Anthony C. Atkinson and Barbara Bogacka
A conversation with Professor Tadeusz Caliński

Article (Published version) (Refereed)

Statistical Science
2015, Vol. 30, No. 3, 423–442
DOI: 10.1214/15-STS522

A Conversation with Professor Tadeusz Caliński

Anthony C. Atkinson and Barbara Bogacka

Abstract. Tadeusz Caliński was born in Poznań, Poland in 1928. Despite the absence of formal secondary eduction for Poles during the Second World War, he entered the University of Poznań in 1948, initially studying agronomy and in later years mathematics. From 1953 to 1988 he taught statistics, biometry and experimental design at the Agricultural University of Poznań. During this period he founded and developed the Poznań inter-university school of mathematical statistics and biometry, which has become one of the most important schools of this type in Poland and beyond. He has supervised 24 Ph.D. students, many of whom are currently professors at a variety of universities. He is now Professor Emeritus.

Among many awards, in 1995 Professor Caliński received the Order of Polonia Restituta for his outstanding achievements in the fields of Education and Science. In 2012 the Polish Statistical Society awarded him The Jerzy Spława-Neyman Medal for his contribution to the development of research in statistics in Poland. Professor Caliński in addition has Doctoral Degrees *honoris causa* from the Agricultural University of Poznań and the Warsaw University of Life Sciences.

His research interests include mathematical statistics and biometry, with applications to agriculture, natural sciences, biology and genetics. He has published over 140 articles in scientific journals as well as, with Sanpei Kageyama, two important books on the randomization approach to the design and analysis of experiments.

He has been extremely active and successful in initiating and contributing to fruitful international research cooperation between Polish statisticians and biometricians and their colleagues in various countries, particularly in the Netherlands, France, Italy, Great Britain, Germany, Japan and Portugal. The conversations in addition cover the history of biometry and experimental design in Poland and the early influence of British statisticians.

Key words and phrases: Agricultural field trial, British Council, building a department of statistics, communism, design of experiments, international collaboration, LINSTAT, Polish biometric school, Poznań, randomization theory of experimental design, Rothamsted, Solidarność.

The conversation took place at the Poznań University of Life Sciences on 28.12.2012, 30.12.2013, 16.04.2014 and 15.07.2014.

Anthony C. Atkinson is Emeritus Professor, Department of Statistics, London School of Economics, London WC2A 2AE, United Kingdom e-mail: a.c.atkinson@lse.ac.uk. Barbara Bogacka is Reader in Statistics, School of Mathematical Sciences, Queen Mary University of London, London E1 4NS, United Kingdom e-mail: b.bogacka@qmul.ac.uk.

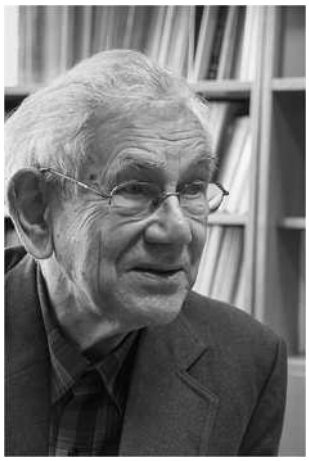

FIG. 1. *Tadeusz Caliński, during the conversation (2014).*

FIG. 2. *Stefan Barbacki (1903–1979).*

1. SCHOOL AND UNIVERSITY EDUCATION

ACA: You were born in 1928 so you should still have been at school during World War II. However, under the German occupation, education was in German and only at a very basic level, so you did not go to school. Let us start in 1945 when you resumed your education in a Polish school and started to make choices that would affect your career.

TC: The fighting in Poznań finished on 23 of February, 1945. Immediately, some schools were trying to open to start the education of Polish young people. On the first of March the Karol Marcinkowski Gymnasium and Lyceum opened and I passed the entry examination for the first class of the gymnasium.

BB: But were there some underground schools in Poznań during the war?

TC: Not many. I was lucky that my aunt was a teacher and after work (in a medical equipment shop) I went to her flat where she taught me. Before the war I had finished five classes out of six of primary school. The sixth I learned with my aunt and I was admitted to the first class of the gymnasium when the war finished. But because we were so behind, we did two years in one, which was quite common in those times. So in 1948 I got the matura (secondary-school certificate). Although I liked mathematics, I was persuaded by our Latin teacher to major in humanities.

I wanted to study forestry at the Faculty of Agronomy and Forestry of Poznań University, perhaps due to the influence of my uncle, a forester. However, at that time, young people from the so-called proletariat were accepted first, with extra points. Although I passed the entry examination, I was not accepted because at that time my father had a private business, so I lost points. But I was admitted in October 1948 to study agronomy.

BB: May we ask what your parents were doing before the war; what kind of profession?

TC: My father was a business representative for companies producing cloth materials, and my mother was a qualified nurse. They were not particularly interested in mathematics, but some relatives of my maternal grandfather had mathematical talents. Also, one of his cousins was an astronomer.

At that time the degree was in two parts: three and a half years for an engineer and another year and a half for a master's degree. In February 1952 I was awarded the Engineering Diploma. I then chose to study for the master's degree, under Professor Stefan Barbacki.

I received the master's degree in September 1953, although I had been working since February 1953. This happened because, in my final year, Professor Barbacki gave an oral examination for his course on designing experiments and their analysis. We were all together in one room. One question was "What is an interaction?" There was a long silence and then I tried to explain how I understood the term. Professor seemed to be satisfied and after the examination he asked me "Would you like to work at my department?" I was very much astonished and do not

This is an electronic reprint of the original article published by the Institute of Mathematical Statistics in *Statistical Science*, 2015, Vol. 30, No. 3, 423–442. This reprint differs from the original in pagination and typographic detail.

remember exactly how I answered, but he offered me a job in his Department of Genetics and Plant Breeding.

2. EARLY CAREER AND THE BEGINNINGS OF POLISH SCHOOL OF BIOMETRY

TC: The job was in the Institute of Husbandry, Fertilization and Soil Science (Instytut Uprawy Nawożenia i Gleboznawstwa, IUNG) at a new experimental station in Baborówko, not far from Poznań. I was employed by Professor Barbacki to help in conducting his field experiments. This was my first professional experience.

BB: What kind of help? Did you have to go to the fields themselves?

TC: Yes, I worked there in the fields, although my first contact with field experiments was during my master's degree at another experimental research station in the Poznań region and my master's thesis was based on a field experiment. So, I had already learned something on how to conduct such experiments.

BB: In your master's degree, did you do some analysis of variance?

TC: Yes. Barbacki ran a lot of experiments and I was involved in some statistical analysis, working, of course, not at a computer. I had access to an electro-mechanical calculator, rather like the one in the famous photograph of Fisher. From 1951, the faculties of Agronomy and Forestry were separate from the University and became an independent university level Higher School of Agriculture (Wyższa Szkoła Rolnicza, WSR). So, I started my study at Poznań University but finished it at WSR.

BB: In Poland, the need for proper statistical analyses of agricultural trials was already advocated as early as 1906 by Edmund Załęski in his lecture on comparative experiments with different varieties of sugar beet (Załęski, 1907). How did his work influence the research of such people as Barbacki and Neyman? Barbacki's book on the General Methodology of Field Experiments in Outline was published in the same year [Barbacki (1935), in Polish] as the famous book of Fisher on the Design of Experiments (Fisher, 1935).

TC: Załęski's lecture was published by the plant breeding company of K. Buszczyński and M. Łążyński, at which, from 1904, he was the head of research and plant breeding work. The paper is regarded as the first systematic lecture on the methodology of agricultural experiments with some application of probability theory. It had three editions

FIG. 3. *Edmund Załęski (1863–1932).*

published in five languages. His ideas were elaborated, from a mathematical point of view, by Jerzy Spława-Neyman, whereas the practical applications were introduced by several plant breeders, particularly Barbacki. In fact, in Poland, earlier than in other European countries, new methods of improving the precision of agricultural experiments were promoted.

BB: And how did the other schools, particularly of course British, influence what was going on in Poland? For example, during 1935–1936 Barbacki had a stipend from the Rockefeller Foundation to visit Fisher in London, which led to a trenchantly titled joint paper (Barbacki and Fisher, 1936) on the undesirability of systematic arrangements.

TC: Scientific relations between the British and Polish school started in fact earlier. In September 1925 Neyman visited University College, London; see Reid (1982). A result of his time there was the fundamental paper Neyman and Pearson (1928).

FIG. 4. *Jerzy Spława Neyman (1894–1981).*

BB: After Barbacki's return from England he founded and started to edit the Review of Agricultural Experimentation (Przegląd Doświadczalnictwa Rolniczego), a monthly journal popularizing statistical methods in agriculture. So, things seemed to be going well in the 1930s, at least statistically.

TC: Yes, apart from Barbacki's book, Neyman, in particular, published several papers on agricultural experimentation (Neyman, 1932, 1934). However, the most important was Neyman, Iwaszkiewicz and Kołodziejczyk (1935) presented on March 28th, 1935, at a meeting of the Royal Statistical Society.

Two events at the end of the 1930s were important for Barbacki. In 1938 the procedure for his habilitation started at the Central College of Agriculture (Szkoła Główna Gospodarstwa Wiejskiego) in Warsaw. In 1939 his next book, "The Analysis of Variance in Problems of Agricultural Experimentation," was published. Unfortunately, the whole edition of the book was destroyed in September 1939 by the war events.

BB: On September 1st, 1939, the Nazis invaded Poland. Soon universities were closed and books burned. What is your understanding of how it was in Poznań?

TC: Poznań was occupied by Germans from September 1939 to February 1945. Poznań University, as all other Polish universities, was closed by the invaders almost immediately. Many professors and other scientific employees were arrested and put in the Poznań extermination camp "Konzentrationslager Fort VII Posen." Some others were imprisoned or sent to various forced labor places in Germany. Most of the remaining were resettled to the General Gouvernement (the Polish territory occupied but not annexed to the State of Germany). Some of those were then active in the underground University of the Western Lands (Uniwersytet Ziem Zachodnich), started in 1940 in Warsaw.

Poznań University reopened in early 1945, but with serious losses among its teaching and research staff. The faculty of Agronomy and Forestry itself lost 12 scientific workers, including 5 professors.

BB: Was Barbacki able to continue his scientific and agricultural work?

TC: From 1925 to early 1945 Barbacki worked at the State Research Institute of Rural Husbandry (Państwowy Instytut Naukowy Gospodarstwa Wiejskiego) in Puławy near Lublin. During the German occupation this place was within the General Gouvernement. Fortunately, the institute was not closed and continued its research work, of course under German administration.

BB: In 2013 there was the 50th anniversary of the Department of Mathematical and Statistical Methods at the Poznań University of Life Sciences. Barbacki was the founder of this department, briefly run by Regina Elandt in the early sixties, but it was you who made it grow and flourish. What were the beginnings of your work there?

TC: When working with Barbacki from 1953 I found that there is something of mathematics in the statistical analysis of experimental designs and data. Barbacki told me that if I am so interested in statistical methods, I should learn more mathematics and undertake some study of mathematics at the university. Fortunately for me, Professor Orlicz, who was the Head of the Department of Mathematics at Poznań University, was a friend of Barbacki, and they both prepared a special program for me.

BB: It means you had an individual program of study?

TC: Yes, I had such a program for three years, but it was not for a degree. They decided which lectures I should attend, which examinations I should sit. This study was not full time, as I was also working at Barbacki's department where I was associated with its section of Experimental Design and Biometry.

BB: How was it to work with Barbacki?

TC: He was always very serious. He was the kind of person who looked after the development of his people and he was very much interested in what they were doing in science. He gave them appreciable freedom in their research. He was not "conducting" them, rather he was interested in what they were doing.

BB: This is probably the best way of supervising.

TC: Yes. He was also very much interested in people's wellbeing, that they had enough money to support themselves. The salaries were low and he tried to find extra jobs for his assistants; I had an extra part time position at the Department of Plant Genetics, Polish Academy of Sciences.

After World War I there were two particularly important agricultural research institutes in Poland. One was in Bydgoszcz, where Neyman started his research work; the other was in Puławy, with which Barbacki was associated. It might be interesting to note that this institute started in 1862 in the Russian part of the partition of Poland.

ACA: Was Załęski involved in this institute?

FIG. 5. *Władysław Orlicz (1903–1990) at his honorary degree ceremony, Adam Mickiewicz University, Poznań.*

TC: No. Before WWI Załęski was a professor at the Agricultural Academy in Dublany, one of the first Polish-language schools of this kind, founded in 1858. It was near Lwów (now Lviv in Ukraine), at that time within the Polish area under the Austrian Empire. Załęski made enormous and innovatory methodological achievements which he then compiled in his textbook Załęski (1927).

BB: And he was the supervisor of Barbacki?

TC: Yes, after WWI Załęski moved to Cracow and became a professor at the Jagiellonian University, where Barbacki was a student in the years 1921–1925. At the end of Barbacki's study he became Załęski's assistant. Then, after receiving his master's degree, he obtained a job at the Puławy Institute. In 1929 he received his Ph.D. in agricultural sciences bestowed by the Jagiellonian University.

Because of the war his habilitation at the Agricultural University in Warsaw was broken. After the war he was habilitated at Poznań University in 1945. There he organized the Department of Experimental Design and Biometry—the first of this type in Poland. In 1951, when the Faculties of Agronomy and Forestry had become the above-mentioned WSR, he ran the Department of Genetics and Plant Breeding.

3. VISITS TO THE UNITED KINGDOM

ACA: Could you tell us something about your visits to the UK and their effect on your scientific development?

TC: My Ph.D. thesis in 1961 was "On the Application of Analysis of Variance to the Results of a Series of Varietal Experiments." My supervisor, Barbacki, then decided that I should spend some time abroad. Actually, my first travel abroad was in 1960 for a mathematical conference in Budapest. Regina Elandt (later Elandt-Johnson) had been invited, but she was already traveling and suggested that I take part in that conference instead of her.

In 1964, due to Barbacki, I was awarded a scholarship by the Polish government to go abroad. I became an honorary research assistant in the Department of Statistics at University College, London, where Professor Maurice S. Bartlett was the Head of the Department. I think Norman Johnson helped me in this, as I knew him from his visits to Poznań to see Regina Elandt.

I arrived to London in January 1964—almost in time to attend the wedding of Regina with Norman.

BB: Why "almost"?

TC: I was to be there on the first of January, and their wedding was on the 4th or 5th. But, as so often in Poland at that time, there was a delay with my passport and I missed the wedding. I arrived to London on the 10th.

Professor Bartlett was my supervisor there. I got a shared room in the department. I could attend lectures and had time to do research. One of the courses I attended was given by David Cox. It was for the "evening students" at Birkbeck College. The lectures were on the experimental design, very important for me.

FIG. 6. *T. Caliński and G. Ross in Rothamsted, 50 years after Caliński's first visit there.*

After some time Professor Bartlett told me "as you are from an agricultural university you should be interested in visiting the Rothamsted Experimental Station." He suggested that I write to Frank Yates. So, I wrote asking if it would be possible to spend some time in Rothamsted. At that time, the English habit and the Post Office working style were such that I got the reply immediately, inviting me for an interview. It was very interesting, but the interview was rather difficult for me. It was difficult because Yates had a habit of keeping his pipe in his mouth and it was hard for me to understand what he was saying. Fortunately, Michael Healy was sitting next to him and he was translating the Yates English to the BBC English.

The discussion was very interesting. He asked me what were my interests in statistics. I did not know his actual interests. Unfortunately, I said I was interested in mixed effects models. Then he asked, "mixed models, in which sense?" I made it even worse because I replied "in the Scheffé sense." He became very unsatisfied because there was some conflict between him and Scheffé, perhaps coming from an earlier meeting of the Royal Statistical Society. Anyway, at the end of this interview he told me "Yes, you can stay here as long as you wish, but remember, never mention the name of Henry Scheffé." So, their conflict was quite deep. I started in April and stayed for half a year there.

I had accommodation in the Manor House of Rothamsted. This was excellent for me. The price was low, good food was served, very nice place itself.

BB: Did you work there with some people?

TC: My supervisor was Desmond Patterson. I was preparing my habilitation "On the Distributions of the F-type Statistics in the Analysis of a Group of Experiments."

ACA: Could you say something about this research? You were using both the exact and approximate distributional results of Box (1954a, 1954b), so it wasn't about design.

TC: Yes, the research concerned not designing but analyzing a group of experiments (e.g., with crop varieties) conducted at different places (environments). The main problem was the influence of the heterogeneity of the error variance and of the interaction variance on the distribution of the F-test statistics used in this type of analysis. Some of Box's results were utilized in constructing approximate tests. The discrepancies between the approximate and the exact significance levels for the F-tests applied to experiments with heterogeneous variances were investigated employing the Rothamsted electronic computer.

ACA: And how did you spend your free time there?

TC: People at Rothamsted were very kind to foreign visitors; I was very often invited for private visits during weekends, sometimes by Polish people who lived in Britain. But most frequently I was invited to their home by Rosemary and Gavin Ross who were then a young couple, two years after their wedding. Gavin was in the Statistics section of Rothamsted, with a particular interest in computing and simulation, and he helped with my habilitation and with Caliński (1966).

There was also a Jewish lady, Miss Blanche Benzian, who often invited me to visit her. What was really peculiar, that in London the only people from the University staff who invited me to their homes were Jewish. Although all members of the Department of Statistics were very friendly, such private invitations I was receiving exclusively from English people of Jewish origin. At Rothamsted it was different, though due to the hospitality of Blanche Benzian I had the interesting occasion to meet also some of her Jewish friends.

In September I had some holiday and I decided to go to Scotland. There were two reasons that I wanted to travel there. One was that I wanted to visit Professor David Finney. I knew him well from the literature and I was very interested to meet him personally. As to the second reason, there were some touring offers of British Council for foreign students, and I booked a trip along the Scottish Western Islands. After the visit to Western Scotland I went to Aberdeen and Professor Finney presented me to his colleagues.

ACA: Did you attend any lectures or seminars there? I remember, when he was at Edinburgh, Finney organized the seminars to be at, I think, five o'clock on Friday, to avoid his colleagues having extended weekends.

You thank Finney at the end of Caliński (1966). Did you have any technical discussion with him about experimental design? By the time you met he had already published his book on design (Finney, 1955) and was in Aberdeen to help establish a "Rothamsted" in Scotland.

TC: My discussions with David Finney, also with Desmond Patterson, concerned mainly general problems that appear in conducting and analyzing series of experiments, variety trials in particular.

BB: Walking in the mountains has always been your great hobby. Did you have a chance to do some walking in the Scottish mountains?

TC: One of David Finney's colleagues was also Polish. He had a camper van and we spent the weekend in the Cairngorm Mountains. We made walks in the mountains, including mushroom picking, an activity not very common among Scottish people. I also visited the Highland Games at Braemar. This was a great opportunity for me, as it was the first time in my life that I could see the Queen and her family.

BB: Let us come back to your visit to the UK. From Scotland you came back to Rothamsted?

TC: Yes, then I came back to Rothamsted and I had another opportunity of meeting people. It happened that William Cochran, the author of a book on experimental design with Gertrude Cox (Cochran and Cox, 1957), was visiting Rothamsted at that time and also stayed in the Manor House. One evening he told me about his experiences in Rothamsted before the last war. He was working then with Frank Yates, with whom he published several joint papers. Really, it was a great pleasure for me to meet such an important person.

Of course, being in Britain I had the opportunity to meet also some other important persons. Particularly, I would like to mention John A. Nelder, whom I had the chance to visit in the National Vegetable Research Station at Wellesbourne, on a trip from Rothamsted.

So it was very interesting for me to be in Rothamsted. Then I returned for the last three months to London and continued my work there.

ACA: Did you talk to Bartlett? He was a very quiet man.

TC: Yes, but he was very friendly. Although he was extremely busy, he was able to read my habilitation thesis and he gave me some suggestions. I remember when I asked him to read it, he told me, sitting at his desk, "Do you see how many papers I have to read? But as you are leaving soon, I will find one evening to read it."

For the last month of my stay in London my wife visited me and we spent December together. We had a week vacation organized by the British Council and we took this opportunity to spend the week at the seashore, at Broadstairs in Kent.

ACA: In December?

TC: Yes, again, there were many foreigners taking part in this event since it was specially organized for foreign students. We also had some friends in London, Polish people. So we spent Christmas Day

FIG. 7. *During one of the Wisła conferences (1995).*

(December 25th, not 24th!) with them. Then we returned home, via Paris. But I decided that I should go to Britain again, because I liked the country very much. Due to the help of Professor Finney, I got a British Council Scholarship for three months in 1969.

This time I visited the University of Edinburgh (Finney's Department of Statistics), University College Aberystwyth in Wales, and finally East Malling Research Station in Kent. There I started a very fruitful collaboration with Clifford Pearce, leading to a paper in which we introduced the notion of basic contrasts (Pearce, Caliński and Marshall, 1974).

BB: This work was a continuation of your previous results (Caliński, 1971), which led to establishing a class of experimental designs called "C-designs." Were these the beginnings of your interest in statistical properties of block designs?

TC: Yes, you could say so. Following these papers, several further concepts and results concerning block designs were presented in publications, for example, Caliński (1993) and the joint papers on the randomization model for experiments in block designs, Caliński and Kageyama (1996a, 1996b).

ACA: Could you say something about the relationship of this work to that of John Nelder on general balance?

TC: The joint work with Sanpei Kageyama followed some concepts of Nelder, particularly those presented in Nelder (1954) and Nelder (1965a, 1965b). Our first use of these results was in Caliński and Kageyama (1991).

During my second stay in Britain I was very much interested in attending the 37th Session of ISI in London in 1969, but this required an extension of my visit; the British Council not only extended my visit for two weeks, but paid for it as well. I was able to attend (for the first time) the ISI session. One interest in this meeting was that I wanted to see famous people whom I knew from the literature, particularly Anderson and Scheffé.

ACA: That was Ted Anderson?

TC: Yes, T. W. Anderson, the author of the book on multivariate statistical analysis (Anderson, 1984). Some of the famous people I had already met, but it was impossible for me to meet those from the United States. I asked somebody how do I recognize Henry Scheffé? I was particularly under the influence of his book on the analysis of variance. Somebody told me "if you see the most ugly looking person, this will be Scheffé." This is so because he was a boxer or wrestler and had a broken nose. So, finally, I had the opportunity to see Henry Scheffé.

ACA: Did he live up to the description?

TC: No, not so bad. During the meeting we also had some opportunity to visit several places, such as certain London palaces. So it was interesting scientifically but also very pleasant socially.

4. BUILDING THE DEPARTMENT OF STATISTICS IN POZNAŃ.

BB: Meeting and listening to the talks by all these famous people, did they somehow influence your work?

TC: Since we wanted to learn multivariate analysis, when I returned from Britain in 1965, we started a weekly seminar in Poznań based on the book by Anderson. Everybody had to present one of the chapters, and in addition we had to solve and present the exercises. The seminar was attended not only by people from our department, but also Poznań University (now called Adam Mickiewicz University—AMU) and from other institutes.

At the same time Witold Klonecki was giving the first course in Poznań on Mathematical Statistics at the Department of Mathematics, AMU. He also ran a seminar for master's students. However, in 1966, Klonecki was invited by Jerzy Neyman to go to Berkeley. He was so fascinated by this that he stayed for two or three years. As a result, the course in Mathematical Statistics ceased, at a time when I was very much interested to get new people to develop the department.

Although the standard of theoretical mathematics was very high at AMU, Professor Orlicz wanted the Poznań School of Mathematics to develop in the direction of applications. In Klonecki's absence I asked Professor Orlicz to instruct one of his co-workers to continue the seminar and the course. However, Orlicz looked at me and said, "You will do it!" So I started in September 1966. Orlicz told me, "You will give the course and prepare all the lectures." Also, he suggested that if there were master's students interested in statistics I should take care of them, that is, I should be supervising their master's theses.

BB: (reading the list) Altogether you supervised 53 master's degree students! I see the last one is Paweł Krajewski in 1980.

TC: Some of these students got jobs at the AMU or the Technical University in Poznań, but many were employed in our department by what is now called the Poznań University of Life Sciences (PULS).

BB: Excellent! But why did the master's seminar stop in 1980?

TC: It did not stop. Mirosław Krzyśko was habilitated in 1977 and started a section of Probability and Mathematical Statistics within the Department of Mathematics at the AMU where the seminar continued.

BB: Let us return to the earlier years of the seminar at your department. Once you had your habilitation, you could formally supervise a Ph.D.

TC: I had got my habilitation in 1966. So, the seminar at our department, from this point of view, was very fruitful. The year 1972 was particularly rich in Ph.D. theses. Altogether I supervised 24 Ph.D. students.

BB: Five students working under your supervision defended their theses in 1972. This is quite unusual, so many students finishing in one year. How did you manage to achieve this?

TC: As I said, the first Ph.D. theses were based on Anderson's book. Such was that of Krzyśko, who received his Ph.D. in 1971, as my first Ph.D. student. The next few theses were also related to multivariate analysis.

BB: Did you continue the collaboration with Krzyśko and his new group?

TC: Certainly. We have been keeping contacts till now. In fact, our last two joint papers both concerning canonical correlations are Caliński and Krzyśko (2005), Caliński, Krzyśko and Wołyński (2006).

BB: A different effect of your supervision came out from the work of Michał Karoński. His seminar on Random Graphs is now well established and brings collaborators from all over the world. Do you know how his interest in this area started?

TC: He very early became interested in taxonomy. I was his supervisor for master's degree in 1968/1969. For this, he presented a paper on Mahalanobis distances and their applications in taxonomy. His Ph.D. thesis, presented in 1974, was on grouping multivariate populations by using a simultaneous test procedure. This encouraged him to go further, finally to random graphs.

BB: While Krzyśko and Karoński were actively developing their groups, at your department the team of Baksalary and Kala worked intensively on the theory of linear models, in particular, aspects of linear algebra. Their joint work was published in such journals as the Annals of Statistics, for example, Baksalary and Kala (1981). How did it happen that such a theoretical area was of so much interest in your department?

TC: It started from a question that was raised at our seminar: how to find bases of a matrix? I suggested that perhaps somebody would elaborate a

FIG. 8. *J. Baksalary, P. Pordzik, R. Kala (1980, Photo by G. H. Styan).*

useful method for this. One or two weeks later Baksalary, and independently Kala, presented their solutions of this problem. So, I suggested that they sit down together and prepare a joint paper. In fact, in the first ten years of their collaboration (1973–1982) they published 41 joint papers in different journals, among them four in The Annals of Statistics (the first in 1976 on Milliken's estimability criterion). They both received their Ph.D.'s in 1975.

BB: Baksalary then moved to Zielona Góra, where he organized a very active group of mathematicians and statisticians.

TC: Under Baksalary's rectorate (1990–1996) of the Tadeusz Kotarbiński Pedagogical University in Zielona Góra, they started to organize scientific meetings of mathematicians for the exchange of views on various problems in linear algebra and mathematical statistics. It seems that those meetings were very fruitful. They were called "Konfrontacje Zielongórskie."

ACA: What is the meaning of "konfrontacje"? In English it is rather aggressive.

TC: No, here it is in the meaning of challenging ideas. It was a yearly meeting, often in Zielona Góra. They invited people from abroad, such as the Finn Simo Puntanen, to join the meetings. This was an initiative of Baksalary. There is a special issue of *Linear Algebra and its Applications*, **410** (2005), devoted to Jerzy Baksalary, in which a list of his coauthors is presented. There were 43 of them, including C. R. Rao.

BB: Your work has spread to many places. One of them is the Institute of Plant Genetics, Polish Academy of Sciences, where one of your Ph.D. students, Zygmunt Kaczmarek, is working. Could you tell us about the collaboration with this institute?

TC: My cooperation with this institute dates from 1956 and my close collaboration with Zygmunt Kaczmarek from 1965. We have published till now 51 joint papers (in the years 1968–2009). In 12 of these publications, one of the co-authors is Paweł Krajewski, my master's degree student and later Kaczmarek's Ph.D. student. He is now one of the most active research workers in that institute. Among those 12 joint publications, 6 concern our joint computer program SERGEN for analyzing series of variety trials and plant genetic or breeding experiments. This program has three editions and has been used by many plant breeding and variety evaluation centers in Poland and also abroad, in Great Britain in particular.

ACA: When was the first Ph.D. thesis on experimental design?

TC: Bronisław Ceranka in 1972. This was the starting point of his contribution to the theory and practice of experimental design.

BB: Then, there was another student of yours, Wiesław Pilarczyk, who worked for a long time at the Research Centre for Cultivar Testing in Słupia Wielka.

TC: I cooperated with this Research Centre (COBORU) from its foundation in 1966. One of the results of this cooperation is the series of Working Seminars on Statistical Methods in Variety Testing organized jointly with our department. Each of these seminars was attended by about twenty participants from several countries. Two very important people attended the first seminar, in 1979: Professors Leo Corsten and Desmond Patterson. Unfortunately, both of them died in 2013.

BB: Your Ph.D. students have themselves supervised many Ph.D.s over the years. These could be called your "grandchildren" and you have done joint work with some of them.

TC: From these 24 Ph.D.s we now have 12 with the title of professor. Unfortunately, two of them

FIG. 9. *First Working Seminar on Statistical Methods in Variety Testing (1979).*

have already died, Baksalary and Wagner. I believe the exact number of grandchildren is currently 61. The most important contributor to this is Michał Karoński, who promoted 11 Ph.D. students, next are Mirosław Krzyśko with 9 promoted Ph.D. students and Radosław Kala with 8 promotions. Among them, there are at least five foreigners: one each from Britain, Japan, Portugal and two from Syria. There are also several great-grandchildren.

BB: Over the years the Department of Mathematical and Statistical Methods grew considerably and now it has over 30 academic staff. But at the beginning it was rather small.

TC: The department was founded in 1963 for Regina Elandt who was so advanced in statistics [see Elandt (1964)] that Barbacki thought she should start a new department of Mathematical Statistics. However, she married Norman Johnson in January 1964 and moved to Chapel Hill in July to join him.

When I returned from Britain there were only two of us. But COBORU started in 1966 and Eugeniusz Bilski left to become their director. While collaboration with Bilski continued, I was looking for new people to join the department in Poznań. Ceranka was the first such.

BB: That was the time when you became the head of the department?

TC: Actually, at the beginning there was no head. Although I got the habilitation in 1966, I did not get the position immediately afterwards. At that time, all positions at the university required acceptance by a University unit of the communist party. Since I was not well seen by the local organization of the party, the first secretary put the documents into a drawer of his desk. I had to wait for two years before I got the position of Docent and then head of the department. The party preferred to support their members. Fortunately, there was no candidate from the party, so eventually I became the head of the department.

ACA: This was a kind of a pattern; some other people's careers were delayed as well.

TC: Yes, for example, Kala had to wait a long time for the title of Professor. His application papers, already supported by the Faculty, likewise had to go to Warsaw for approval. Kala's habilitation was in 1981, but he was involved in Solidarność and so was punished by the party. Kala only became a Professor in 1990. The eighties were the worst years in our academic (and not only) life.

BB: Even so, there was active research in the theory of experiments, the theory of estimability in linear models, multivariate analysis and matrix algebra and linear spaces. This is a wide spectrum of interests for one department.

TC: You have to remember that there was intensive cooperation with people from other academic or research institutions, to some extent under the influence of our joint weekly seminar.

5. STATISTICAL EDUCATION IN POLISH UNIVERSITIES

ACA: The recent history of Poland breaks very naturally into decades. One of your activities (I don't

FIG. 10. *Tadeusz Caliński at LINSTAT 2008.*

know how much time it took) in the 1970s was in encouraging undergraduate education in statistics. I think this was initially a completely new idea in Poland?

TC: Again, this was due to Professor Orlicz who was at that time one of the most important members of the Committee of Mathematical Sciences at the Polish Academy of Sciences. One day he told me, "I have a duty for you. I will introduce you to our committee so that you become a member, but you have to give a talk (exposé) explaining the importance of mathematical statistics. We would perhaps then find some possibility of supporting the development of the subject in Poland." I demurred, but he answered, "You have to do it!" Not accepting an "invitation" from Professor Orlicz was not an option.

So, I joined this committee and I gave an exposé on problems of mathematical statistics, urging support for its development, which was favorably received. A few months later, a commission was created for development of mathematical statistics in Poland. I became a vice-chairman; Józef Łukaszewicz was the first chairman, as he was from Wrocław, where some applications of statistics were more developed than in Poznań. Witold Klonecki was also a member.

One of the ways of developing mathematical statistics in Poland was to organize inter-university conferences. The first was in 1973 in Wisła. These conferences were a very good platform for contacts with people from various places in Poland and for inviting people from abroad; one year it was a Polish national meeting and the other year it was international. The Commission continues to work and the conferences continue to take place.

BB: Some of these international conferences were the origin of the LINSTAT series. In fact, you organized the first one of the LINSTAT series in Poznań in 1984, at quite a difficult time in Poland, briefly after Martial Law was withdrawn. I remember that time of political unrest and economical shortages, contrasted with the high-spirited atmosphere at the conference and the lively discussions in the conference room. LINSTAT became a regular conference, a European meeting on mathematical statistics, largely devoted to applications of linear algebra in statistics. The one in 2008 was to honor your 80th Birthday

TC: This was really a great pleasure for me, particularly because several of my old friends were able to participate in it; unfortunately not all of them.

Another purpose of the Commission was to introduce lectures on mathematical statistics at Polish universities, such as those Professor Krzyśko was giving in Poznań. They were also introduced in Warsaw, Wrocław and in many other universities at the departments of mathematics.

BB: What was your role in the introduction of statistics into the curriculum at the mathematics departments at Polish universities, in Poznań in particular?

TC: My role was to initiate the work, to organize the Commission's activities, and to run meetings several times a year to discuss how to support the idea of the development of mathematical statistics in Poland. I was chairman from 1975 to 1980, followed by Ryszard Zieliński (Warsaw), Mirosław Krzyśko (Poznań) and Roman Zmyślony (Zielona Góra). They and all other members contributed very much to the activity of the Commission.

BB: The document you presented in 1972 to the Committee of Mathematical Sciences includes support for international collaboration, help for libraries in the supply of international journals and books on statistics and its applications and also assistance in obtaining funds for research. Now, after more than 40 years of its work, how do you see its achievements and its role for the future?

TC: The Commission has fruitfully promoted statistical education and research in Polish universities and research institutes. Now its main activity

is fostering cooperation between groups of mathematicians interested in statistics within Poland and also with those from other countries. The organization of national and international conferences on mathematical statistics and its applications remains central. The Poznań group is quite active in this.

6. INTERNATIONAL COLLABORATION

ACA: You not only followed Regina Elandt as Head of Department, but you also followed her to North Carolina?

TC: Yes, in 1977, from January to September, I visited the National Institute of the Environmental Health Sciences (NIEHS), its Biometry Branch. I had a very good time there, initially on my own, latterly with my wife. For me being in the USA at that time in 1977 was as to be on a different planet. The system was different, even to find, for example, accommodation which, with the help of Norman Johnson, only took a few hours. Also, the telephone was arranged in one day. Everything was very easy, while in communist countries obstacles were put in every step of our everyday life. We visited all the Eastern States, from Florida to the Niagara Falls.

I was accommodated at Chapel Hill, but the Institute was in the so-called Research Triangle Park. The first institute, the Research Triangle Institute, was founded in the sixties, the Statistics Research Division being headed by Gertrude M. Cox. But when I arrived there were already more than 20 institutes or laboratories within that Park.

At the beginning I had to travel to this place, about seven miles, but there was no bus. So, a colleague was giving me a lift there every morning. Before buying a car, I attended a course for driving in Durham. Fortunately the lady who was giving the course lived in Chapel Hill, and she kindly drove me back home after the lessons. She was very interested in the fact that I was from a communist country. She asked me, "How is it possible that you can come to the United States from a communist country?" Well, I said that this was possible. Then she asked me, "Did you have any problems with the FBI?" I said, "Until now, not." Then I got an instruction from her: "If you have any problems with them, please let me know, because all of them have got their driving licences at my course." But, actually, I did not have any problems with that agency.[1]

When my wife joined me, I bought a small car, a Volkswagen, which I bought from a student for $700. Then I sold it, just before leaving, for $650. So, I had a car for about half a year for 50 dollars.

We visited various places, mainly for scientific reasons. For example, I was invited by Professor William Mendenhall to the University of Florida, in Gainesville, to give a seminar. I was also attending the Joint Statistical Meetings in Chicago, where I gave a talk. Also, for the first time in my life, I saw Professor Kempthorne. I found out that he was not only a very good scientist, but also a very good dancer. There was a conference dinner and after the dinner somebody said, "Now we have time for dancing." The first, who immediately asked a lady to dance, was Oscar Kempthorne. They were dancing and we were all looking at them, as very good dancers.

BB: During your stay there did you undertake a special research topic or collaborate with some people?

TC: In addition to Gainesville and Chicago, I gave two seminars at the University of Chapel Hill. I also continued some written cooperation with Clifford Pearce.

ACA: Did you have a delay in the mail? I collaborated with Fedorov in the Soviet Union and they put the letters in the bottom drawer for three weeks.

TC: Yes, the contact from Poland was slow and in the US the letters to and from Pearce came in a few days. I was also preparing some work for analyzing series of variety trials, in particular, in connection with variety by environment interaction. Shortly after returning to Poland, I was invited to a conference in Italy and there I presented the results obtained in North Carolina.

ACA: Before your visit to the United States, you were already involved in collaborations with scientists in other countries. How were these collaborations abroad funded? One of the features of communist states was that they were always short of "hard" currency.

TC: There were various possibilities. In the early seventies some people from our university were able to get scholarships from the Margrabina Umiastowska Foundation. This was not controlled by the government but by Polish people in exile, although somehow it was possible to use such scholarships (perhaps some people from the Ministry were also interested in traveling). Anyway, due to this foundation, I received an invitation (scholarship) in 1971 and spent one month in Italy.

[1] For other comments from a visiting statistician on driving in North Carolina see Box (2013), page 72.

This was a very busy time, visiting several people. The last was Professor Giulio Alfredo Maccacaro, head of the Department of Biometry and Medical Statistics at the University of Milan. In 1949–1950 he had worked under Fisher at Cambridge. I gave a talk on the methods of multivariate analysis in biomedicine (later published as "Metodi di analisi multivariata in biomedicina"). Afterwards Professor Maccacaro asked whether I would like to come back to give a two week course. I was very much astonished, but, of course, I gave a positive answer. So, my next stay in Italy was in Milan in May 1972.

BB: I remember, you were also visiting Italy in the later years.

TC: Several times. After the course on Multivariate Analysis which I gave there, some of the geneticists became very interested. Mirella Sari-Gorla was one of those who attended this course, also Alessandro Camussi and Ercole Ottaviano. So, we started working together and there have been several joint papers published as a result. Later Zygmunt Kaczmarek joined us and also visited Milan several times and then also Paweł Krajewski, both from the Institute of Plant Genetics, who continued this collaboration. There is now a much larger group of people, including scientists in the Netherlands, Ireland, Spain and the United States, and many joint publications have appeared, for example, the *Science* paper Kaufmann et al. (2010).

ACA: Mention of the work of Krajewski makes me realize that we haven't so far at all mentioned your work on genetics.

TC: As I have said, at the beginning of my academic career I worked at Barbacki's Department of Genetics and Plant Breeding. Also, I cooperated with the Institute of Plant Genetics of the Polish Academy of Sciences. My first participation in genetical research papers was in Barbacki et al. (1978a, 1978b). Later, some results of my cooperation with the Italian geneticists were published in Camussi et al. (1985), Sari-Gorla et al. (1997) and Caliński et al. (2000).

My visits to Italy extended over several years. In 1983 I was invited by the Italian Region of the Biometric Society to give a course of lectures in Gargnano, the northeast part of Italy, in a villa which belonged to the University of Milan. They were organizing summer courses there and I gave lectures on Multiple Comparison Methods.

Actually, I attended these summer schools several times. Gargnano, on the Lago di Garda, is historically very important because it was the last headquarters of Mussolini. After the change of power in 1944, Mussolini was arrested and held somewhere in the mountains, but the Germans were able to free him. Most of Italy was already taken by the allied forces, but the northern part was still under the German occupation. After the war this house was assigned to the University of Milan. Once, when I was leaving, somebody asked me, "Do you know whose room you have been using?" It was previously Mussolini's room!

The last visit to Italy was in 1997 for the Conference of the Italian Region of the Biometric Society, where I presented a paper based on joint work with my French colleague Michel Lejeune (Caliński and Lejeune, 1998). I was also giving lectures in the south of Italy, at the University of Bari.

BB: You also had a strong collaboration with people from Wageningen in the Netherlands?

TC: In 1970 the seventh International Biometrical Conference was in Hanover. Clifford Pearce invited me to give a talk in his session on experimental design. In that presentation I made some references to results of Leo Corsten. After my talk Leo approached me and said, "I am Leo Corsten to whom you were referring in your talk." So, that is how it started. In 1975 I visited Corsten's Department of Mathematics at the Agricultural University of Wageningen, and in 1976 Leo Corsten visited our Department in Poznań. These visits led to an exchange agreement between our universities. Rob Verdooren was the most frequent visitor who also collaborated with COBORU. As a result of this cooperation, I have a joint paper with Leo Corsten published in Biometrics (Caliński and Corsten, 1985) and Baksalary and Kala have a joint paper with Corsten in Biometrika (Baksalary, Corsten and Kala, 1978). It was a very fruitful cooperation and, of course, also it was a chance to see a bit of the Netherlands.

The collaboration reached its climax in 1994 when Corsten got the Honorary Doctorate from our University. It was an important event here. In 1993 in Poznań we had the second LINSTAT conference. In connection with it a party was organized at which I presented Corsten to our Rector. He asked me later whether we should award Corsten an honorary degree for the long-lasting cooperation.

ACA: As well as visits abroad, international conferences in Poland also provided a means of making new contacts.

FIG. 11. *Leo Corsten (1924–2013) during the Honorary Degree Ceremony, 1994.*

TC: Certainly. The ISI Session in Warsaw in 1975 was an occasion to invite several people to Poznań. Among others, C. R. Rao was able to visit us, also Parachuri R. Krishnaiah, Gavin Ross, Richard Tomassone, Clifford Pearce, Dieter Rasch and Marie Jeanne Laurent-Duhamel.

As a result of the visit of R. Tomassone, head of the Département de Biométrie, Institut National de la Recherche Agronomique (INRA), an agreement on cooperation with French biometricians was signed at the ministerial level. I visited INRA at Jouy-en-Josas in 1979, several exchange visits followed and in 1982–1990 we jointly organized six French–Polish Biometric Seminars, interchangeably in France or Poland. Some joint research papers resulted from this cooperation.

ACA: And how did you start your collaboration with Kageyama, that led to the two books Caliński and Kageyama (2000, 2003)?

TC: I first met Kageyama in 1983 when Klonecki and I visited the Indian Statistical Institute in connection with a conference in New Delhi. My invitation to him to visit Poznań initiated an exchange of visits. Due to his support, I received a research

FIG. 12. *C. R. Rao Speaking at the seminar in Poznań, 1975.*

scholarship from the Japan Society for the Promotion of Science so I could visit him for four weeks. It was a very well paid stipend and business class travel expenses were also covered. However, with such support, I had to work very hard. During this one month, I had to give seminars in seven places at various institutes, including the Catholic Nanzan University in Nagoya.

ACA: Could they be the same talks?

TC: No, no. Different talks. Well, perhaps, some were a bit repeated. Mainly, of course, the visit was connected to my joint work with Sanpey Kageyama. So most of the time I was staying at the Hiroshima University. One of my lectures was for the Nagoya Chapter of the Japan–Polish Society, where I had to say something about Poland. So I was very busy. Still we had time to work on the book.

BB: This is a book with a very broad coverage, so I imagine it required a lot of meetings and discussions. Do you still have contact with him?

TC: Yes, in his last letter he said he was planning to retire completely in 2015. He has already retired from his university, but he is still employed in a private university. In Japan, if you retire, you cannot be employed again at the same university, but you can get a part-time job at another one.

BB: The subject covered by the two books has also been actively researched in the Department by several other people. The problem of recovery of interblock information or of combining the estimators from different block strata still needs more research, although several papers were published on this issue. What is your view on the problem?

TC: I think the problem is basically solved, though some extensions and practical applications would be welcome. Another paper in this direction has been published recently (Caliński and Łacka, 2014). Further results will probably be published by others.

ACA: More recently, there has been an extensive collaboration with Portugal.

TC: In 2001 I gave a two-week course for Ph.D. students on experimental design in the Department of Mathematics at the New University of Lisbon (Universidade Nova de Lisboa). I was invited by Professor João Tiago Mexia. In fact, I was there five times, first in 2001 and the last in 2007, each time giving some courses.

In 2006 I also took part in the 13th International Conference on the Forum for Interdisciplinary Mathematics. This was in Tomar in Portugal, connected with the occasion of an Honorary Doctorate for C. R. Rao.

FIG. 13. *Sanpey Kageyama and Tadeusz Caliński.*

7. "SOLIDARNOŚĆ" AND MARTIAL LAW

ACA: In 1980 there was the rise of Solidarność. Just before martial law you became vice-rector for academic staff development and international collaboration.

TC: In 1981, for the first time, we had free election of the authorities of the University. Previously the authorities had been nominated by the Minister of Higher Education. In 1981 I was elected as a Vice-Rector for Staff Development and International Cooperation. We started to work in September of that year. But as you know, in December 1981 Martial Law was introduced in Poland. Then, at a meeting of the Rector and Vice-Rectors, with the first secretary of the party organization and a representative of the martial authorities, a military officer, we were told that according to their decision the Rector had to resign. They thought that we would still work. Then I said, "We were elected as vice-rectors to this particular rector, Professor Wojciech Dzięciołowski, and if he has to resign, we also resign." So we all did. Thus, our work was very short, from September until the beginning of January. We took part in the ceremony of opening the new academic year in October, which was very impressive, but our work soon stopped. Then, quickly, other people were appointed to these positions.

However, I was still a member of the University Senate and I was still attending its meetings. At one of these somebody from the Ministry of Higher Education was present and he told me that they were dissatisfied that I resigned. This person was responsible for international contacts and he secretly told me that he would help me to continue my cooperation with France since our collaboration was part of an agreement with France at the ministerial level.

Therefore, despite Martial Law, they allowed me to go to France in 1982, to take part in the COMPSTAT Symposium in Toulouse, where I gave an invited talk on some problems in analyzing nonorthogonal designs. It was astonishing that I got a passport, as most people were not allowed to go abroad at that time. But it was due to the support of the Ministry.

Later, we had the first French–Polish Biometric Seminar in Rennes, which had already been accepted by the Ministry. So, I and, this time, my colleagues as well were allowed to go to France, despite the Martial Law in Poland. I traveled to the meeting in Toulouse by car together with my wife, who also got a passport. On the way we stopped at a car park somewhere in Germany, and somebody noted that we were from Poland. He turned to others of his company and said with a smile, "Look, this is a brother of Wałęsa."

ACA: But, in general, travel abroad for extended periods must have been very difficult in the 1980s.

TC: Yes, it was quite difficult. Everything was under the control of the first secretary. For example, in 1985 I got an invitation to Canada for half a year to give a course there. Although Martial Law was over by then, I did not get a passport. The Rector of our university had a letter from the Ministry of Higher Education refusing me permission to visit Canada due to "negative environmental opinion." So I was not allowed to travel abroad for the whole year, even to socialist countries.

However, as I have said, we had a cooperation programme between the Ministry of Agriculture of France and our Ministry. I was responsible for the section of Biometry. Some people in our Ministry were very much interested to continue this cooperation. So, they supported me and at the beginning of 1986 I was able to go to France for half a year. But when I went to the passport office to get my passport somebody was standing behind me and when my passport was put on the desk, that person took it and asked me to go to the back of the office for an "interview." He was from the secret police and said to me, "We finally decided that you can go." He also asked, "How is it possible that there are 32 people in your department and nobody belongs to the party?" I said, "I am not a person who instructs my staff about politics. You should ask this question to the first secretary of the University's division of the party. Perhaps they do not make proper propaganda." Then he told me that when I am abroad I should not contact those Polish people who are against the Polish Democratic Republic and so on. But this kind of instruction was quite common. The same instructions which I got before going to Britain.

ACA: Did anybody ask you questions when you came back?

TC: Usually, when we had instructions before leaving, they would say, "Remember when you are back home please contact us." But I never did.

8. LOOKING BACK, LOOKING FORWARD

ACA: You retired in 1988. But since then you have continued to be very much involved in statistical research and collaboration. Looking at the citations of your publications, the paper Caliński and Harabasz (1974) is much more highly cited than any other of your papers. What is it about, and how did it strike the jackpot?

TC: Our paper was included in a comparison of 30 procedures for determining the number of clusters in a data set. Our procedure provided excellent results, taking the first place among all examined. These results were published by Milligan and Cooper (1985). Our procedure was then incorporated into the SAS package.

ACA: Thinking back over your scientific work, which publications gave you most satisfaction?

TC: First of all, the *Biometrics* paper with discussion, Caliński (1971). It makes some suggestions for constructing useful block designs that are simple in analysis and optimal in some sense for practical application. These suggestions have been then advocated by many authors, starting from Saha (1976). More recently, in the book by Raghavarao and Padgett (2005), Section 2.9 is devoted to these patterns. Apart from this, the various papers published jointly with my British, Dutch, French and Italian colleagues gave me much satisfaction. Of course, also the joint works with Kageyama. I have also to mention that a subject of great interest, from the beginning of my research work till now, has been the analysis of series of variety trials. Most of my papers on this topic have been published jointly with Stanisław Czajka and Zygmunt Kaczmarek. The cooperation with them, and later with some other colleagues, gave me great pleasure and satisfaction. See, for example, our last paper, Caliński et al. (2009).

ACA: And which do you think have been unduly neglected?

TC: I have never thought about that.

ACA: Of course, there is more to academic life than publications. What activities have you found most rewarding?

TC: Definitely the promotion of Ph.D. students and the scientific support of young researchers. I have been lucky to give lectures for Ph.D. students and young researchers at various courses and conferences organized in Poland and other countries. Even after my retirement in 1988, I have had, eight times, the pleasure to give such courses of lectures in several places abroad, in Italy, Germany and Portugal. In general, international cooperation has been giving me much satisfaction. I am very glad that this type of activity is continued by my younger colleagues in Poznań, particularly by Stanisław Mejza, as president of the Polish Biometric Society and organizer of the International Biometric Colloquia in Poland; by Wiesław Pilarczyk, as a member of the International Union for the Protection of New Varieties of Plants, and coorganizer of the International Working Seminars on Statistical Methods in Variety Testing; and by Augustyn Markiewicz, member of the Poznań Linear Algebra Group and coorganizer of the International Workshops On Matrices and Statistics (IWMS), as well as of LINSTAT. Also, some of my younger colleagues participate actively in various international research projects. In particular, Paweł Krajewski is very active in several international projects concerning plant genetics, including the project Marie Curie ITN Epitraits, coordinated by the University of Amsterdam. Results of these projects have been published in leading journals. As far as our department is concerned, a very active national and international cooperation has recently been started by Idzi Siatkowski and his bioinformatics group. It includes various research institutes in Poland, Switzerland, USA, Austria and Germany. The first results of this cooperation were published in 2011, for example, Schmidt et al. (2011).

BB: All of this is largely due to your initiatives and your actions.

TC: Yes, but the initiatives were also supported and extended by my co-workers, in the past particularly by Jerzy Baksalary. We were very lucky here in Poznań; despite the political situation, we were able to establish very wide cooperation with other research institutes in the world. I am really very happy that some of my colleagues are now very much involved in this.

ACA: Looking to the future, do you have any hopes or expectations for the future of our subject? Do you have any suggestions for young people?

TC: I think that the future of mathematical statistics depends very much on cooperation with other disciplines. So going back into history, for example, to Neyman, he developed the theory of mathematical statistics due to his strong contact with applied people. His first papers were connected with agricultural experiments. He got some ideas for estimation and for testing hypotheses by working on data from agricultural experiments. You could say the same of Fisher and many others, for example, Nelder.

So, if we really want to develop mathematical statistics, it is important to encourage young people to be in touch with researchers from other fields of science and industry and to be involved in the analysis of data. This is well understood in our Department of Mathematical and Statistical Methods, now headed by Anita Dobek (my Ph.D. student of 1980).

BB: Your wide international collaboration helped to develop statistics in Poland. But you were also very active in collaboration with other universities and institutes in Poland. These activities have been recognized both by your honorary degree from the Agricultural University of Warsaw and by many awards, the most recent one being The Neyman Medal awarded by The Polish Statistical Society in 2012, the centenary year of the Society.

TC: Yes, I was quite involved in the organization of the Congress of Polish Statistics at the occasion of 100th Anniversary of the Polish Statistical Association. As to the Medal, it was awarded for those who contributed to the development of statistical research in Poland.

BB: Also, both Regina Elandt-Johnson and you were awarded the Honorary Doctorate Degree from PULS, you in 1998 and she in 2001. Were you her promoter for this award?

TC: Yes, that is correct. It was a great honor for me.

ACA: We have mentioned Neyman on several occasions. He, of course, made much of his career outside Poland. Were there times when you wished you had also moved abroad? Do you think you made the right decision?

TC: I have never thought about leaving my country for another one. I liked to travel abroad, but I did not have any temptation of staying in a foreign country forever.

FIG. 14. *R. C. Elandt-Johnson at the Honorary Degree Award.*

ACA: And now to finish with a few questions about less statistical matters, you said that there was not great interest in mathematics in the earlier generations of your family. What about your children and grandchildren? Any interest?

TC: In pure mathematics not, in informatics yes. My son works for IBM. So, some mathematical interest is somehow included. His son, Yann, has an M.Sc. in Economics and, of course, he also attended lectures on econometrics, which he liked very much.

ACA: I know you like traveling. You particularly travel in East Poland and in Western Ukraine, is that right?

TC: We have been traveling from the very beginning. But in the nineties, when it was easier to travel wherever we wanted, some people in our university started to organize excursions to those parts of the previous Soviet Union which historically belonged to Poland, that is, the eastern parts of that Poland which existed before the partitions at the end of the eighteenth century. The first place we went to was Wilno (Vilnius), as my wife was born there. We started in 1993 and visited all our eastern neighbors: Lithuania, Latvia, Estonia, Belarus, Ukraine and also Romania. The last was very interesting; we went southeast through Ukraine to Romania and visited the northern part of the country, Bucovina, where there are very unusual Christian orthodox churches. It was near the town of Suczawa (Suceava), around which there are several orthodox monasteries. They are very beautifully painted both inside and outside. They originate from the 15th or 16th century and are still in very good condition. Of course, this part did not belong to Poland, except in the far past. These monasteries are fortified. This part of Romania was always under some other country's rule and it was not allowed to built fortresses around castles, but it was allowed to do so around monasteries. One of the reasons for us to go there was that there is still a Polish community living there, who originally came to develop the salt mines.

FIG. 15. *Maria and Tadeusz Caliński.*

FIG. 16. *Tadeusz Caliński and Anthony Atkinson during the conversation, 2014.*

ACA: What about other interests of yours, for example, music?

TC: I very much like music, in particular, baroque music. In Poznań, every year we have a baroque festival. Last year it lasted more than a week and there were about fourteen concerts. Of course, we listen a lot to music on records. Every year in August there is also a baroque festival called "Music

in Paradise" organized in a monastery Paradyż, in Western Poland, which we very much like to attend.

All: Thank you very much.

REFERENCES

Anderson, T. W. (1984). *An Introduction to Multivariate Statistical Analysis*, 2nd ed. Wiley, New York. MR0771294

Baksalary, J. K., Corsten, L. C. A. and Kala, R. (1978). Reconciliation of two different views on estimation of growth curve parameters. *Biometrika* **65** 662–665. MR0521835

Baksalary, J. K. and Kala, R. (1981). Linear transformations preserving best linear unbiased estimators in a general Gauss–Markoff model. *Ann. Statist.* **9** 913–916. MR0619297

Barbacki, S. (1935). *Ogólna Metodyka Doświadczeń Polowych w Zarysie*. Biblioteka Puławska, Puławy.

Barbacki, S. and Fisher, R. A. (1936). A test of the supposed precision of systematic arrangements. *Annals of Eugenics* **7** 189–193.

Barbacki, S., Caliński, T., Surma, M., Kurhańska, G., Adamski, T., Kaczmarek, Z., Dobek, A., Karczewska, A. and Jeżowski, S. (1978a). Transgressions in barley (*Hordeum sativum* Jess.) 7a. Transgressions of F6 and F7 hybrids from the cross Burea × Brown. *Genetica Polonica* **19** 403–421.

Barbacki, S., Caliński, T., Surma, M., Kurhańska, G., Adamski, T., Kaczmarek, Z., Dobek, A., Karczewska, A. and Jeżowski, S. (1978b). Transgressions in barley (*Hordeum sativum* Jess.) 7b. Transgressions of F6 and F7 hybrids from the crosses, Alsa × Burea, Impala × Himalaya, Lubuski × Lonhi, Lubuski × Brage Korn and Kazimierski × Brage Korn. *Genetica Polonica* **19** 423–436.

Box, G. E. P. (1954a). Some theorems on quadratic forms applied in the study of analysis of variance problems. I. Effect of inequality of variance in the one-way classification. *Ann. Math. Statistics* **25** 290–302. MR0061787

Box, G. E. P. (1954b). Some theorems on quadratic forms applied in the study of analysis of variance problems. II. Effects of inequality of variance and of correlation between errors in the two-way classification. *Ann. Math. Statistics* **25** 484–498. MR0064361

Box, G. E. P. (2013). *An Accidental Statistician*. Wiley, Hoboken, NJ. MR3236035

Caliński, T. (1966). On the distribution of the F-type statistics in the analysis of a group of experiments. *J. R. Stat. Soc. Ser. B. Stat. Methodol.* **28** 526–542. MR0210210

Caliński, T. (1971). On some desirable patterns in block designs (with discussion). *Biometrics* **27** 275–292.

Caliński, T. (1993). Balance, efficiency and orthogonality concepts in block designs. *J. Statist. Plann. Inference* **36** 283–300. MR1234856

Caliński, T. and Corsten, L. C. A. (1985). Clustering means in ANOVA by simultaneous testing. *Biometrics* **41** 39–48.

Caliński, T. and Harabasz, J. (1974). A dendrite method for cluster analysis. *Comm. Statist.* **3** 1–27. MR0375641

Caliński, T. and Kageyama, S. (1991). On the randomization theory of intra-block and inter-block analysis. *Listy Biometryczne—Biometrical Letters* **28** 97–122.

Caliński, T. and Kageyama, S. (1996a). The randomization model for experiments in block designs and the recovery of inter-block information. *J. Statist. Plann. Inference* **52** 359–374. MR1401030

Caliński, T. and Kageyama, S. (1996b). Block designs: Their combinatorial and statistical properties. In *Design and Analysis of Experiments. Handbook of Statist.* **13** 809–873. North-Holland, Amsterdam. MR1492584

Caliński, T. and Kageyama, S. (2000). *Block Designs: A Randomization Approach. Vol. I: Analysis. Lecture Notes in Statistics* **150**. Springer, New York. MR1781064

Caliński, T. and Kageyama, S. (2003). *Block Designs: A Randomization Approach. Vol. II: Design. Lecture Notes in Statistics* **170**. Springer, New York. MR1994124

Caliński, T. and Krzyśko, M. (2005). A closed testing procedure for canonical correlations. *Comm. Statist. Theory Methods* **34** 1105–1116. MR2189420

Caliński, T., Krzyśko, M. and Wołyński, W. (2006). A comparison of some tests for determining the number of nonzero canonical correlations. *Comm. Statist. Simulation Comput.* **35** 727–749. MR2240041

Caliński, T. and Łacka, A. (2014). On combining information in generally balanced nested block designs. *Comm. Statist. Theory Methods* **43** 954–974. MR3171084

Caliński, T. and Lejeune, M. (1998). Dimensionality in MANOVA tested by a closed testing procedure. *J. Multivariate Anal.* **65** 181–194. MR1625877

Caliński, T., Kaczmarek, Z., Krajewski, P., Frova, C. and Sari-Gorla, M. (2000). A multivariate approach to the problem of QTL localization. *Heredity (Edinb)* **84** 303–310.

Caliński, T., Czajka, S., Kaczmarek, Z., Krajewski, P. and Pilarczyk, W. (2009). A mixed model analysis of variance for multi-environment variety trials. *Statist. Papers* **50** 735–759. MR2551347

Camussi, A., Ottaviano, E., Calinski, T. and Kaczmarek, Z. (1985). Genetic distances based on quantitative traits. *Genetics* **111** 945–962.

Cochran, W. G. and Cox, G. M. (1957). *Experimental Designs*, 2nd ed. Wiley, New York. MR0085682

Elandt, R. (1964). *Statystyka Matematyczna w Zastosowaniu do Doświadczalnictwa Rolniczego*. Państwowe Wydawnictwo Naukowe, Warsaw. MR0185785

Finney, D. J. (1955). *Experimental Design and Its Statistical Basis*. Univ. Chicago Press, Chicago.

Fisher, R. A. (1935). *The Design of Experiments*. Oliver and Boyd, Edinburgh.

Kaufmann, K., Wellmer, F., Muiño, J. M., Ferrier, T., Wuest, S. E., Kumar, V., Serrano-Mislata, A., Madueño, F., Krajewski, P., Meyerowitz, E. M., Angenent, G. C. and Riechmann, J. L. (2010). Orchestration of floral initiation by APETALA1. *Science* **328** 85–89.

Milligan, G. W. and Cooper, M. C. (1985). An examination of procedures for determining the number of clusters in a data set. *Psychometrika* **50** 159–179.

Nelder, J. A. (1954). The interpretation of negative components of variance. *Biometrika* **41** 544–548. MR0065083

NELDER, J. A. (1965a). The analysis of randomized experiments with orthogonal block structure. I. Block structure and the null analysis of variance. *Proc. Roy. Soc. Ser. A* **283** 147–162. MR0176576

NELDER, J. A. (1965b). The analysis of randomized experiments with orthogonal block structure. II. Treatment structure and the general analysis of variance. *Proc. Roy. Soc. Ser. A* **283** 163–178. MR0174156

NEYMAN, J. (1932). O metodach opracowywania doświadczeń wielokrotnych. *Roczniki Nauk Rolniczych i Leśnych* **28** 154–210.

NEYMAN, J. (1934). O pewnych twierdzeniach z rachunku prawdopodobieństwa, które służą za podstawę do rozwiązywania szeregu zagadnień doświadczalnictwa rolniczego. *Roczniki Nauk Rolniczych i Leśnych* **31** 233–276.

NEYMAN, J., IWASZKIEWICZ, K. and KOŁODZIEJCZYK, S. (1935). Statistical problems in agricultural experimentation. *Supplement to the Journal of the Royal Statistical Society* **2** 107–180.

NEYMAN, J. and PEARSON, E. S. (1928). On the use and interpretation of certain test criteria for purposes of statistical inference. *Biometrika* **20** 175–240, 263–294.

PEARCE, S. C., CALIŃSKI, T. and MARSHALL, T. F. DE C. (1974). The basic contrasts of an experimental design with special reference to the analysis of data. *Biometrika* **61** 449–460. MR0386176

RAGHAVARAO, D. and PADGETT, L. V. (2005). *Block Designs: Analysis, Combinatorics and Applications. Series on Applied Mathematics* **17**. World Scientific, Hackensack, NJ. MR2187913

REID, C. (1982). *Neyman—From Life*. Springer, New York. MR0680939

SAHA, G. M. (1976). On Caliński's patterns in block designs. *Sankhyā Ser. B* **38** 383–392. MR0654153

SARI-GORLA, M., CALIŃSKI, T., KACZMAREK, Z. and KRAJEWSKI, P. (1997). Detection of QTL × environment interaction in maize by a least squares interval mapping method. *Heredity* **78** 146–157.

SCHMIDT, M. T., HANDSCHUH, L., ZYPRYCH, J., SZABELSKA, A., OLEJNIK-SCHMIDT, A. K., SIATKOWSKI, I. and FIGLEROWICZ, M. (2011). Impact of DNA microarray data transformation on gene expression analysis—comparison of two normalization methods. *Acta Biochim. Pol.* **58** 573–580.

ZAŁĘSKI, E. (1907). Instrukcja dla urządzania doświadczeń porównawczych z różnymi odmianami buraków cukrowych. K. Buszczyński i M. Łążyński.

ZAŁĘSKI, E. (1927). *Metodyka Doświadczeń Rolniczych*. Wydawnictwo Rozpraw Biologicznych, Lwów.