic origin and is used in everyday speech as well as scientific discourse with many different contents, I added a restricting predicate: specific.

My professional background is mathematical and statistical, not philosophical. The concept has therefore not been carved out in a conceptual analysis, but, on the contrary, its necessity has appeared in my practical activity as a statistical consultant. (Rasch 1977, 58)

It is the two earliest and most popular members of this "very important class of models" which Rasch applies in *Probabilistic Models*. Although the book focuses on the measurement of reading accuracy, speed, and intelligence, the basic principles employed are fundamental to all scientific work.

When first suggesting the models (for measuring) I could offer no better excuse for them than their apparent suitability, which showed in their rather striking mathematical properties. In Rasch (1961) a more general point of view was indicated, according to which the models were strongly connected with what seemed to be basic demands for a much needed generalization of the concept of measurement.

In continuation of that paper my attention was drawn to other fields of knowledge, such as economics, sociology, history, linguistics, evaluation of arts, etc. where claims are arising of being taken just as seriously as Natural Sciences.

On a first sight the observational material in Humanities would seem very difficult from that in physics, chemistry and biology, not to speak of mathematics. But it might turn out that the difference is less essential than it would seem. In fact, the question is not whether the observations are of very different types, but whether Sciences could be firmly established on the basis of quite different types of observation. (Rasch 1967.)

The psychometric methods introduced in Rasch's book go far beyond measurement in education or psychology. They embody the essential principles of measurement itself, the principles on which objectivity and reproducibility, indeed all scientific knowledge, are based. (Rasch 1960, xix)

THE FRIEND

One day in November 1959 Jimmie Savage asked me whether I knew a Dane named Rasch. He had encountered Rasch at a biostatistics conference in Washington. Drawing on a 1947 association in Chicago, Rasch had pressed for a return visit. He had a new way to construct objective mental measurements. Jimmie had some money for a visiting professor. If he invites Rasch, will I guarantee students? Having no control over students, I guaranteed myself.

Georg began his lectures in March 1960. At first they are jammed — most of the statistics department, quite a few social scientists, even some students. Georg is bold, dramatic, and uncompromising. He is also enthusiastically forthright about the futility of many traditional procedures. Unfortunately the statisticians are not interested in changing their ways and the social scientists find it "too mathematical." By three weeks only one "student" remains.

Nevertheless, Georg marches in each morning, sets up his notes, grasps the lectern and delivers a lecture. Then he

scans the room, focuses on his one student, steps off the platform and squeezes into the seat beside me to answer all my questions.

But it is lunchtime. In order not to interrupt our discourse, Georg invites me to his kitchen where, while continuing our animated discussion, we mash sardines into black bread with plenty of oil and black pepper and wash them down with Danish beer.

Ben Wright and Georg Rasch in Athens, Georgia, April 1973.

Why did I stay with Georg when my students and colleagues departed? Was it my promise to Jimmie? Was it my compassion for Georg? Of course. But the clincher was a dawning realization that Georg had discovered a practical solution to the most stubborn and seemingly insurmountable obstacle to any real social science, the almost complete absence of reproducible measures.

Later, as we became comfortable, I dared to tell Georg about my disappointments with the instability of the many factor analyses I had performed. Georg told me about his 1953 article on this very problem. The danger in factor analysis is that it seldom reproduces its results. But only when it can be demonstrated to have done so can it serve as a useful scientific method.

Intrigued by my failed attempts to control semantic differential data with factor analysis, Georg insists on taking a look at my data. Always ready for a new problem, he sits right down and begins to do some quick calculations and to draw a few rough plots. Then he writes out a "Rasch" model for rating scales and we try to apply it to my data by hand. It is May 1960.

Georg's new model makes its public debut in his June 1960 Berkeley Symposium talk and travels home to Denmark to become the basis for Erling Andersen's education. We never finish applying it by hand but after I spend the spring of 1964 and, then with Bruce Choppin, the summer of 1965 in Copenhagen with Georg the new model finally gets applied to my semantic differential data through a pairwise FORTRAN algorithm, "BIGPAR," written by Bruce in the fall of 1965.

The day after my family and I arrived in Copenhagen in May 1964, I went to Georg's Institute about 11am. He was very happy to see me, showed me around quickly and hurried me off to lunch at his "nearest favorite restaurant," The Little Prince, where Georg was very well known to the management. Course by course, the proprietor brought us samples of every kind of dish imaginable. In Denmark they call this the "Alretning" which I believe means "everything in the kitchen." Georg encouraged me with "the advice the wise old Chinaman gave his son. If one eats slowly there is no limit to how much one can eat."

So we ate slowly and for hours. Frequently in the course of our infinite banquet we stopped religiously to toast one another and slake our thirst. This was especially important when eating herrings on black bread smeared with lard — a Danish delicacy.

After each bite it was de rigueur to look directly into one another's eyes, raise our glasses toward each other, emit a hearty "Skol" and down the 2 ounces of Akvavit in a gulp. This was necessary so that "the herring could swim." Two ounces of liquid, however, almost always proved insufficient to keep the herrings happy. So we usually followed the Akvavit with a half bottle of good Danish beer "to keep those herrings swimming."

Later, as we moved on from fish to beef, we shifted naturally to a "nice red wine" which kept us and I suppose the herrings swimming through meat and cheese but had to yield to an even "nicer white wine" to float fruits and desserts which in their turn must be saluted farewell with some "fine cognac." The proprietor who had been with us off and on all afternoon finally sat down with us at about 3:30 to help smoke a rich cigar and sip "very old Madeira." Georg apologized that he himself had never learned to smoke. But he assured us that his dear wife Nille did smoke and especially liked cigars.

Most of the time we did not meet at his Institute. Instead I took a perfect commuter train out to suburban Holte where he lived in a handsome mansion of many large rooms. Our mathematical work, however, took place upstairs in a rather small bedroom because that was the only place in the house where Nille had allowed Georg to install a blackboard. And without a blackboard, Georg could not work at all.

Georg had a regular round of consultations at various research institutes: The Army, The Serum Institute, The Eugenics Society, and Erik Thomsen's Institute for Educational Research which published Georg's great book.

These consultations usually took place after lunch. Georg would introduce me to everyone there, settle down in the big chair and invite the young men attending to report their progress with the measurement research they were doing under his direction. Once they got started Georg's eyes would fall shut and it would look for all the world like he was definitely sound asleep. Not at all surprising considering what we had had for lunch. This usually embarrassed the host who would hasten to my side and whisper into my ear that Georg was not really asleep. And perhaps not. For when the reports where done and the voices of the young reporters faded away, Georg would shake himself, open his eyes, tell them in detail exactly what to do next and rush us off to the next consultation.

When Georg and Nille gave us a dinner party out in Holte. Georg met each guest at the door, asked them what they would like to drink, and then, whatever they asked for, be it sherry, whiskey, vermouth or a dry martini, always had their first drink with them. He had a vodka martini with Claire and then a Bourbon whiskey with me.

At the dinner table a large bottle of red wine was put between every lady and gentleman so that the gentleman could keep the lady's glass full without inflicting upon her the embarrassment of asking for more. Throughout the many courses, whenever a guest caught the eye of another anywhere around the table, each grasped their wine glass firmly, raised it high, invoked a hearty "Skol" and finished the glass. As far as I can remember it was a lovely evening. I believe that was the evening Nille taught me to whisper endearments in Danish into the inviting ears of her beautiful daughters.

My subsequent gastronomical adventures with Georg never fell short of our first lunch. On Laesoe in August 1967 where I spent a month in his 200-year-old thatched roof farm house, we began each day by cooking a fine English breakfast which we served to Nille on a tray in her bedroom and then downed ourselves in the little dining room that looked out on the yard.

Then Georg would take me back to his office/bedroom, "created out of the former pigpen of the farm house" where one wall was a large blackboard. There we would spend 3 or 4 hours working on the mathematics and implications of his measurement models and would just be getting really serious when the sound of clinking glasses would drift down the garden path toward our mathematical sanctuary. It was Nille with a choice of cocktails before lunch.

Whenever possible lunch was in the garden and it was always fulsome: herrings, cheeses, cold meats and salads, and, of course, the essential Akvavit and beer to help it down. Needless to say, after lunch we all napped or perhaps "passed out" would be a better description.

About 3pm Georg would push his head through the small window just above the bed in my tiny guest room, look fiercely down upon my unconscious form and shout, "BOO!!" That was when we submitted ourselves to Nille's devotion to race car driving and surged out to explore the island. Georg always sat in back, clutching the dog, "just in case." We careened around the narrow lanes of the little flat island to visit Nille's many island conquests, the fishing folk who lived on the island for whom Nille was the grandest of urban ladies.

We usually took a large box of candy to the island "Fat Lady," so fat in fact that she had not been able to squeeze through a door or window of the room she inhabited for decades. The "Fat Lady" held court every afternoon, listened to and resolved family and financial disagreements, and told fortunes. The grateful islanders never failed to bring her a few more pieces of candy.

When the weather was warm we went to Danzigmann beach, a sandy peninsula jutting out into the Kattegat toward Sweden. We changed into our bathing suits in front of one another without the least self-consciousness. Georg, who was then 66, set off on his "traditional" run way down the beach and back and then we threw ourselves into the 50-degree water for a brief and extraordinarily invigorating "swim." Georg usually did more of that than I did. Nille took the sun. And then of course we had a "bite to eat" which often took the better part of an hour to complete.

In the evening a local lady referred to by Nille as "Mrs. Laeso" served by candlelight the sumptuous banquet that Nille had somehow gathered and supervised during the morning and perhaps when Georg and I were napping.

There were many courses and several wines. Often there was amazing, "just caught today," fish, virgin lobsters, and crabs which Nille had collected from her fishing friends down at the docks. As the evening darkened we talked about old times, their childhoods, their young marriage, the hardships of the 1930's and the war. Often as Georg remembered a particular time or moment he wept with the joy and sadness of it.

After the long meal we usually went into the next room, put an Italian or French opera on the phonograph, sipped cognac and/or Madeira, and Nille and I smoked our cigars.

Once in a while we drove out into the Laesoe night to visit a party at a fisherman's thatched cottage. The light was by candle as no electricity had as yet reached these cottages. The music was homemade and the dancing lively, much like our American folk dancing. Most of the dancing was done by the women, as the men seemed cautious about becoming involved in anything so impulsive. Nille sported about the room arousing excited, happy conversation with the ladies, introducing me each time, and then getting me to dance with each lady in turn and also having a dance or two herself. All the while Georg would sit contentedly in a comfortable corner sipping beer. "On occasions like these, I only get involved at the highest diplomatic level."

Most nights before we finally retired we took Nille's dog for a walk down the country road beyond the cottage. Sometimes it was pitch black, sometimes bright moonlight. We held hands and talked and laughed as we walked.

I worked and played with Georg for 20 years. He was

always happy, optimistic, full of fun, ready for anything. He loved puns and knew countless anecdotes of endearing human foibles. He was generous, wise, infinitely forgiving, and the most modest genius I have ever met.

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[1] The quotations come from David Andrich's interviews with Rasch on Laesoe June 1979 and Rasch's February 1980 letter to me.