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An interview was conducted on 23 November 2003 with R. Clifford Blair on the occasion on his retirement from the University of South Florida. This article is based on that interview. Biographical sketches and images of members of his academic genealogy are provided.

Keywords: R. Clifford Blair, nonparametric, Wilcoxon rank sum, rank transform, multivariate permutation tests, step-down multiple comparison test, comparative statistical power

> "In the last 30 years there have been important changes in the canons of good statistical practice or data analysis. Until recently, and thanks to the work of J. V. Bradley and R. C. Blair among others, it is no longer heresy to say that distribution–free tests – such as the Wilcoxon-Mann–Whitney are preferable to their normal theory alternative – the t test." (Bruno D. Zumbo & Donald W. Zimmerman, 1993, *Canadian Psychology*, *34*(4), p. 441)

Background

JMASM: What are some of the memorable events from your childhood?

RCB: I was born in an area that is now Tampa, Florida. It was rural – I remember the cows, chickens, and pigs. We lived on a tiny, dirt road. We were poor; once my mother boiled an onion for three of us "youngins." She told us we were having onion soup, but no, she really didn't like onions so she would be having only the broth.

The people who lived in our area were farmers who came from southern Georgia. When the depression came along, they moved into the cities looking for work. My parents worked in a cigar factory.

This was in the time before machines were used, so my mother worked with hand-rollers. She went as far as the 7th grade. Her family lived in the Lake Okeechobee area, where they picked vegetables and hunted sea turtles. Her father, my grandfather, was a part-time Baptist minister and part-time moon-shiner.

My father, who died when I was nine years old, made it to the 2nd grade. He was a mechanic in the cigar factory. When he came into contact with the Spanish of the Cuban community he fell in love with the language. Eventually, he learned how to speak and read Spanish, and he especially enjoyed reading Mexican classics.

I went to a school where the girls wore shoes, but most boys didn't. Actually, there were two kinds of students – "by the dayers" and "by the weekers." The dayers were children who turned in their twenty cents lunch money day by day. The weekers were the upper class; those who had the entire week's lunch money on Monday. The boys among the weekers had shoes, but those of us who were dayers, the lower members of the social hierarchy, didn't have shoes. (My hobby is writing short stories,

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and I've written on my experiences at that little school, Thomas A. Edison Elementary.)

I went on to Memorial Junior High School and then Hillsborough High School. I was in a class for the trainable and mildly retarded. There had always been good programs for the severely retarded, where the children were taught how to tie their shoes and other functional skills. However, the school system relied on informal programs for the mildly retarded. Our classes were taught by a basketball coach. He wasn't credentialed; he had the job because he had spare time.

My favorite class was personal hygiene. The coach taught us that we should wash under both of our arms. And then, we would have a test. The questions would be something like "1. You should wash under how many arms? (a) only your left arm, (b) only your right arm, (c) both arms", and my preferred answer, "(d) none of the above." Alas, this was too much for me, and I failed eleventh grade. So, I ran away from home. I went to Atlanta for a few weeks, hung out with some bums, almost starved to death, and had no choice but to come back.

My mother thought that my vision played a role in my lack of attention at school. It had been checked, but the doctor hadn't made the proper diagnosis. The degenerative eye disease I have is extremely rare in juveniles. When I returned from Atlanta, my mother decided to have my vision checked again. I was taken to a specialist, who determined I was nearly blind. I was bundled up and sent off to the Saint Augustine State School for the Blind, where I was viewed as being mildly retarded and having a severe visual impairment.

After I graduated, the Bureau of Blind Services sent me to a rehabilitation center in Daytona. It was popular at that time to give blind people jobs in a hospital or post office. They would run a small concession stand, selling candy, coffee, and cigarettes. It wasn't clear if I could be taught how to make change. I spent a lot of time sitting at a table with giant paper dollars, and large disks representing quarters, dimes, nickels, and pennies. The teacher would say, "I am buying two candy bars, they're seven cents a piece, and I am giving you a dollar. How much change do you give me back?" I would tear one of the giant paper dollars in half and give it as change. After a while, it was determined that perhaps I might be more suitable for a career in manual labor.

They enrolled me in a class where we were taught how to work with plants in a nursery. I was in a horticulture class where everyone, including the teacher, was completely blind. Obviously, even though I have severely limited vision, it was sufficient to make me the king of the class. An important event occurred at that time, which was to change my life.

In an effort to help me make change for a dollar, the rehabilitation center had given me a magnifying glass. We were outside working with the plants, and I started complaining about the firebugs that were biting me. I said "These firebugs are eating me up, are they bothering you boys?", but of course they said "No, they aren't bothering us." Then, I started swatting all around me, making a lot of noise in doing so, for the entire day.

The next day, when we were working outside, I took out my magnifying glass and focused it on the back of their necks, so they would feel it burning. One classmate slapped his neck and said, "Damn Cliff, they're getting me now. I can feel them biting me all over the back of my neck." They really thought we were being attacked by firebugs!

I entertained myself doing that for quite a while, but then got sent to the school nurse, who was the disciplinarian. She called me in and said, "Cliff, this is not a discussion. I'm going to tell you this only once: All the talk about the outbreak of firebugs will cease immediately. You are dismissed."

The school officials decided, because of this incident, that I was a bit too precocious, so they gave me a quick screening IQ test, which was the first such test I had taken. My scores didn't match my academic profile. They called in a paid intern who was a doctoral student in psychology from University of Florida, who gave me another test. On that basis, he decided to take me to Gainesville, to visit the Department Chair, who gave me a complete battery of tests. So, I went from washing under (both) your arms to enrolling in college.

Now, I figured I was going to do higher mathematics in college, so I set about memorizing all of my nines tables! Then, I enrolled in some classes. I was so scared that I would be sick to my stomach, brush my teeth, and go to class. There were no disabilities offices in those days; you either made it or you didn't. I do recall one professor who made it clear he wanted me out of his class. After a while, I decided he was right, and left school.

I got a job in a factory in Tampa emptying trash cans. After being there for about a week, one of the ladies on the assembly line asked me what I did. "I'm the trash man." I felt good about that reply, so I continued telling my co-workers that I was the trash man. After a while, an elderly gentleman called me over. He said, "I hear you've been telling everybody that you're the trash man. You better get this straight. You are *not* the trash man. *I'm* the trash man. You're the *assistant* trash man." So, I guess I promoted myself a little bit when I called myself the trash man, because I was, in fact, only the assistant trash man.

After a few years, I decided to try college again and came to the University of South Florida (USF). I continued to read and catch up. I would find big words in the dictionary, and try to use them in a sentence. My first success was, in fact, in an English class. The assignment pertained to a story about funerals, funeral homes, and death. I wrote a very somber, thoughtful, introspective yet reflective, spiritual essay. It was an intellectual breakthrough for me, and I was quite pleased with my effort.

The professor came into class with the graded papers. He said, "I have a paper here that was a delight. It's probably one of the finest examples of humorous satire I have read from a student. I was reading it to the passengers in my carpool, and the driver laughed so hard he drove off the road. We thought we would be killed. The student who wrote this is Cliff Blair; Cliff, congratulations!" My very first college success: I got an A+. I went on to graduate from college!

JMASM: What role did humor play in your youth?

RCB: Although in retrospect, I was somewhat depressed as a child, life became very funny to me. I would take a closer look at what was happening to me and laugh.

Maybe it started back at that rehabilitation center. I had been out with a couple of guys one night. We had been drinking beer, and at curfew they went back. I had decided to stay for a few more hours, and then I tried to sneak into the dormitory from the rear entrance. I had not been in the back, it was dark, and I have very limited vision. I came upon a fence, and in climbing over it I got stuck. I was caught upside down! I continued to struggle, and after a long while I finally broke free.

The next day the supervisor announced we had been vandalized that night. "It was awful," he said. "Someone trashed my wife's rose garden." It turned out I wasn't caught in a fence; it was a rose trellis. If I had only walked either a little to the right or to the left I would have avoided it. The incident was very funny, and I began to see the world as being a bit odd – as if I was viewing it upside down.

Research

JMASM: What interested you in statistics?

RCB: After the long journey through the undergraduate program, I decided I wanted to get a Master's degree. I didn't have the money, but there was a new program in ageing studies that had stipends for students. So, I decided to obtain a Master's degree in that subject. I took a course in social and behavioral science measurement theory from Professor John Neel, who's now at Georgia State University.

He had introduced Chebyshev's theorem, which certainly caught my attention, because it was way over my head. In the context of that lecture, he mentioned to the class that he was proud that the department had just obtained a programmable calculator made by Wang. It was programmed in pseudo-assembler, with two-digit numbers. I was very impressed with the device, but of course none of the other students demonstrated any interest – they were happy enough to get through the course. John offered to give me a closer look at it, and he showed me how it was programmed.

We had recently learned about Pearson's product-moment coefficient of correlation, so I asked and obtained permission to try and write a program to compute it on the Wang. I spent probably about a week working on it, but finally coded the correlation coefficient. I showed John how I could input data for the X and Y variable and it would produce the result.

Professor Neel seemed impressed with my efforts, and in our ensuing conversations, he discovered the paucity of my math background. He gave me his ninth grade algebra book. I studied it, and I liked it. So, I started taking more of his classes in measurement and statistics. I completed the Master's degree and enrolled in the Ph. D. Measurement, Evaluation, and Research program in the College of Education.

To prepare, I took algebra, trigonometry, and introduction to calculus. Then, I took a Fortran class in the College of Engineering. (Later, as an Assistant Professor, you and I took a three course calculus sequence together.

JMASM: I enjoyed the refresher. I enrolled in the course so I could take notes for you, because by then you weren't able to see writing on the chalk board.

RCB: Correct.) Eventually, it became time for me to do my dissertation, and by then I was primarily interested in statistics. I was looking for a statistics topic, but the focus in the department was on measurement, evaluation, and research methods. Therefore, I went to the math department and met Professor James J. Higgins. We discussed various statistical topics.

Jimmy was really trained as a probablist, but had become a statistician. I asked him to chair my dissertation committee, and Bruce Hall, the measurement expert from the College of Education, was the co-chair.

One of the things that fascinated me when I first started college was footnotes. Due to my vision, either I had never seen them before or I simply ignored them. They were tiny markings that I hadn't recognized as letters of the alphabet. As I went through college, therefore, I made it a point to read them.

I read some footnotes in statistics books regarding the comparative power of nonparametric hypothesis tests. Book after book that I read indicated that nonparametric tests have the advantage of not needing the specification of the population (i.e., normality), but the unfortunate shortcoming was that they lacked statistical power as compared with parametric tests. Nonparametric tests were often described as rough, crude, quick, and dirty.

However, by this time I had read about asymptotic relative efficiencies (AREs) – I first came across it in a footnote. I saw a quote from William Mendenhall who said something like "Don't pay much attention to these things, because asymptotic relative efficiencies deal with infinite sample sizes and infinitesimal treatment effects" which has little application in the real world. Nevertheless, the ARE's indicate that a test such as the Wilcoxon Rank Sum (WRS), for example, should have a huge power advantage over the independent samples t test. But, textbook authors claimed it doesn't. I wondered about what point the WRS loses power or what its comparative power would be for small samples.

I made it my practice to check things out empirically, because I had taken that Fortran course. For example, when I heard the claims about the central limit theorem, I wrote a computer program to see what happens to the distribution of sample means as either the sample size increases or the number of re-samplings increases for a fixed sample size. So, I began to do the same thing to check the power comparisons between the two tests.

We didn't have personal computers at that time, so I had to go across campus to the computer center. It was still in the days when we had to use a key punch machine to punch cards. I can remember the evening, just before they closed, that I obtained the first power results. The power of the Wilcoxon Rank Sum (WRS) test was *way above* that of the t test for certain nonnormal distributions. I checked and rechecked the code carefully, so I knew the results were correct.

Then, my hands began to shake, and I couldn't see the results even with my high powered loupe. Gradually, though, it dawned on me that hundreds of books that I had read were wrong. Authors explained the WRS must be less powerful because when original scores are converted to ranks, information in the data set is lost, and there is a resulting loss in statistical power. The explanation is logical, but wrong.

JMASM: There was a departmental library in the (former) College of Education building at USF. It was a sizable collection of statistics, measurement, research, and evaluation books. I checked out a book by Glass and Hopkins. At the place where they indicated nonparametric tests were less powerful, someone had written in the margin, “poo-poo.” Are you the culprit?

RCB: Yes, that was me. That was the standard thing that I wrote in the margins on this issue. By that time I had gotten enough preliminary results to know the statement was wrong. I got full of myself. I realized that I was right, and the big guys were wrong.

In the late 1970s, I tried to publish a couple of papers, but the editors would reject them. The reviewers indicated there must be a problem with the computer program, and that would be why the WRS has more power.

I read C. Alan Boneau’s work. He published some Monte Carlo simulations in 1962 in *Psychological Review*. (His more famous article appeared in *Psychological Bulletin* in 1960.) Unlike other authors who claimed the WRS was less powerful, at least he contended there wasn’t any difference. But, he had only investigated limited study conditions, such as very small sample sizes.

JMASM: The power advantages of the WRS over the t test, under departures from population normality – but not homoscedasticity – for a shift in location alternative, increases as the sample size increases. Yet, the recommendation in many textbooks is the opposite: As the sample size gets smaller, the security blanket of the central limit theorem is lost, so that is when one should turn to a nonparametric test. However, the recommendation of when to use a nonparametric test is being incorrectly dictated by the limitation of the t test, when in fact it should be based on the properties of the WRS. Do you agree?

RCB. Yes. To get the huge power advantages in nonparametric tests, certainly use them when there are large sample sizes!

Then, I saw the 1972 article by Glass, Peckham, and Sanders in *Review of Educational Research*. Their view was that the parametric tests are robust enough so that there’s never a need to turn to the less efficient and less powerful nonparametric tests. They referred to James V. Bradley’s work. But, they discounted it because they claimed we now understand more about the robustness of the parametric test.

JMASM: They said, “applied statistics experienced an unnecessary hegira to nonparametrics”. A hegira means to escape danger!

RCB: They believed “the flight to nonparametrics was unnecessary”. They said Bradley’s work threatened the “safety of the herd.” I took that as demeaning. Apparently those of us who dabble in statistics have the mentality of being part of a herd.

I communicated frequently with James Bradley, and I was greatly influenced by his work. So, I took it upon myself to respond to Glass et al. My article appeared in *Review of Educational Research* in 1980.

This brings up the issue as to how I wrote my early manuscripts. When I first started writing I didn’t have a mentor. I would find a journal that I wanted to target, and read some articles that had been published in it. Unfortunately, in the controversies in the literature raging at that time, many combatants wrote harsh statements about one another. I was given to understand, therefore, that this was the scientific manner of publishing an article. So, I wrote my manuscripts in a fashion that raked various supporters of parametric procedures over the coals. I made of lot of people mad with me.

It turned out to be helpful, though, as reviewers were so angry with me, that instead of just rejecting my work, they spent considerable energy in response. I got very important tutoring from some of the best researchers in the field that way. They cited reference after reference, and, I would look each one up. It was a very valuable experience. Of course, it didn’t help that I was writing articles touting the benefits of nonparametric procedures, and reviewers figured I was wrong anyway.

After a while, though, my articles began to get published. I was gratified to see that in subsequent editions of many of the textbooks I referred to earlier, the authors made changes to the text. Sometimes they quoted me, and sometimes they didn't. Later, they quoted me and you, and sometimes they didn't. But, the main point is that they changed their texts. They finally recognized that nonparametric tests are often far more powerful than parametric tests under the commonly found conditions discussed in many of our articles.

Because I hadn't had a proper background in mathematical statistics, I had to rely mainly on the computer programs. This had the advantage of requiring me to consider practical issues and conditions that, quite frankly, sometimes are overlooked or discounted by mathematical statisticians. I learned a lot about the real properties of statistics this way. Also, by this time, Jimmy was giving me articles to read, and that also helped a lot.

One day, in 1983, a doctoral student approached me about a dissertation topic. We discussed the rank transform. Ronald Iman – former President of the American Statistical Association – and William Conover, who presented the procedure, based their recommendation to use it on Monte Carlo evidence, but it was our contention that their support was insufficient. They had primarily examined the rank transform in the context of independent two sample and one-way layouts. Although they examined its properties under a factorial design, it was in nonrealistic contexts, such as the presence of only main effects, only an interaction, or very small main and interaction effects.

Therefore, I suggested the student examine the robustness and power properties of the rank transform in the context of the 2×2×2 layout, with the presence of small, medium, and large higher-order interaction, lower-order interactions, and main effects. I thought the rank transform was a neat idea, but they hadn't sold it completely. Many sources indicated it was much more difficult to preserve robustness with respect to Type I errors when normality is violated – and similarly to detect – interaction effects, as compared with main effects.

That doctoral student wrote the Fortran program. By now the key punch machines were being replaced with terminals, so the process of coding, compiling, executing, debugging, and so forth was much faster. Soon, results began to appear. I got a call late one night, and the student was concerned. He had gone over the program many times, but was still not getting good results for the rank transform. He was telling me that the Type I error rate for the test for interaction at a particular sample size and effect size had ballooned from 0.05 to 0.35. He was telling me that matters got *much* worse as the sample size got larger! We concluded that the statistic was flawed. This student and I then went on to write a number of articles on the rank transform. You were that student - remind me about what happened when we tried to publish those results.

JMASM: The main results were sent to a certain prestigious journal in 1985. After about six months, the Editor advised us that the paper was lost and to supply another copy. About nine months latter, we received a letter wherein a reviewer had requested a complete set of printouts – this was in the day of green and white 132 column-wide fan-fold computer paper, and the results were contained in a stack several feet thick. We mailed the printouts for the primary results immediately, but the manuscript was kept in review for almost two and a half years.

The article was rejected. The Editor based the decision on the weight of a single reviewer. That reviewer said that although he could find nothing wrong in the study conditions of the Monte Carlo, the procedures we used, or in the reporting of the results, he recommended the paper be rejected because it contradicted what well-known people had already written on the subject.

That well-known person the reviewer was quoting was, in fact, himself. Although the signature line had been blocked out to preserve anonymity, the editorial assistant had inadvertently failed to block out the affiliation.

Eventually, in 1989, Juliet Popper Shaffer published the primary dissertation results in the *Journal of Educational Statistics* – [now *Journal of Educational and Behavioral Statistics*]. But earlier, in 1987, the secondary results from the dissertation were published by Donald B. Owens in *Communications in Statistics* – remember our concluding sentence in that article? Subsequently, the literature review was published by Penelope L. Peterson in *Review of Educational Research* in 1990.

RCB: It's the nature of the beast.

The rank transform is essentially worthless in the context of factorial ANOVA.

JMASM: Two of my doctoral students further examined the rank transform in 1997. Michael J. Nanna found it to work well in the context of the two independent samples Hotelling's T^2. However, Todd C. Headrick demonstrated that as poor as the rank transform performs in the context of the two dependent samples t test and in factorial ANOVA, it performs even worse in the context of factorial ANCOVA.

RCB: It has always amazed me at how long people hold on to procedures that don't work. Years after all this was published, people still publish articles stating that it is a controversial topic. So-and-so say it does work, but Sawilowsky and Blair say it doesn't work.

JMASM: So would you say the jury is still out on the rank transform when it's Type I error rate goes to 1.00? This could be useful if you can't find a way to get a new drug to the market.

RCB: Yes, when nominal alpha is 0.05, 1.00 is a little high for a Type I error rate. It is my recollection that certain statisticians were on the pharmaceutical dog and pony circuit. If you had a drug you couldn't take to market, here's a statistic that guarantees rejection of the null hypothesis. It went so far that a major statistics software company, SAS, advised in their user manual to run the data through PROC RANK, and do the normal theory test on the ranks.

JMASM: Perhaps, many of the older textbook authors that you contradicted were not alive, and those who were alive were not in front of you to confront you. However, weren't you afraid to take on the scholarship of the discipline; afraid that you were taking on something bigger than you?

RCB: I considered myself to be a minor character, a tiny speck. I was once told that a prominent person in the field was asked, at a conference, about some of my work that appeared to refute his work. He said, "It is too trivial for comment." At first, I was devastated by those remarks. Then, two colleagues explained that my work must be hitting the mark, otherwise it wouldn't be characterized as trivial, but as being wrong.

JMASM: You were once invited to speak at a national conference on a panel discussion regarding the rank transform. Another invitee (from my generation, not yours) spoke favorably on the procedure. A member of the audience complained that a lot of time and money was spent attending these conferences to obtain a "take-home" message, and yet the question remained why the two of you obtained different results. Your answer was perhaps the other person's work was based on different study parameters, different conditions, etc. The other person's reply: "Blair obviously is wrong".

RCB: Yes, I recall that. You and I had studied the rank transform in the context of the 2×2×2 and 3×4 layouts. He had only examined its properties in the less complicated 2×2, and even there, he only modeled very small main and interaction effects. The problems with the rank transform get much worse in a hurry.

JMASM: Nevertheless, I thought you were slighted, because his response wasn't about your work, but about you. Anyway, he was safe in saying that the bad results on the rank transform you were reporting were wrong.

RCB: Yes, for some reason people wanted to ignore the poor properties of the rank transform.

These experiences led to something that changed my perspective. I had read articles where people had gotten into confrontations, and that they were using coded words for "stupid." I went down that road myself, and used harsher language raised to the third power.

But, one day, I was sitting in my office when I was on the faculty at Johns Hopkins, and the phone rang. It was Boneau, who I had raked over the coals more than a decade prior. He had just come from his retirement dinner, or something of that nature. He said, "Did I really do such poor work?"

Before I got that call, I had already come to the conclusion that much of the problem with his work was he was limited to late 1950s – early 1960s computer equipment (IBM 650), and that is why he examined such small samples (e.g., n = 5), and other such limited study parameters. It took ½ hour to generate a thousand random samples. It was a major undertaking for him to do the study he did.

So, when he asked, “Did I really do such poor work?” I felt like I had a stake thrust into my heart. I never felt so bad about anything in my life. There was a real voice out there that I had caused considerable pain. Until that point, all I had done was an academic exercise; from then on I realized that there are real people behind published research.

I told him that I would be forever grateful and in his debt if he could understand that the style with which I had been writing was attributable to the exuberance of my youth. He seemed to indicate that I was forgiven. I swore at that time, that I would never write another article with harsh language. And I didn’t, for at least six months!

JMASM: However, in deference to you, this is a role of an Assistant Professor. The role is primarily oversight and critique. While full Professors are professing and philosophizing, someone has to do the grunt work and check the details. That is one way Assistant Professors make their mark. This was you in the role of Assistant Professor back then. Perhaps, you might have written in a kindler, gentler fashion.

RCB: Agreed. There is no joy or anything to gain in putting others down. Yes, we were providing an important service in keeping an eye on the reporting of bad statistical work.

I was excited about results that I knew no one else knew. That is what gave me satisfaction. I recall in the defense of my dissertation, we had to have an outside person as the moderator. I got the best statistician to serve, because I was confident of my results, and I wanted to be put through the flames, knowing that if I could survive, my work would be correct.

JMASM: Do you recommend doctoral students follow your example and put themselves through the same thing?

RCB: No! Anyway, I thought the role of nonparametric statistics was an important one. I had some insights into the problem, and I had results that I knew no one else had.

My initial interest was on nonparametrics, the rank transform, and later multivariate permutation and step-down comparison tests. After the Boneau incident, however, I realized that a person could spend a career critiquing bad advice in statistics textbooks, or in statistics journals. I had gotten to the point where I could spot it easily. In fact, you and I published an article in *Biometrics* in 1993, where we had spotted such a problem.

JMASM: I share your concern (and some of your skill) in spotting flaws in published research – and why not? After all, I was your student. There is a related question, and perhaps you’ve given it some thought. The literature you have been referring to is important. People turn to the peer reviewed journals to find solutions, to solve the problems of our society. Do you value the literature in helping to solve the woes of humanity? You are retiring from a College of Public Health at the University of South Florida, where issues are studied because lives are at stake. And, even if the lives are not at stake, certainly the quality of life is at stake. Along with many of our colleagues, we could pick apart (not for the fun of it, even though we might enjoy it) the validity of study findings in a hurry. Should we, then, turn to the literature to help solve our problems?

RCB: Yes, but first there needs to be a lot more replication of research before the literature can be considered useful. Doctoral students come along and ask if a certain topic might be viable, and get it turned down because it has already been done. Yet, the study has never been replicated. They should not be discouraged.

JMASM: Isn’t that the primary role of a Master’s thesis?

RCB: I don't see a problem with a major thrust of a doctoral dissertation being a replication of an important study.

I used to tell my students when they took their statistics courses that it would enable them to read the applied literature with a more discerning eye. [Ending my career in a College of Public Health], I pointed out to them that frequently in the medical literature, it will be reported that a statistic was computed, and p < .001. But, nowhere in the article is sufficient information revealed to judge what was done, much less if it was done correctly.

For example, I'm aware of the background of a specific article on an aspect of diabetes published in the literature about twelve years ago. The author had learned how to use SAS. He would flip through the user's manual and try and find a statistic with a data set that looked like his. He had a repeated measures design, but didn't recognize it as such. I was first amazed, and then disappointed, that the article was accepted and published.

However, all is not lost. I've been consulting with Roy Beck, a Professor of Ophthalmology and Epidemiology at USF, for about ten years now. The quality of his work in the Optic Neuritis Treatment Trial is pristine. The randomizations are conducted properly, the researchers are masked (I used to say "blinded", but I don't anymore), and the quality of the research methodology is so high, when they publish work – it is valid. I believe there are certain other groups where the scholarship is superior. But, unfortunately, most of what gets published is junk.

JMASM: Can you speak about the Monte Carlo?

RCB: We have both worked primarily using Monte Carlo methods. One thing that has concerned me was that mathematicians and mathematical statisticians so look down on it that it is difficult to get work published using the methodology. I recall you tried to publish an article where the reviewer remarked that anyone with a computer on their desk could have come up with the statistic. Yes, anyone could have come up with it if they had the insight you had, but none did before you. Unfortunately, important results were not considered publishable in that journal because of the methodology to get the results. The reviewer focused on the method you used. But, there was no closed form mathematical expression to solve the problem. It could have only been handled with Monte Carlo and related methods.

Similarly, I had a manuscript I sent to a certain prestigious journal that the Editor refused to send out for review. The reason was Monte Carlo methods had been used. The point of the manuscript was to show that a procedure previously published in that journal wasn't that bad, but here was a superior technique. The results were obtained via Monte Carlo methods, and the Editor couldn't get past that.

JMASM: A mathematician colleague of mine once said that he finds little value in Monte Carlo methods, other than it was a notable mathematician – von Neumann in 1949 – who coined the phrase in taking a procedure previously conducted by hand and successfully applying it to machines. His rationale: Suppose I wanted to determine the value of a certain function, and did so using Monte Carlo methods. I might run 1,000 repetitions and get a certain value. But, I could then run 1,001 repetitions, and presumably get a better estimate. Or better yet, I could run one million iterations.

I countered that Newton–Raphson, Cauchy, and Riemann are also estimation or approximation procedures. His argument seemed to be that an estimate obtained from the labor of the human mind is legitimate, but from a machine is not.

RCB: Monte Carlo results will never produce *the* answer to a problem. However, if an estimate is acceptable, I don't see the difference between the Newton–Raphson and the Monte Carlo result.

Recently, I was building a table of critical values for a new statistic I've developed. Each critical value was being obtained via permutation methods. I needed to produce over 146 trillion permutations to obtain each value. I realized I would never be able to complete the table this way. So, instead of getting all possible permutations, I took a million random permutations to produce an estimate. I checked the approximate randomization results with several fully articulated critical values, and I had accuracy to more decimal places than I was reporting in the table.

JMASM: So why does Eugene Edgington say in a number of places in his 1980 book *Randomization Tests* that if one conducts an approximate randomization procedure, don't report this technical detail, so as not to confuse the reader? Why was his advice to hide this?

RCB: Throughout the ages, when new things were discovered, various disguises were used until the public learned to accept them. For some reason, especially in mathematics, there is the tendency to get hung up on the method, rather than the answer.

But, the practical value in using this method is obvious to anyone who, for example, needs to build a table of critical values. The results are correct, and they work.

I recall telling you many years ago, that if it could be shown to work reliably and produce valid results, I would gladly give up Monte Carlo methods in favor of waving gourds and feathers over a pile of goat guts – although I suppose I would have to draw the line at doing it while nude.

I read a book recently about the Indian mathematician Srinivasa Ramanujan, who was self-taught. Later, when he came to England with the assistance of G. H. Hardy to study number theory, he said, to paraphrase, "I hope and pray that no one finds a practical application for my work, because it would belittle it." He felt a practical application would detract from the beauty and elegance of what he had accomplished in mathematics. I suppose many mathematicians have the same fear, for mathematics should be viewed through the artistic and philosophical lenses.

That is fine for the mathematician. But, the line is crossed, for example, in civil engineering. The bridge stands up with mathematical models that are only simulated (and perhaps crudely at that), even if not elegantly derived. Indeed, perhaps the models can never be properly solved mathematically, but the bridge is useful and is still needed.

Another danger is when we take our values and insights from our discipline and try to carry it over to another discipline in order to criticize it. I had a student, many years ago (before the advent of personal computers), who I was trying to teach Fortran. I said, "A = A + B." In computer language, this statement simply means that the value in the register representing A is to be incremented by the value held in register B. The student, who was working on a Master's degree in mathematics, said, "A and B must both be zero".

She became irate when I wouldn't accede to her point. I was using symbols that she recognized, but not in the same fashion that she was accustomed to seeming them being used. I learned then that one doesn't casually or easily take the symbols and rules of one discipline and apply them in a critical fashion to another discipline.

JMASM: Perhaps with time, Monte Carlo work will become more acceptable. I noticed that Monte Carlo work in the past would appear in the final section of a journal article, only to buttress the primary results. But, of late, I've noticed the main findings are obtained via Monte Carlo methods, and the latter section contains squiggles in support.

RCB: Younger statisticians have more abilities and faith in Monte Carlo. Previously, a lot of reliance was placed on asymptotic theory, and the question of how that worked wasn't investigated too closely, except to say, it is "asymptotically chi-squared" or "asymptotically normal". Today, researchers are finding results based on small samples Monte Carlo studies, and when large samples are impractical, such as in permutation work, they rely on asymptotics to show the results should hold for larger samples.

JMASM: As time has passed, I've noticed that the algorithm is usually more important than the code. There are a lot of books available showing important Monte Carlo techniques, but the compiler for the language used hasn't been available, or updated, in decades.

In my opinion, the best platform for Monte Carlo work is still Fortran, even though many consider it a dead language. It executes powers of ten faster (if the program written in another language or package will even execute) than code written in higher level programming languages such as S-plus, R, SAS IML; or statistical packages, such as SAS, SPSS, and Minitab. Was it the development of algorithms to simulate reality, the Monte Carlo method, or the Fortran that you found fascinating?

RCB: It was the whole ball of wax. If I just need a result, I may use an inefficient algorithm if it happens to be quicker to code. Other times, I might get caught up in making the code look pretty or elegant. I took to simulations with Monte Carlo because I never had an electric train as a child.

JMASM: You were never much involved in the social aspects of the American Statistical Association, the American Educational Research Association, and other professional organizations. What positive or adverse effect did that have on your career?

RCB: I was never ambitious. Learning what I discovered with these Monte Carlo studies was the reward for me. The big thrill was demonstrating that nonparametric tests can be more powerful than classical procedures. It was nice when other people recognized this, and found my work worthy of being published. I was most excited by the discovery of new knowledge.

JMASM: Do you believe not hob-knobbing in the social settings of the profession prevented you from receiving fellowships, grants, awards, or other types of recognition?

RCB: I really didn't care about those things. It wasn't important to me. In retrospect, though, I probably had much to learn from many people in the profession, and perhaps had I had more contact with them, my career might have gone in other directions.

At critical moments, though, I have been able to connect with established mathematical statisticians. I would have an idea, I would work it out, and I would enlist the assistance of someone who could help me with the details necessary to build a rigorous argument.

JMASM: In the days of Sir Ronald Fisher, E. J. G. Pitman, and Sir Maurice Kendall, apparently the world was not ready for rank-based nonparametric statistics. Frank Wilcoxon said, to paraphrase, "I've got an approximate, rapid procedure", or a "quick and dirty" procedure, perhaps inadvertently setting the tone for the ensuing battle.

Nonparametric rank tests gained steam with the publication of Sidney Siegal's *Nonparametric Statistics for the Behavioral Sciences* in 1956 (a top 15 cited work on Thompson's Web of Science), and both Donald Fraser's *Nonparametric Methods in Statistics* and Tate and Clelland's *Nonparametric and Shortcut Statistics* in 1957. However, there was an immediate backlash. Gaito, in an article in *Psychological Review* in 1960, said, "It is encouraging to note that some individuals have been reluctant to embrace wholeheartedly the nonparametric technique", and cited an article by Grant in the *Annual Review of Psychology* the year prior, who said, "Some much needed negative thinking has recently appeared on nonparametric techniques". The big debate throughout the 1970s – 1980s, that you participated in, was on the comparative power of rank based nonparametric statistics.

The 1980s brought the robust descriptive statistics' movement into the inferential statistics arena. The 1980s – 1990s, with the advent of inexpensive and powerful personal computers, puts us in the era of practical permutation and exact statistics.

I have colleagues who proclaim that even if there was a time for nonparametric rank tests, that time has passed. So, I ask you, "Was there ever a time, or better, will there ever be a time for nonparametric rank tests?

RCB: I've seen the argument for permutation tests – we have a PC so why convert to ranks and do a rank based test when the permutation test can be done? This is the problem that we've discussed already, and unfortunately, it seems few people understand this. If you examine Monte Carlo results, it will be learned that permutation tests give virtually the same power as their parametric counterparts. For example, the permutation t test gives almost identical power as the two independent samples t test. The reason for turning to a rank based nonparametric test, such as the WRS, is because of its power advantages!

Motivated by concerns about robustness of parametric tests, colleagues ask, “Why convert to ranks when it is now possible to do a permutation test on the original scores?” The answer is that I’m not turning to an alternative test because of the t test’s robustness; its Type I error rates are adequate under nonnormality. I’m turning to rank based nonparametric tests because of their power advantages.

For example, I’m currently working with visual acuity scores with a skew coefficient of 3. With that level of skew, the WRS will have four to ten times the power over the t test *and the permutation t test*. That is the reason for selecting a rank based nonparametric test.

Jim Higgins and I wrote a letter to the *American Statistician* in 2000 in response to someone promoting permutation tests. His point was you don’t have to lose power anymore by converting to ranks because you can now do a permutation test. Our response was to cite our research that showed the opposite – power is gained by converting to ranks. Their reply was there might be a theoretical reason to believe that, but in applied research those considerations don’t apply.

I took some of Roy Beck’s data and replicated a number of the studies we previously published, such as the article you and I published in *Psychological Bulletin* in 1992 using Ted’s [Theodore Micceri] real education and psychology data sets. The same four to ten times the power advantage accrued to the rank based nonparametric test. I started to write a retort to their reply, but I decided it was to no avail. This battle is endless.

JMASM: So there never has been a good time, according to the experts and masters, to do a rank based nonparametric test?

RCB: It never had its time, except perhaps briefly before calculators were invented. It was a quick way to analyze data. If you were working with sixty countries’ Gross National Products, the numbers would be too large to sum and square, but in converting to ranks it became manageable.

One reason why it never had its time was because rank based nonparametric statistics were always presented as a way to control Type I error in the absence of normality.

JMASM: That reminds me of the time you sent me to the library to retrieve Jeffrey Rasmussen’s 1985 article in *Evaluation Review*. He was critical of your work, and set out to refute it.

He constructed a study where he first applied a data transformation designed to maximize homoscedasticity and stabilize within-group normality before conducting the t test, but he failed to do any type of data cleansing before conducting the Wilcoxon Rank Sum test. He concluded your work showed phantom power advantages.

It was not a fair comparison. The Type I error properties of the WRS are invariant under departures from population normality, but its power properties are not! He should have also conducted some equitable form of data cleansing prior to conducing the WRS. As you say, people view the role of nonparametrics only as a method to control Type I error, forgetting about power considerations. I was so upset about this, by the time I returned to your office with the paper, my knuckles were white from grasping the article so tightly.

RCB: That’s why to this day I root against the Purdue Boilermakers football team.

For a time, you and I, and others working in this area, had an impact as far as what textbook authors wrote on rank based nonparametric tests and on the rank transform. But, as time passes, authors seem to be drifting back. I suppose we must leave it to the next generation to rediscover the power of rank based nonparametric tests.

Teaching

JMASM: In terms of classroom teaching, your *c.v.* indicates you’ve won many awards, and some of them multiple times. How did you make the transition from scholarship to teaching? Are there students at the end of your words, or are you directing your lectures to the discipline?

RCB: My focus is on the students. I’ve developed certain ideas regarding teaching.

They are based on my experiences as a college student. I would often listen to a professor and wonder, "Isn't there a clearer way to say this?"

There were certain areas of statistics that I found fuzzy and difficult to grasp. I wanted to find a way to transfer information in a better way. Richard Taylor – a student in one of my classes and now a faculty member – and I spend a lot of time on this issue in developing our biostatistics textbook that is to be published by Prentice-Hall. We take the material apart, piece by piece, to make sure each concept has a logical flow, and is understandable.

In my experience, a lot of what went on in the classroom, on the part of the professor, was done for reasons other than promoting learning. There is a strutting factor, to show you my importance, how much more I know than you, or how powerful I am that I can ruin your career with a low grade. If the focus is on the learning process, instead of all that, it changes the classroom dynamic and environment.

I was once criticized by a faculty member (who later was turned down for tenure). He bragged about how much more difficult the students found his course. He would point out that I was teaching statistics courses, which everyone knows should be more difficult than subject matter courses, and yet my students found that, after doing the required reading and homework, the course was rather easy. His courses, however, were received as being very difficult, and he took great pleasure in that. I explained to him that was what I spent most of my time doing in developing my lectures – to find ways to make the material understandable and obtainable, not difficult and incomprehensible.

Daniel Purdom [Professor of Educational Leadership and Higher Education at USF in the 1970s through 1990s] used to say no learning takes place without pain. I think about that statement all the time. For some students, and perhaps in some disciplines, that may be the case. But in my teaching experience it is not true, nor is it necessary. A lot may be learned in statistics without pain.

JMASM: The reactions to "statistics", when responding to people who ask what subject I teach, are "that was my worst subject", "that was my hardest subject", or "I hated it."

RCB: Or, "that was my worst teacher." Unfortunately, we are overrun with bad teachers of statistics. When I first started teaching I used to say, "One of the things we desperately need is more dead statisticians."

I remember a certain statistics course I took. It was taught by the meanest, nastiest person I ever met. He was very full of himself, and the main point of his lectures was to demonstrate how smart he was and how dumb we were. He wrote a formula on the chalk board. He used "N-1", explaining that was the way to unbias the estimate. He pointed to me and asked, "Cliff, what is N-1?"

I knew how to determine "4-1" or "2-1". But, I didn't know how subtraction was done when mixing letters and numbers. It didn't make sense to me. Most professors will go on to the next person. But, he wouldn't let it go, and he continued to grill me. "Come now, Cliff," he barked. "I just went over this. What is N-1?" I started sweating and was very nervous. Finally, a revelation came to me: "*M*" I yelled! He suggested I drop the class, which of course I did. If I was going to be a teacher, I knew then what kind of professor I didn't want to be.

It is vital to know when a little bit of pressure may be applied, and when a little bit of pressure must be released. I try to "take the temperature" of the class. I can tell when things start to get tense, and that is when I put aside the prepared lecture and launch into a story to make the same point. I let my students see my fingers wiggle when I'm adding or subtracting. It changes the atmosphere from drudgery to pleasure.

Administration

JMASM: Why did you accept an administrative post?

RCB: I became Chair of the Department for two reasons: (1) it was experiencing some difficulties and needed help, and (2) the Associate Dean asked me to do it as a personal favor. I hated every minute of it, as I knew I would, and I would never do it again. I didn't accept it for only altruistic reasons; I was offered a Sabbatical leave the year following my tenure as Chair of the Department.

JMASM: Would you recommend someone who has recently acquired tenure to aspire to the Chair's position?

RCB: Only if that person likes working with budgets, or having faculty members in your office wringing their hands about all the things that afflict faculty members. I soon found the key to success as Chair, and I believe I had the reputation of being a successful administrator.

When people came to me for money for travel, equipment, etc., I said yes. Then, when the business manager would want to set up an appointment with me, I would find ways to put it off. I knew the date when my tenure as Chair would be up, and I was going immediately to the Sabbatical, leaving the finances in the capable hands of the next Chair. As a result, even today people talk about how remarkable it was that I was able to fulfill their every request.

Advice to Junior Faculty

JMASM: What is your advice to the new assistant professor?

RCB: Get out now while you can! I came into this business exactly at the right moment. What I enjoyed most about being a professor was my degrees of freedom.

I could chase the Wilcoxon test, the rank transform test, and the permutation step-down test. Back then, if I needed to do a Monte Carlo, I only needed the capabilities of a Tandy Radio Shack Model 80 personal computer to do the work.

However, today, in many universities, it is almost not possible to get tenure without bringing in federal dollars. And, I mean specifically federal grants, because state and local money doesn't provide sufficient indirect or overhead. The professor has to tailor the research agenda to meet the funding initiatives.

Active pursuit of a half million dollar federal grant, or more, is paramount in the life of junior faculty. Very little consideration is given to what happens in the classroom, and hardly anyone cares about the quality of research if the number of publications is sufficiently high. Unfunded research, even if it wins the Nobel Prize, does not bring in dollars to the university.

To be fair, universities with this orientation make this clear to new assistant professors, and I imagine in places where they don't, the faculty figure it out for themselves. In the contact I have with some faculty struggling with this, I see that they are not pursuing what they really love; about what motivated their careers into academe, but rather, the pursuit of money for the university or for their laboratories.

Read the 1982 book *Betrayers of the Truth* by William Broad and Nicholas Wade, and the more recent *The Baltimore Case* by Daniel Kevles. They opened my eyes about funded research, although I've suspected that type of thing for many years.

There are universities officials who proclaim an ambition to create grant mills, a production line to capture federal dollars. Much of the fraud in research comes from this mind-set. Perhaps the Principal Investigator didn't commit the fraud but was under pressure, and put so much pressure on junior faculty, fellows, postdocs, and graduate students that they committed the fraud. I've concluded that the quality of research decreases when the primary purpose for conducting it is to obtain research dollars instead of answering a research question.

If a faculty member is interested in pursuing a topic, and seeks funding for it – that's great. However, a study conducted primarily for the sake of providing the university it's indirect will be problematic. In order to get the grant renewed there are certain outcome expectations. It obviates the ability to do large scale, high quality research when the driving force is money instead of truth and new knowledge.

If an assistant professor asked what should be concentrated on to get promoted and tenure, I would respond to go after grant money. What I had for thirty years, the pursuit of new knowledge for the sake of new knowledge, in many universities, no longer exists.

Journal Articles v Textbooks

JMASM: The impression I got in my early years from you was that the success of a research agenda should be judged by peer-reviewed publications. I got the impression that people who write textbooks do so because they can no longer conduct research worthy of publication in peer-reviewed journals and periodicals. Twenty years later, you've given me the "William Mendenhall" maneuver: the supposition that there will never be a statistic or procedure named after me, so why not write a textbook, and indeed, turn out a dozen flavors of the same textbook for a dozen different markets. I see that your first project in retirement is the completion of your biostatistics book.

RCB: Your perception is correct. In my early years as a professor, I only wanted to generate new knowledge. I wasn't interested in setting down the same material that everyone else knows. I was in hot pursuit of questions I wanted to know the answers, such as how does the power function of this procedure compare with a competitor under realistic, applied conditions?

A few years ago, Richard Taylor and I started having conversations about what takes place in the classroom. It led to the desire to write a book that followed along the lines of my quest in research: write a textbook that uses new, and hopefully better, methods to communicate statistical knowledge. I would have never pursued writing *yet another* statistics book, but I thought I had enough ideas on improved pedagogy, materials, and methods to write a worthy new textbook. This, then, became a challenge to me. Therefore, I viewed writing this type of textbook as an extension of my initial reasons for being a professor.

The biostatistics book is turning out to be a different type of book, and at this time I don't really know how it will be received. When I sent the manuscript to a prospective publisher, the reviewers said it was terrible and should not be published. I've had enough papers rejected over the years that my first thought was perhaps it was not the best outlet, as opposed to being crushed that the text was worthless. And, upon closer inspection, I noted the reviewers said this textbook failed to use the standard approach in presenting this concept, failed to promote the standard analysis in that context, failed to use the standard examples, assignments, and so forth. I was gratified about those comments, because that was what I had set out to do; write a textbook that didn't follow the standard approach, but represented new knowledge and new methods. I look forward to it coming out soon as a Prentice-Hall title.

Retirement

JMASM: John W. Tukey purportedly published more after he retired than prior to his retirement. What's in store for R. Clifford Blair?

RCB: There are a number of projects I would like to pursue. I recently presented a poster at the Society for Clinical Trials. They are concerned with, for example, the impact of adding ten patients to a trial. The MRI and doctor's fees can amount to $20,000 per patient. I showed, keeping the power level constant, what happens to the required sample size in terms of how much smaller samples need to be when using nonparametric rank tests. There was considerable excitement; people were running around hollering and waving their arms to come view the poster. This made me think about going back and re-fighting some of the old battles on nonparametrics.

Or, redo the old studies, which were conducted in the context of hypothesis tests, but conduct them again in the confidence interval paradigm. Of course, the results – in terms of the length of the interval being smaller for the nonparametric rank test as compared with the parametric counterpart – will be the same. For some reason, in turning from hypothesis testing to confidence intervals, all that you and I, and our like-minded colleagues, have accomplished is lost, and needs to be demonstrated once again.

I would like to return to a study I started with Dennis Boos at North Carolina State University some years ago. It pertained to permutation multiple comparisons. I believe there are a couple of other papers still left in me, and perhaps a textbook to replace the Pedhazur linear models book.

We've just moved to a small, rural community in Butler, Tennessee. It's an isolated place where everyone takes care of one another. It is a modest, quiet place. If you have a desire to see a traffic light you will have to go out of your way to find one.

They say mountain people don't warm to outsiders, but they've welcomed us with open arms. Life there is about family reunions, blue grass music, picnics, and school activities.

My wife, Cathy, who was a Teacher of the Year in Florida, teaches in a small school with 126 students. She has eight students in her classroom. It's the type of school where classes are let out early because the bus driver has a dental appointment, and the Principal raises money to assist in building indoor plumbing for the poorer families.

We have several pieces of property there, including a small cabin on a river. I will be happy there.

R. Clifford Blair
January, 2005

Table 1. Publications of R. Clifford Blair as of January, 2005.

Book

1. Blair, R. C., & Taylor, R. A. (2005, in press). *Biostatistics for the health sciences*. NY: Prentice Hall.

Book Chapters

2. Blair, R. C., & Karniski, W. (1994). Distribution-free statistical analyses of surface and volumetric maps. In R. W. Thatcher, E. R. John, & M. Huerta (Eds.), *Functional neuroimaging: Technical foundations*, (pp. 19-28). San Diego, CA: Academic Press, Inc.

3. Holmes, A. P., Blair, R. C., Watson, J. D. G., & Ford, I. (1996). Nonparametric analysis of statistic images from functional mapping experiments. In R. Myers, V. Cunningham, D. Bailey, & T. Jones (Eds.), *Quantification of brain function using PET*. San Diego, CA: Academic Press, Inc.

Peer Reviewed Publications

4. Durso, M. D., & Blair, R. C. (1978). A comparative study of achievement of 393 high school students enrolled in first year Century 21 and Gregg shorthand. *NABTE Review*, *5*, 96-103.

5. Blair, R. C., & Higgins, J. J. (1978). Tests of hypotheses for unbalanced factorial designs under various regression/coding method combinations. *Educational and Psychological Measurement*, *38*, 621-631.

6. Blair, R. C., & Higgins, J. J. (1978). Comments on 'Contrast coding in least squares regression analysis.' *American Educational Research Journal*, *15*, 149-151.

7. Blair, R. C. (1978). I've been testing some statistical hypotheses...can you guess what they are? *Journal of Educational Research*, *72*, 116-118.

8. Blair, R. C. (1979). A reaction to 'The right-test but the wrong occasion.' *NABTE Review*, *6*, 63-64.

9. Blair, R. C. (1979). A comment on the relative usefulness of the two independent means t-test and Wilcoxon's rank-sum test for the analysis of educational data. *Florida Journal of Educational Research*, *21*, 97-110.

10. Hurley, B. F., Glasser, S. P., Phelps, C. P., Anderson, D., Blair, R. C., & Riggs, C. E. (1980). Cardiovascular and sympathetic reactions to in-flight emergency responses among base fire fighters. *Aviation Space and Environmental Medicine*, *58*, 788-792.

11. Blair, R. C., & Higgins, J. J. (1980). A comparison of the power of Wilcoxon's rank-sum statistic to that of Student's t statistic under various non-normal distributions. *Journal of Educational Statistics*, *5*, 309-335.

12. Blair, R. C., & Phillippy, S. W. (1980). Another look at the robustness of the two independent means t-test to population non-normality. *Florida Journal of Educational Research*, *22*, 1-13.

13. Blair, R. C., & Higgins, J. J. (1980). The power of the t and Wilcoxon statistics: A comparison. *Evaluation Review*, *4*, 645-650.

14. Blair, R. C., Higgins, J. J., & Smitley, W. D. S. (1980). On the relative power of the U and t-tests. *British Journal of Mathematical and Statistical Psychology*, *33*, 114-120.

15. Blair, R. C. (1981). A reaction to 'Consequences of failure to meet assumptions underlying the fixed effects analysis of variance and covariance.' *Review of Educational Research*, *51*, 499-507.

16. Blair, R. C., & Higgins, J. J. (1981). A note on the asymptotic relative efficiency of the Wilcoxon rank-sum test relative to the independent means t-test under mixtures of two normal distributions. *British Journal of Mathematical and Statistical Psychology*, *34*, 124-128.

17. Blair, R. C. (1982). An examination of some commonly held attitudes regarding the nature and usefulness of nonparametric tests. *Florida Journal of Educational Research*, *24*, 29-35.

18. Blair, R. C., & Lawson, S. B. (1982). Another look at the robustness of the product-moment correlation coefficient to population non-normality. *Florida Journal of Educational Research*, *24*, 11-15.

19. Blair, R. C., Higgins, J. J., Topping, M. E. H., & Mortimer, A. L. (1983). An investigation of the robustness of the t-test to unit of analysis violations. *Educational and Psychological Measurement*, *43*, 69-80.

20. Blair, R. C., & Higgins, J. J. (1984). Some comments on 'The unit of analysis: Group means vs. individual observations.' *Florida Journal of Educational Research*, *26*, 5-20.

21. Phillips, E. R., & Blair, R. C. (1984). Investigating variables related to sex differences in students' abilities to solve word problems in algebra. *Focus on Learning Problems in Mathematics, 5*, 47-61.

22. Blair, R. C., & Higgins, J. J. (1985). A comparison of the power of the paired samples rank transform statistic to that of Wilcoxon's signed ranks statistic. *Journal of Education Statistics*, *10*, 368-383.

23. Blair, R. C., & Higgins, J. J. (1985). Comparison of the power of the paired samples t-tests to that of Wilcoxon's signed rank test under various population shapes. *Psychological Bulletin*, *97*, 119-128.

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28. Karniski, W., & Blair, R. C. (1988). Stability of the global field power. *Psychophysiology*, *25*, 460-460.

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31. Kanarek, K., Kuznicki, M. B., & Blair, R. C. (1991). Infusion of total parenteral nutrition via the umbilical artery. *Journal of Parenteral and Enteral Nutrition*, *15*, 71-74.

32. Blair, R. C. (1991). New critical values for the generalized t and generalized rank-sum procedures, *Communications in Statistics — Simulation and Computation*, *20*, 981-994.

33. Roetzheim, R. G., Brownlee, H. J., Pamies, R. J., Vandurme, D. J., Herold, A. H., Woodard, L., & Blair, R. C. (1992). Reverse targeting in a media-promoted breast cancer screening project. *Cancer*, *70*, 1152-1158.

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35. Karniski, W., Wyble, L., Lease, L., & Blair, R. C. (1992). The late somatosensory evoked potential in premature and term infants. II. Topography and latency development. *Electroencephalography and Clinical Neurophysiology*, *84*, 44-54.

36. Sawilowsky, S., & Blair, R. C. (1992). A more realistic look at the robustness and Type II error properties of the t-test to departures from population normality. *Psychological Bulletin*, *111*, 352-360.

37. Blair, R. C., & Morel, J. G. (1992). On the use of generalized t- and generalized rank-sum statistics in medical research. *Statistics in Medicine*, *11*, 491-501.

38. Blair, R. C., & Morel, J. G. (1992). On the use of generalized t- and generalized rank-sum statistics in medical research. Rejoinder. *Statistics in Medicine*, *11*, 507-509.

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Non-Peer Reviewed Publication

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Figure 1. Academic Genealogy of R. Clifford Blair via Higgins, Loève, and Hadamard.

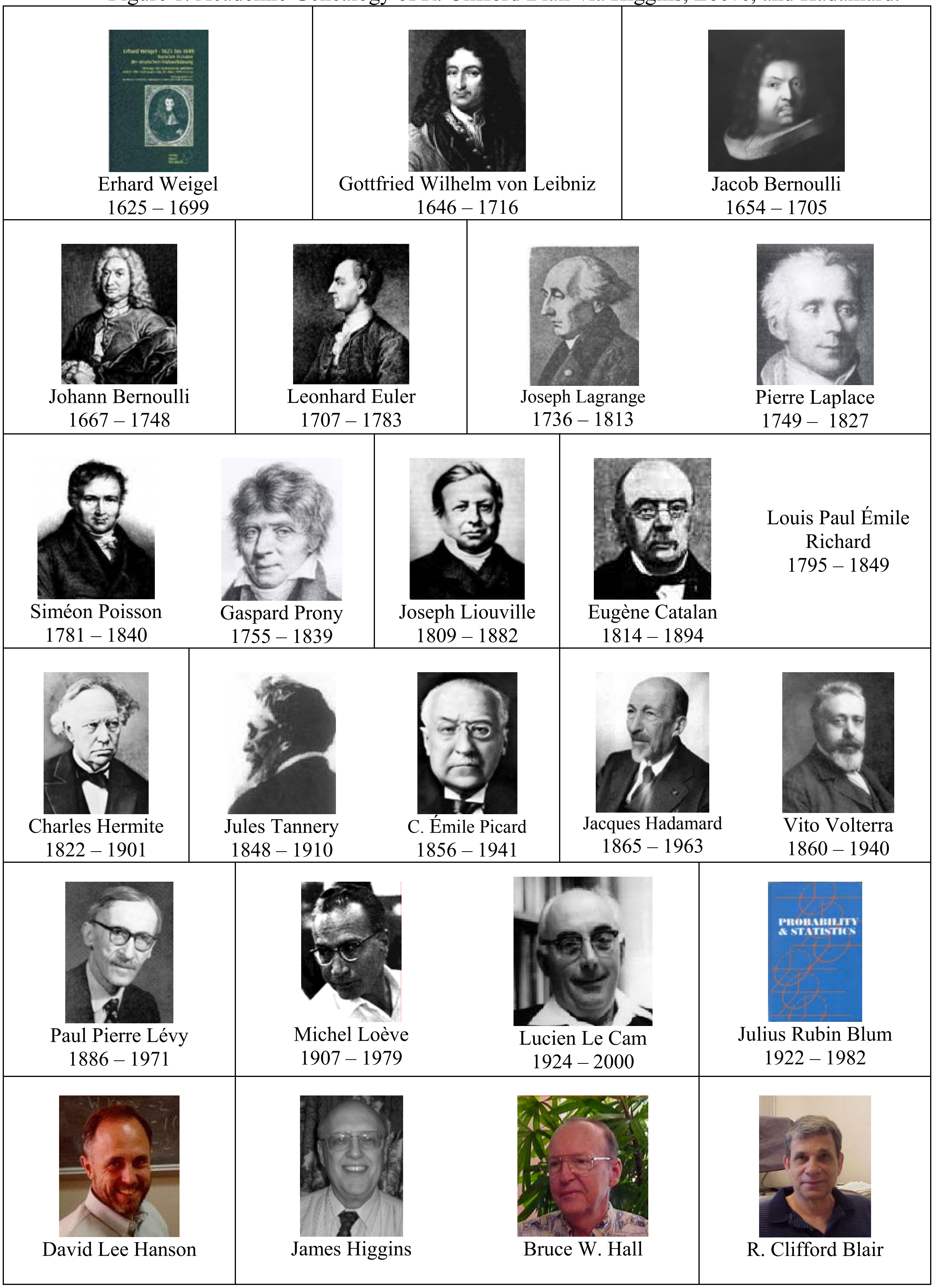
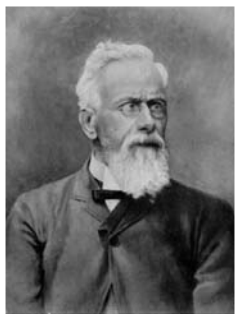


Figure 2. Academic Genealogy of R. Clifford Blair via Higgins, Loève , and Volterra.

Pietro Paoli
1759 - 1839

Vincenzo Brunacci
1768 – 1818

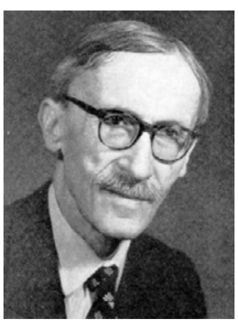
Ottaviano F. Mossotti
1791 – 1863



Enrico Betti
1823 – 1892

Vito Volterra
1860 – 1940

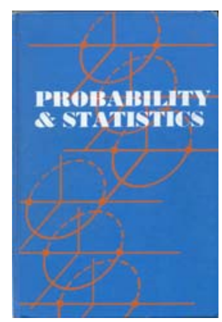
Jacques S. Hadamard
1865 – 1963



Paul Pierre Lévy
1886 – 1971

Michel Loève
1907 – 1979

Lucien Le Cam
1924 – 2000



Julius Rubin Blum
1922 – 1982

David Lee Hanson

James J. Higgins

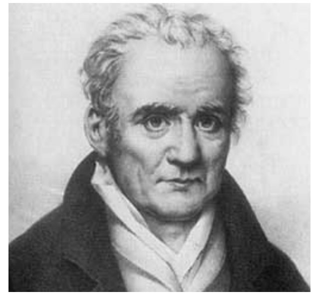
Bruce W. Hall

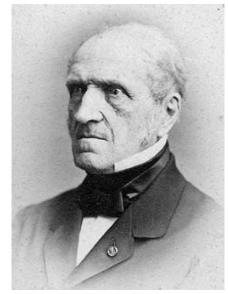
R. Clifford Blair

Figure 3. Academic Genealogy of R. Clifford Blair via Higgins, Le Cam, Zaremba, Darboux, & Poisson.

(For continuation through Poisson, see Figure 1. For continuation through Monge, see Figure 4.)


Siméon Poisson
1781 – 1840


Gaspard Monge
1746 – 1818


Michel Chasles
1793 – 1880


J. Gaston Darboux
1842 – 1917


C. Émile Picard
1856 – 1941

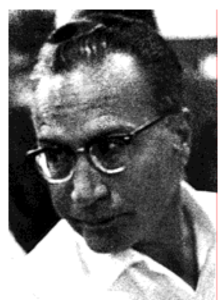

Stanislaw Zaremba
1863 – 1942

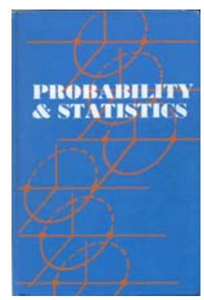

Georgy Voronoy
1868 – 1908


Wacław Sierpiński
1882 – 1969


Jerzy Neyman
1894 – 1981


Lucien Le Cam
1924 – 2000


Michel Loève
1907 – 1979



Julius Rubin Blum
1922 – 1982


David Lee Hanson


James J. Higgins


Bruce W. Hall


R. Clifford Blair

Figure 4. Academic Genealogy of R. Clifford Blair via Higgins, Le Cam, Zaremba, Darboux, and Monge.

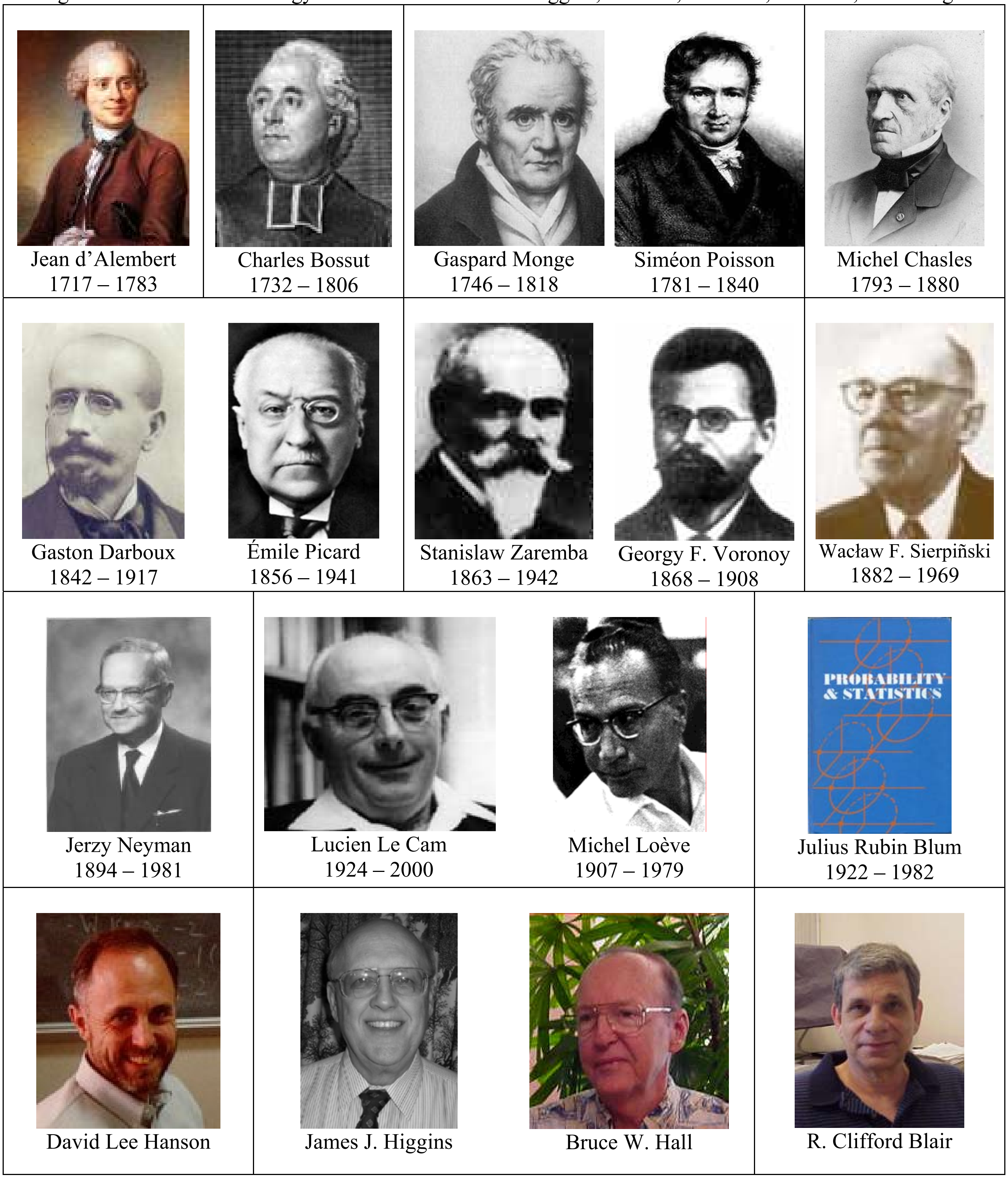
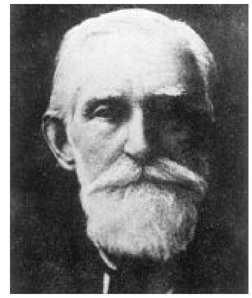


Figure 5. Academic Genealogy of R. Clifford Blair via Higgins, Le Cam, and Voronoy.


Joseph von Littrow
1781 – 1840


Nikolai D. Brashman
1796 – 1866


Pafnuty Chebyshev
1821 – 1894


Andrei A. Markov
1856 – 1922

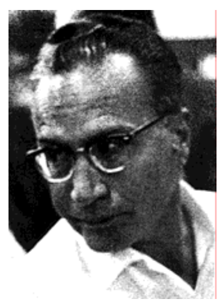

Georgy F. Voronoy
1868 – 1908

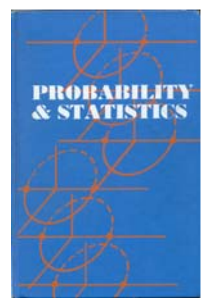

Stanislaw Zaremba
1863 – 1942


Wacław F. Sierpiñski
1882 – 1969


Jerzy Neyman
1894 – 1981


Lucien Le Cam
1924 – 2000


Michel Loève
1907 – 1979



Julius Rubin Blum
1922 – 1982

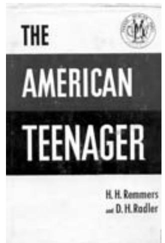

David Lee Hanson

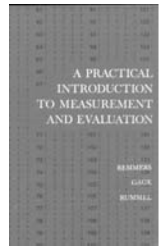

James J. Higgins


Bruce W. Hall

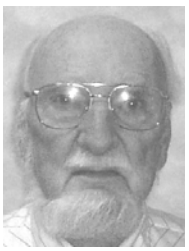

R. Clifford Blair

Figure 6. Academic Genealogy of R. Clifford Blair via Hall and Stoker's Doctoral Advisor.





Hermann Henry Remmers
(1892 - ??)


Howard Stoker

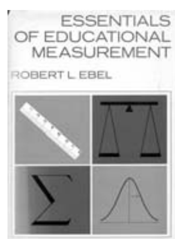

Bruce W. Hall


James J. Higgins

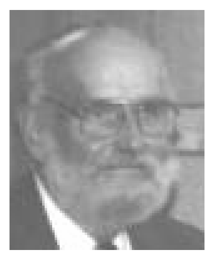

R. Clifford Blair

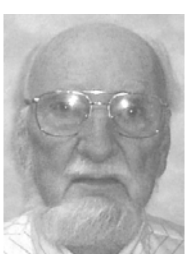
Figure 7. Academic Genealogy of R. Clifford Blair via Hall and Stoker's Master's Committee.


E. F. Lindquist
1914 – 1998



Robert Ebel


Al Hieronymus

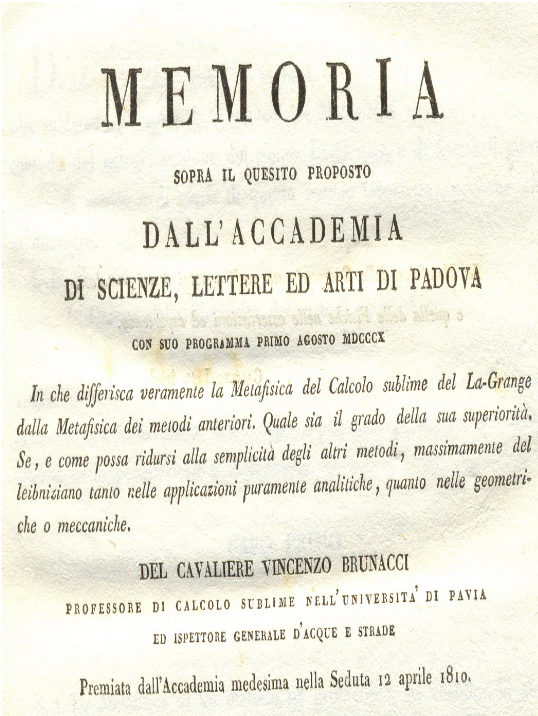

Howard Stoker


Bruce W. Hall


James J. Higgins


R. Clifford Blair

Figure 8. Selected Title Pages from the Academic Genealogy of R. Clifford Blair.*



MEMORIA

SOPRA IL QUESITO PROPOSTO

DALL'ACCADEMIA

DI SCIENZE, LETTERE ED ARTI DI PADOVA

CON SUO PROGRAMMA PRIMO AGOSTO MDCCCX

In che differisca veramente la Metafisica del Calcolo sublime del La-Grange dalla Metafisica dei metodi anteriori. Quale sia il grado della sua superiorità. Se, e come possa ridursi alla semplicità degli altri metodi, massimamente del leibniziano tanto nelle applicazioni puramente analitiche, quanto nelle geometriche o meccaniche.

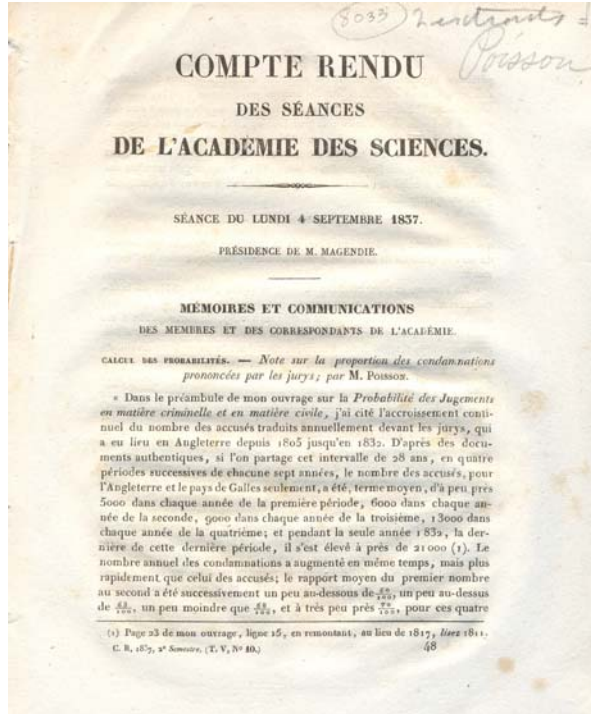
DEL CAVALIERE VINCENZO BRUNACCI

PROFESSORE DI CALCOLO SUBLIME NELL'UNIVERSITA' DI PAVIA

ED ISPETTORE GENERALE D'ACQUE E STRADE

Premiata dall'Accademia medesima nella Seduta 12 aprile 1810.

Vincenzo Brunacci
1768 – 1818



COMPTE RENDU

DES SÉANCES

DE L'ACADÉMIE DES SCIENCES.

SÉANCE DU LUNDI 4 SEPTEMBRE 1837.

PRÉSIDENCE DE M. MAGENDIE.

MÉMOIRES ET COMMUNICATIONS

DES MEMBRES ET DES CORRESPONDANTS DE L'ACADÉMIE.

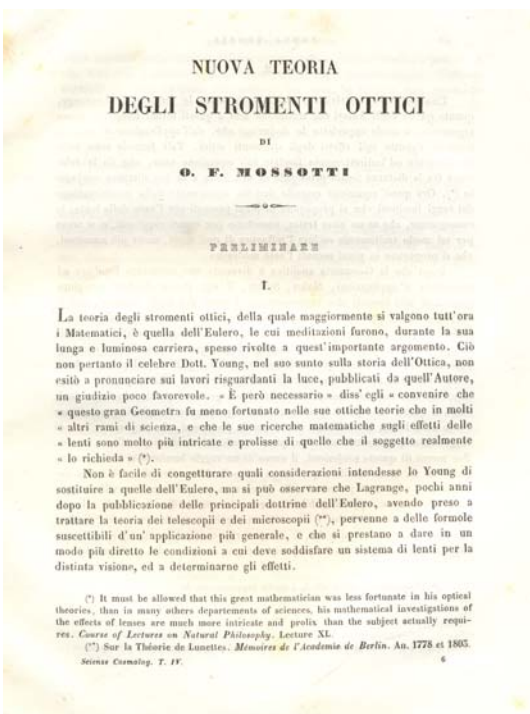
CALCUL DES PROBABILITÉS. — *Note sur la proportion des condamnations prononcées par les jurys;* par M. Poisson.

« Dans le préambule de mon ouvrage sur la *Probabilité des Jugements en matière criminelle et en matière civile*, j'ai cité l'accroissement continuel du nombre des accusés traduits annuellement devant les jurys, qui a eu lieu en Angleterre depuis 1805 jusqu'en 1832. D'après des documents authentiques, si l'on partage cet intervalle de 28 ans, en quatre périodes successives de chacune sept années, le nombre des accusés, pour l'Angleterre et le pays de Galles seulement, a été, terme moyen, d'à peu près 5000 dans chaque année de la première période, 6000 dans chaque année de la seconde, 9000 dans chaque année de la troisième, 13000 dans chaque année de la quatrième; et pendant la seule année 1832, la dernière de cette dernière période, il s'est élevé à près de 21000 (1). Le nombre annuel des condamnations a augmenté en même temps, mais plus rapidement que celui des accusés; le rapport moyen du premier nombre au second a été successivement un peu au-dessous de [illegible], un peu au-dessus de [illegible], un peu moindre que [illegible], et à très peu près [illegible], pour ces quatre

(1) Page 23 de mon ouvrage, ligne 15, en remontant, au lieu de 1817, *lisez* 1811.

C. R., 1837, 2e *Semestre*. (T. V, N° 10.) 48

Siméon Poisson
1781 – 1840



NUOVA TEORIA

DEGLI STROMENTI OTTICI

DI

O. F. MOSSOTTI

PRELIMINARE

I.

La teoria degli stromenti ottici, della quale maggiormente si valgono tutt'ora i Matematici, è quella dell'Eulero, le cui meditazioni furono, durante la sua lunga e luminosa carriera, spesso rivolte a quest'importante argomento. Ciò non pertanto il celebre Dott. Young, nel suo sunto sulla storia dell'Ottica, non esitò a pronunciare sui lavori risguardanti la luce, pubblicati da quell'Autore, un giudizio poco favorevole. « È però necessario » diss'egli « convenire che « questo gran Geometra fu meno fortunato nelle sue ottiche teorie che in molti « altri rami di scienza, e che le sue ricerche matematiche sugli effetti delle « lenti sono molto più intricate e prolisse di quello che il soggetto realmente « lo richieda » (*).

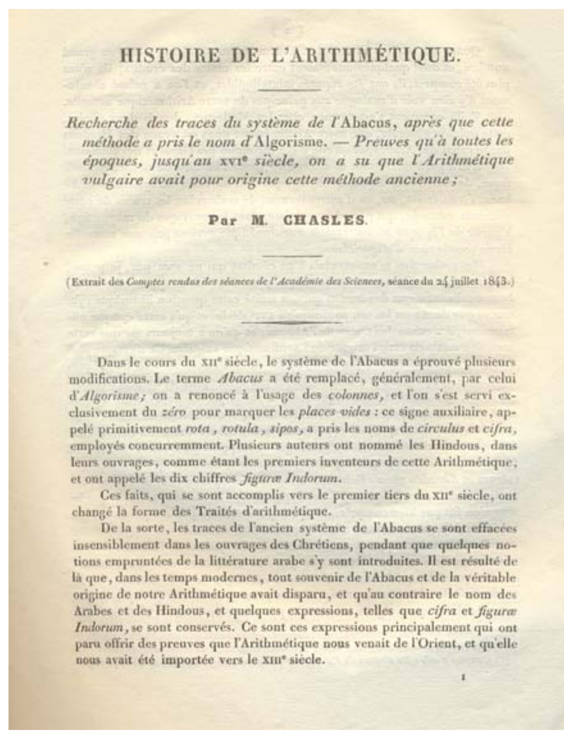
Non è facile di congetturare quali considerazioni intendesse lo Young di sostituire a quelle dell'Eulero, ma si può osservare che Lagrange, pochi anni dopo la pubblicazione delle principali dottrine dell'Eulero, avendo preso a trattare la teoria dei telescopii e dei microscopii (**), pervenne a delle formole suscettibili d'un'applicazione più generale, e che si prestano a dare in un modo più diretto le condizioni a cui deve soddisfare un sistema di lenti per la distinta visione, ed a determinarne gli effetti.

(*) It must be allowed that this great mathematician was less fortunate in his optical theories, than in many others departements of sciences, his mathematical investigations of the effects of lenses are much more intricate and prolix than the subject actually requires. *Course of Lectures on Natural Philosophy*. Lecture XL.

(**) Sur la Théorie de Lunettes. *Mémoires de l'Academie de Berlin*. An. 1778 et 1803.

Scienze Cosmolog. T. IV. 6

Ottaviano Mossotti
1791 – 1863



HISTOIRE DE L'ARITHMÉTIQUE.

*Recherche des traces du système de l'*Abacus, *après que cette méthode a pris le nom d'*Algorisme. *— Preuves qu'à toutes les époques, jusqu'au* XVIe *siècle, on a su que l'Arithmétique vulgaire avait pour origine cette méthode ancienne;*

Par M. CHASLES.

(Extrait des *Comptes rendus des séances de l'Académie des Sciences*, séance du 24 juillet 1843.)

Dans le cours du XIIe siècle, le système de l'Abacus a éprouvé plusieurs modifications. Le terme *Abacus* a été remplacé, généralement, par celui d'*Algorisme;* on a renoncé à l'usage des *colonnes,* et l'on s'est servi exclusivement du *zéro* pour marquer les *places vides* : ce signe auxiliaire, appelé primitivement *rota*, *rotula*, *sipos*, a pris les noms de *circulus* et *cifra*, employés concurremment. Plusieurs auteurs ont nommé les Hindous, dans leurs ouvrages, comme étant les premiers inventeurs de cette Arithmétique, et ont appelé les dix chiffres *figuræ Indorum*.

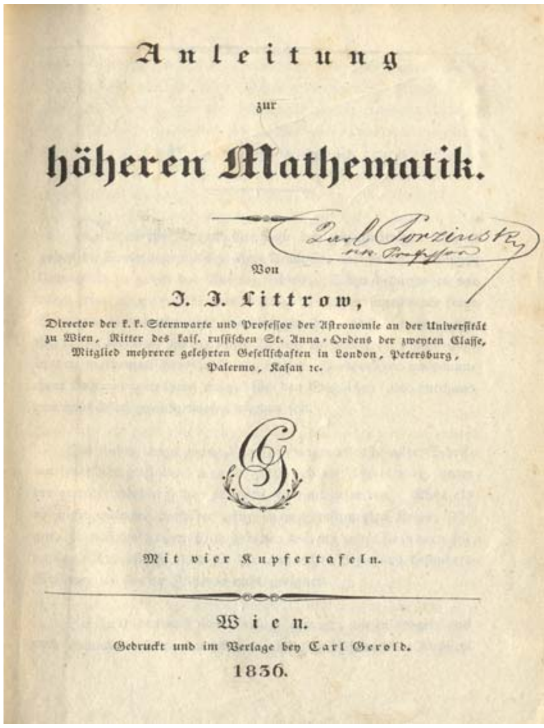
Ces faits, qui se sont accomplis vers le premier tiers du XIIe siècle, ont changé la forme des Traités d'arithmétique.

De la sorte, les traces de l'ancien système de l'Abacus se sont effacées insensiblement dans les ouvrages des Chrétiens, pendant que quelques notions empruntées de la littérature arabe s'y sont introduites. Il est résulté de là que, dans les temps modernes, tout souvenir de l'Abacus et de la véritable origine de notre Arithmétique avait disparu, et qu'au contraire le nom des Arabes et des Hindous, et quelques expressions, telles que *cifra* et *figuræ Indorum*, se sont conservés. Ce sont ces expressions principalement qui ont paru offrir des preuves que l'Arithmétique nous venait de l'Orient, et qu'elle nous avait été importée vers le XIIIe siècle.

1

Michel Chasles
1793 – 1880

Figure 8 (con't). Selected title pages.



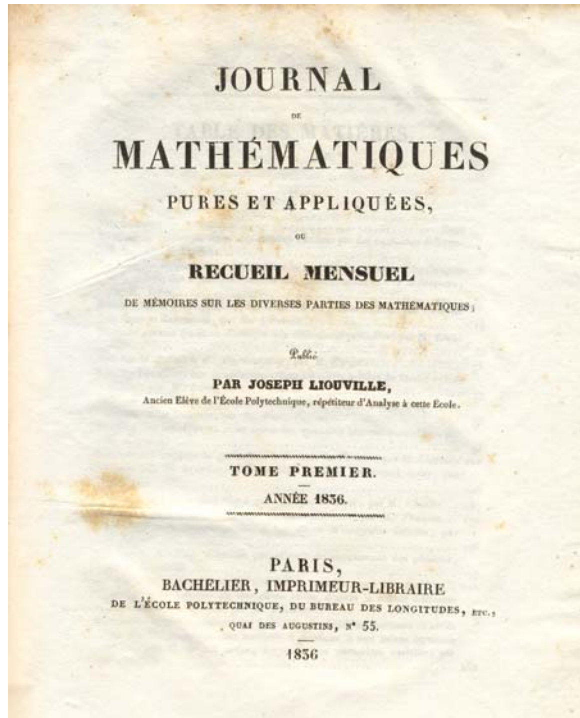
Anleitung zur höheren Mathematik.

Von J. J. Littrow, Director der k. k. Sternwarte und Professor der Astronomie an der Universität zu Wien, Ritter des kais. russischen St. Anna-Ordens der zweyten Classe, Mitglied mehrerer gelehrten Gesellschaften in London, Petersburg, Palermo, Kasan rc.

Mit vier Kupfertafeln.

Wien. Gedruckt und im Verlage bey Carl Gerold. 1836.

Joseph von Littrow
1781 – 1840



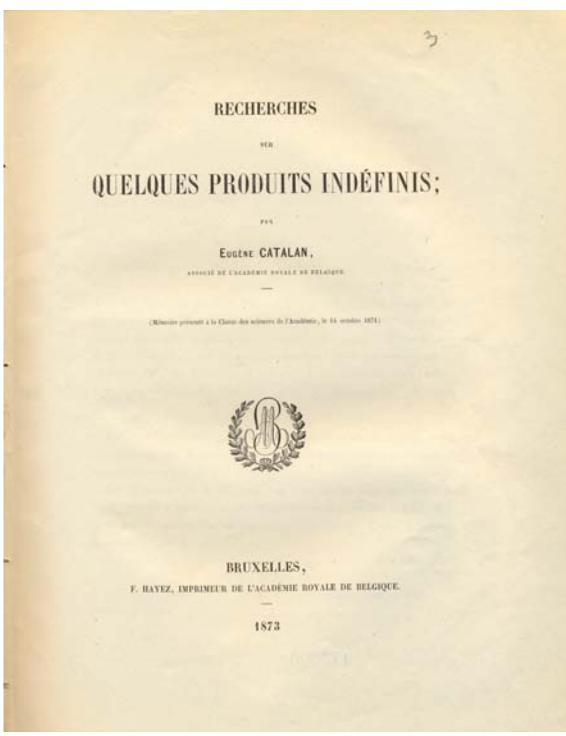
JOURNAL DE MATHÉMATIQUES PURES ET APPLIQUÉES, OU RECUEIL MENSUEL DE MÉMOIRES SUR LES DIVERSES PARTIES DES MATHÉMATIQUES;

Publié PAR JOSEPH LIOUVILLE, Ancien Élève de l'École Polytechnique, répétiteur d'Analyse à cette École.

TOME PREMIER. ANNÉE 1836.

PARIS, BACHELIER, IMPRIMEUR-LIBRAIRE DE L'ÉCOLE POLYTECHNIQUE, DU BUREAU DES LONGITUDES, ETC., QUAI DES AUGUSTINS, N° 55. 1836

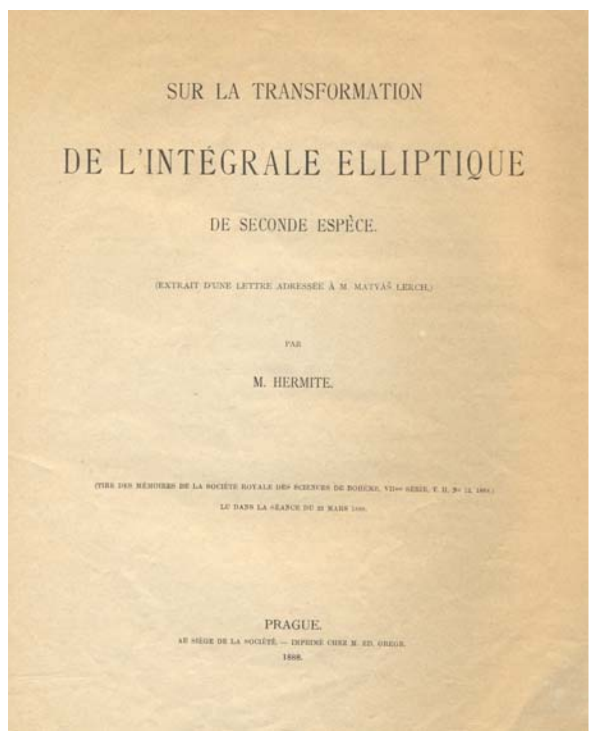
Joseph Liouville
1809 – 1882



RECHERCHES SUR QUELQUES PRODUITS INDÉFINIS; PAR EUGÈNE CATALAN, ASSOCIÉ DE L'ACADÉMIE ROYALE DE BELGIQUE.

BRUXELLES, F. HAYEZ, IMPRIMEUR DE L'ACADÉMIE ROYALE DE BELGIQUE. 1873

Eugène Catalan
1814 – 1894



SUR LA TRANSFORMATION DE L'INTÉGRALE ELLIPTIQUE DE SECONDE ESPÈCE.

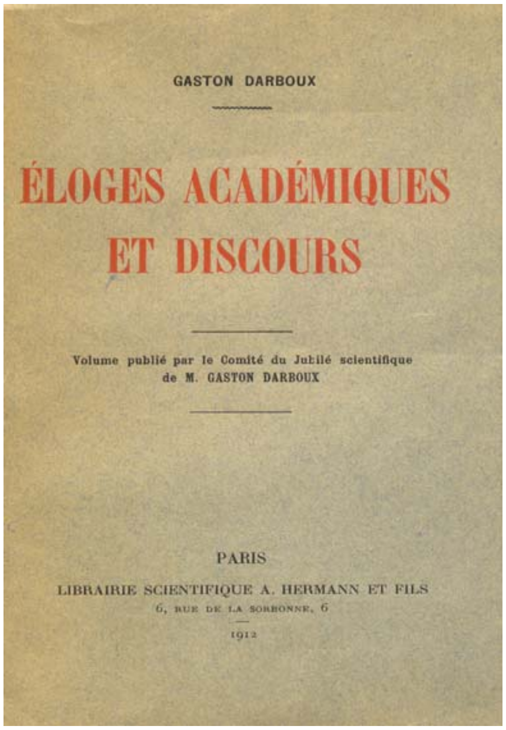
(EXTRAIT D'UNE LETTRE ADRESSÉE À M. MATYÁŠ LERCH.)

PAR M. HERMITE.

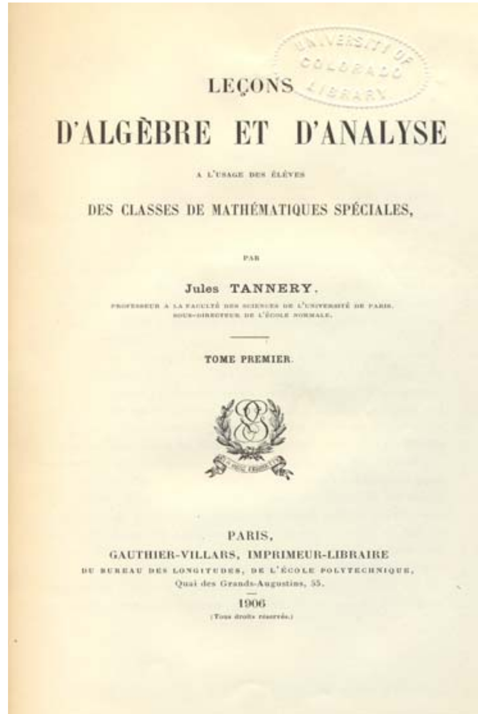
PRAGUE. 1888.

Charles Hermite
1822 – 1901

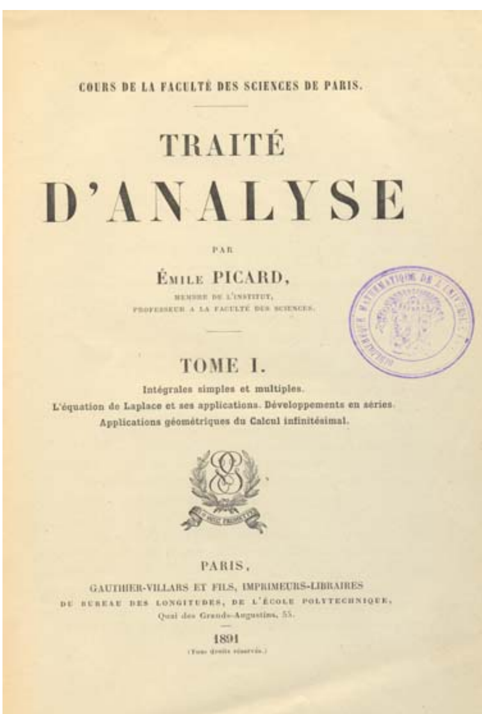
Figure 8 (con't). Selected title pages.



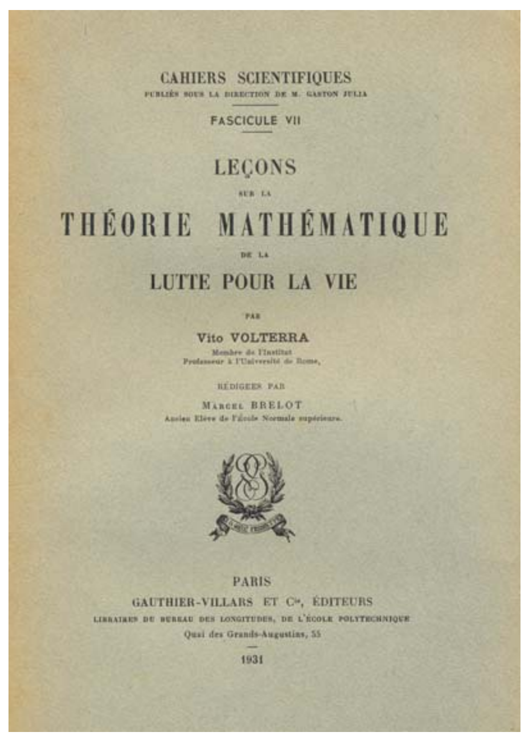
Gaston Darboux
1842 – 1917



Jules Tannery
1848 – 1910

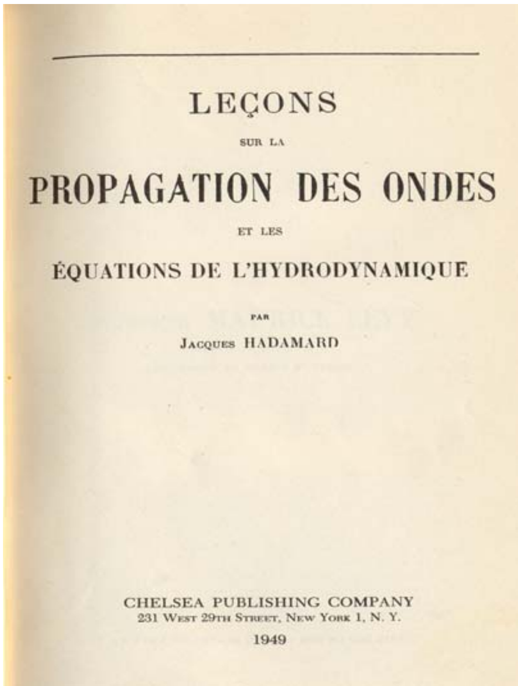


Charles Émile Picard
1856 – 1941

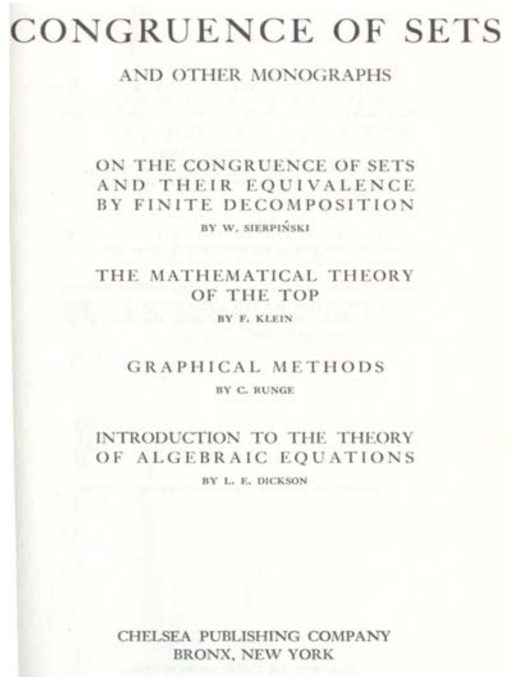


Vito Volterra
1860 – 1940

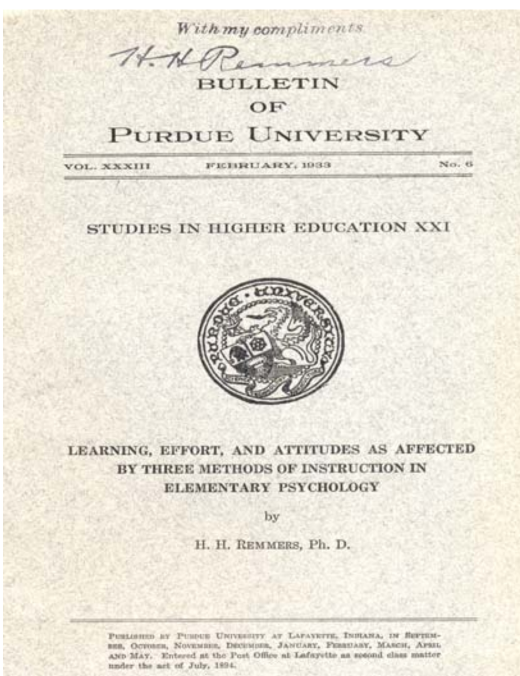
Figure 8 (con't). Selected title pages.



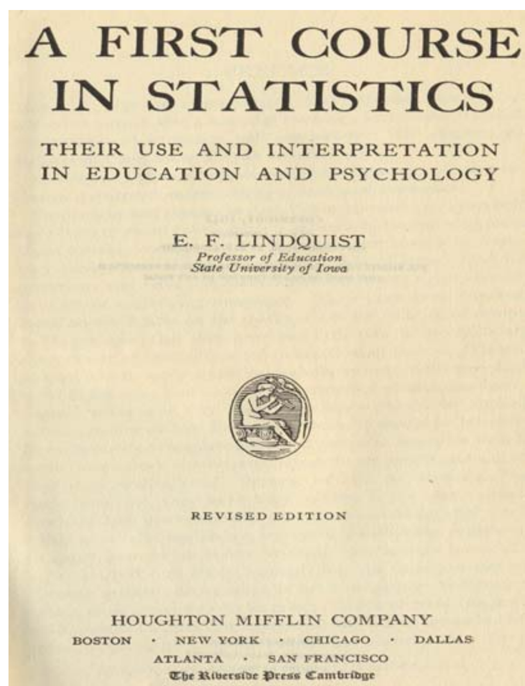
Jacques Salomon Hadamard
1865–- 1963



Wacław F. Sierpiñski
1882 – 1969



Hermann H. Remmers
1892 – 19??



E. F. Lindquist
1914 – 1998

*Scanned from the personal library of Shlomo S. Sawilowsky

Figure 9. Philatelic, Numismatic, and Bank Note Images
from the Direct and Broader Academic Genealogy of R. Clifford Blair*

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

Gottfried Wilhelm von Leibniz
1646 – 1716

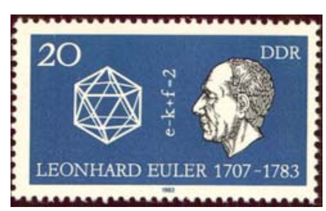

Gottfried Wilhelm von Leibniz
1646 – 1716

Jacob Bernoulli
1654 – 1705

Leonhard Euler
1707 – 1783

Leonhard Euler
1707 – 1783

Leonhard Euler
1707 – 1783



Leonhard Euler
1707 – 1783

Figure 9 (con't): Philatelic, Numismatic, and Bank Note Images.

 Leonhard Euler 1707 – 1783	 Leonhard Euler 1707 – 1783	 Joseph-Louis Lagrange 1736 – 1813
 Gaspard Monge 1746 – 1818	 Gaspard Monge 1746 – 1818	 Pierre-Simon Laplace 1749 - 1827
 Pierre-Simon Laplace 1749 - 1827	 André Marie Ampère 1775 - 1836	 André Marie Ampère 1775 – 1836
 Evariste Galois 1811 – 1832	 Urbain J. J. Le Verrier 1811 – 1877	 Urbain J. J. Le Verrier 1811 – 1877
 Pafnuty Chebyshev 1821 – 1894	 Pafnuty Chebyshev 1821 – 1894	 Pafnuty Chebyshev 1821 – 1894
 Jules Henri Poincaré 1854 - 1912	 Stanislaw Zaremba 1863 – 1942	 Wacław F. Sierpiñski 1882 – 1969

*Scanned from the personal collection of Shlomo S. Sawilowsky and from internet sources (see references below).

Figure 10. R. Clifford Blair, early mentor, former doctoral students, and former graduate assistants.

John H. Neel
Early Mentor

Shlomo S. Sawilowsky
Former Doctoral Student,
Graduate Teaching Assistant, and
Graduate Research Assistant

Theodore Micceri
Former Doctoral Student and
Graduate Teaching Assistant

R. Clifford Blair
Karen N. Perrin, former Graduate Assistant
Richard A. Taylor, former Graduate Assistant

Table 2. Descendents in the academic genealogy of R. Clifford Blair, including doctoral candidates at the dissertation stage, as of January, 2005.

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1. R. Clifford Blair. (Ph. D.), A comparison of the power of the two independent means t test to that of the Wilcoxon's Rank-Sum Test for samples of various sizes that have been drawn from a variety of non-nonmal populations." 131 pp., 1980.

Doctoral Students of R. Clifford Blair

2. Shlomo S. Sawilowsky. (Ph. D.), "Robust and power analysis of the 2×2×2 ANOVA, rank transform, random normal scores, and expected normal scores transformation tests." 159 pp., 1985.

3. Theodore Micceri. (Ph.D.), "Testing for normality and evaluating the relative robustness of location estimators for empirical distributions derived from achievement tests and psychometric measures." 239 pp, 1987.

Doctoral Students of Shlomo S. Sawilowsky

4. Joyce Washington. (Ed. D.), "Health education and measuring the effects of minority student self-concept as it relates to school performance." 104 pp., 1993.

5. Sharonlyn Morgan-Harrison. (Ph. D.), "Some construct validation evidence for two new measures of self-determination." 89 pp., 1994.

6. Deborah L. Kelley. (Ph. D.), "The comparative power of several nonparametric alternatives to the analysis of variance in a 2x2x2 layout." 214 pp., 1994.

7. Dennis J. Mullan. (Ph. D.), "An investigation of a residential customer satisfaction model at an electric utility." 102 pp., 1995.

8. Uju P. Eke. (Ph. D.), "A construct validation of a Self-Determination instrument: Using adult substance abuse consumers in residential settings." 79 pp., 1996.

9. Patrick D. Bridge. (Ph. D.), "The comparative power of the independent-samples t-test and Wilcoxon rank sum test in nonnormal distributions of real data sets in education and psychology." 113 pp., 1996.

10. Margaret P. Posch. (Ph. D.), "Comparative properties of nonparametric statistics for analyzing the 2xc layout for ordinal categorical data." 78 pp., 1996.

11. Thilak Gunasekera. (Ph. D.), "Effects of pretest sensitization associated with cooperative learning strategies on the achievement level of adult mathematics students." 97 pp., 1997.

12. Todd C. Headrick. (Ph. D.), "Type I error and power of the rank transform analysis of covariance (ANCOVA) in a 3x4 factorial layout." 355 pp., 1997.

13. Michael J. Nanna. (Ph. D.), "Analysis of Likert scale data in disability and medical rehabilitation research." 220 pp., 1997.

14. Anil N. F. Aranha. (Ph. D.), "Modeling self-determination among the elderly: A psychometric study of health care decision-making." 102 pp., 1998.

15. William Cade. (Ph. D.), "Sampling procedures and Type I error rates (for nonnormal populations)." 81 pp., 1998.

16. Cynthia Creighton. (Ph. D.), "Critical thinking skills and learning styles of first-year students in weekend occupational therapy programs." 80 pp, 1999.

17. Michael Wolf-Branigin. (Ph. D.), "Point pattern analysis in measuring physical inclusion of people with developmental disabilities." 182 pp., 1999.

18. Gail Fahoome. (Ph. D.), "A Monte Carlo study of twenty-one nonparametric statistics with normal and nonnormal data." 519 pp., 1999.

19. Joe Musial. (Ph. D.), "Comparing exact tests and asymptotic tests with colorectal cancer variables within the National Health and Nutrition Examination Survey III." 189 pp., 1999.

20. Juanita M. Lyons. (Ph. D.), "Methodology for the determination of the reliability of database derived data." 115 pp., 2000.

22. Jim Gullen.(Ph. D), "Goodness of fit indices as a one factor structural equation model." 61 pp., 2000.

23. Karen Crawforth. (Ph. D.), Measuring the interrater reliability of a data collection instrument developed to evaluate anesthetic outcomes." 144 pp., 2001.

24. Scott Compton (Ph. D.), "Type I error and power properties of seven two-sample tests when treatment affects location and scale." 276 pp., 2001.

25. Kathy R. Peterson. (Ph. D.), "A study of six modifications of the ART (aligned rank transform) used to test for interaction." 361 pp., 2001.

26. Rimma Novojenova. (Ed. D.), "Measurement of teacher's personalization in the classroom environment." 152 pp., 2002.

27. Jennifer M. Bunner. (Ph. D.). "Forming a bracketed interval around the trimmed mean: Alternatives to S_w. 112 pp., 2003.

28. Bruce R. Fay (Ph. D.), "A Monte Carlo computer study of the power properties of six distribution-free and/or nonparametric statistical tests under various methods of resolving tied ranks when applied to normal and nonnormal data distributions." 528 pp, 2003.

29. Karen Lee (Ph. D.), "Parametric and nonparametric IRT models for assessing differential item functioning."113 pp., 2003.

30. Stephaine Krol-Jersevic. (Ed. D.), "Measuring oral communication apprehension in children." 89 pp., 2004.

31. Jack Hill (Ph. D.). "The effects of pseudorandom number generator and initial seed selection on Monte Carlo simulations." 251 pp., 2005.

32. Amittai ben Ami. (doctoral candidate)
33. Holly Atkins. (doctoral candidate)
34. Tana Bridge. (doctoral candidate)
35. Dave Fluharty. (doctoral candidate)
36. Roberta Foust. (doctoral candidate)
37. Kalvin Holt. (doctoral candidate)
38. Kevin Lawson. (doctoral candidate)
39. Saydee Mends-cole. (doctoral candidate)
40. Kundisai Ndhelela (doctoral candidate)
41. Bulent Ozkan. (doctoral candidate)
42. Patricia Pelavin. (doctoral candidate)
43. Candice Pickens. (doctoral candidate)
44. Carol Piesko. (doctoral candidate)
45. Andree' Sampson (doctoral candidate)
46. Lori Shingledecker. (doctoral candidate)
47. Boris Shulkin. (doctoral candidate)
48, Piper Farrell-Singleton (doctoral candidate)
49. Andrew Tierman. (doctoral candidate)
50. Michele Weber. (doctoral candidate)
51. Keith Williams. (doctoral candidate)

Doctoral Cognate (2nd advisor) Student

52. Mary Pratt Cooney. (Ph. D.), "Process drama and actor training." 130 pp., 1999.

Doctoral Student of Todd C. Headrick

53. Simon Y. Aman. (Ph.D.), "An empirical investigation of nonparametric alternatives of Hottelling's T^2 under non-normality." 2005.

Doctoral Students of Gail Fahoome

54. Franklin Harrell. (doctoral candidate)
55. Sia Robinson. (doctoral candidate)

Biographical Sketches

Brief descriptions of members of R. Clifford Blair's academic genealogy are provided below. Information in these synopses was obtained from a variety of sources, including Abailard and Berg (1970). (Considerable material from that reference is available verbatim in the online MacTutor History of Mathematics.) Other references included Burton (1997), James (2002), Temple (1981), and the Mathematics Genealogy Project (http://www.genealogy.ams.org/).

André Marie Ampère's (1775 – 1836) biographical sketch appears here even though he is not in the direct academic lineage, because he was an influential instructor of Joseph Liouville, who took his course in mechanics at École Polytechnique and later his course in electrodynamics at the Collège de France. Ampère is primarily known for his work in chemistry and physics (e.g., light, heat, magnetism, electricity). However, he conducted considerable research in probability, which led to *The Mathematical Theory of Games*, and also a text on the calculus. In 1814, he was elected to the Institut National des Sciences. This was a remarkable honor for the home-schooled and non-degreed Ampère, as he was elected over Augustin Louis Cauchy (1789 – 1857), one of the greatest mathematicians of the 19th century.

Dominique François Jean Arago's (1786 – 1853) biographical sketch appears here even though he is not in the direct academic lineage, because he was an influential instructor of Joseph Liouville. Arago was a Professor of Analytical Geometry at the École Polytechnique, and subsequently became Director of the Paris Observatory. Along with Louis Paul Émile Richard, one of his students was Urbain Jean Joseph Le Verrier (1811 – 1877). His research was on light, electricity, and magnetism. He served many years as the Secretary of the Académie des Sciences.

Jacob (Jacques or **James) Bernoulli** (1654 – 1705), following the wishes of his parents, reluctantly studied philosophy at the University of Basel and obtained the Master's in 1671, and then earned the licentiate in theology in 1676. After graduating, his travels led him to studying mathematics with Robert Boyle (1627 – 1691), Robert Hooke (1635 – 1703), Johann van Waveren Hudde (1628 – 1704), and Nicolas Malebranche (1638 – 1715). He started a private school for mathematics in Basel in 1682, and the following year he obtained a teaching position in mechanics at the University in Basel. He became Professor and Chair of Mathematics there in 1687. His early publications were on logic, algebra, and geometry. When his younger brother Johann sought his assistance in the study of mathematics, Jacob became a disciple of Leibniz. He published extensively in the newly established *Acta Eruditorum*, expounding on the calculus of Leibniz. Bernoulli's name is associated with the famous law of large numbers that is pervasive in probability theory. Bernoulli numbers made their appearance posthumously in *Ars Conjectandi* published in 1713, which contained the fundamentals of permutation and combinatorial theory.

Johann (John) Bernoulli (1667 – 1748), as with his brother Jacob, reluctantly followed his parent's wishes, and was employed in the family business as a salesman. He approached his brother to tutor him in mathematics. In 1695, he was appointed Professor of Mathematics at Groningen. Upon the demise of Jacob in 1705, he assumed the Professorship and Chair in Mathematics at Basel. Along with his brother Jacob, Johann published extensively in *Acta Eruditorum* on the calculus of Leibniz. Some work attributable to Johann was published in the name of his employer, Guillaume François Antoine, the Marquis de L'Hôpital (1661 – 1704). An example is the limit theorem commonly called L'Hôpital's rule. In Johann's correspondence with Leibniz, the phrase "integral calculus" was coined, and Johann adapted his brother's prior use of the elongated "s" for the integral symbol "$\int$." Later in his life, Johann was to help convince the parents of one of his students that their son should pursue mathematics instead of theology. That student was Leonhard Euler.

Enrico Betti (1823 – 1892) was a student of Ottaviano Mossotti at the Università di Pisa, and succeeded him in 1864 as the Chair of Mathematical Physics. Betti obtained his doctorate in 1846. He was a secondary school teacher, and later served at Università di Pisa as a faculty member and Rector. He was also the Director of the teaching college at Scuoloa Normale Superiore, Pisa. In addition to Vito Volterra, another one of his students was Luigi Bianchi (1856 – 1928). Betti played an important role in the development of mathematics in schools in the new Kingdom of Italy, translating classical texts (e.g., Euclid's *Elements*) into Italian, and similarly, in the world-wide transition from classical to modern algebra. His research interests were in algebra and topology. His 1871 topology work, which benefited from correspondence with Bernhard Riemann (1826 – 1866), provided the basis for what are called Betti numbers. Betti's theorem, a law of reciprocity in elasticity theory, was developed in 1878. He was Undersecretary of State for Education in 1874, and served as Senator in the Italian parliament in 1884.

Raymond Clifford Blair obtained his Bachelor's degree in International Studies in 1970, Master's of Arts in Gerontology the following year, and the Ph.D. in Measurement and Research in 1979, at the University of South Florida (USF). He became an Instructor at USF in 1976, and then accepted a position as an Assistant Professor in Evaluation and Research in 1979. He rose through the ranks, and became a full Professor in 1984. In 1987, he accepted the position of Coordinator of Measurement, Research, and Statistics, and Associate Professor at The Johns Hopkins University. He returned to USF the following year, accepting the joint position of Associate Professor in the Department of Pediatrics, College of Medicine, and the Department of Epidemiology/ Biostatistics, College of Public Health. He was promoted to full Professor in the Department of Epidemiology and Biostatistics in 1997. He served as Deputy Chair from 1997 - 2000, and Interim Chair from 2000 - 2002. He was appointed Professor Emeritus in 2004. In 1993, he was awarded a grant by the IBM Corporation to develop pseudo-random number generators for the IBM RT PC computer. He published 70 articles, which appear in Table 1. His theoretical research was primarily on nonparametric rank tests, permutation statistics, multivariate statistics, and multiple comparison procedures. He published applied articles in biostatistics, public health, and medicine. Along with Shlomo S. Sawilowsky, his former doctoral student, he won the 1986 Distinguished Researcher Award of the Florida Educational Research Association and a 1987 Distinguished Paper, State and Regional Associations, of the American Educational Research Association. He won the 1995 and 1998 Distinguished Teacher awards of the USF Public Health Student Association. In 1996, he was honored as the USF Outstanding Teacher.

Julius Rubin Blum (1922 – 1982), in his youth, was sent by his parents from Germany to the United States. They perished in the Nazi holocaust before they could follow. He attended the University of California, Berkeley, was a member of Phi Beta Kappa, and obtained the Ph. D. in 1953. Officially, he was a student of Michel Loève. According to Professor Jane-Ling Wang, "Le Cam was Blum's thesis adviser in reality, but the university did not allow him to be the official adviser as they had been concurrent students at Berkeley. Le Cam graduated before Blum and supervised his thesis. Le Cam told me, and many others, this interesting story" (personal communications. Dr. Wang is Professor of Statistics, University of California, Davis (UCD), and received the Ph. D. in 1982 as a student of Le Cam at the University of California, Berkeley). Blum took a faculty position at UCD, and became the Chair of the Department of Statistics. In 1963, he became Professor and Chair of Mathematics at the University of New Mexico. In 1974, he joined the mathematics faculty at University of Wisconsin, Milwaukee. He returned to UCD as Associate Dean in 1979. His research interests, over 80 publications, were in stochastic approximation, multivariate generalization, ergodic theory, and nonparametric statistical inference. He co-authored the popular textbook *Probability and Statistics* in 1972 (with Professor Judah I. Rosenblatt, formerly of Case Western Reserve University and now with the University of Texas Medical Branch), which is available online at:
http://www.bioinfo.utmb.edu/rosenblatt/index.html.

Charles Bossut (1732 – 1806), a student of d'Alembert, was a Professor of Mathematics at Mézières, and then a Professor of Hydrodynamics at the Louvre. His two textbooks on mathematics and mechanics were widely used. He was awarded several prizes by the Académie des Sciences, and was elected member in 1768. In addition to Gaspard Monge, his students included Jean Charles de Borda (1733 – 1799) and Charles Augustin de Coulomb (1736 – 1806).

Nikolai Dmetrievich Brashman (1796 – 1866) was a teacher of mathematics at the University of Kazan, before accepting the position of Professor of Applied Mathematics in Moscow in 1834. He won the Demidov Prize from the Russian Academy of Sciences in 1836 for work in mechanics and mathematics. He founded the Moscow Mathematical Society.

Del Cavaliere Vincenzo Brunacci's (1768 – 1818) early mathematics training was under Stanislao (Sebastiano) Canovai (1740 – 1811). In 1785, he studied medicine at the University of Pisa. His mathematics instructor was Pietro Paoli. In 1788, he received a degree in medicine. He was appointed Professor of Nautical Mathematics in 1790. He joined the faculty at the University of Pavia in 1801, and eventually became its Chancellor. He published many books and articles, primarily on analysis and integral calculus. In 1806, he was awarded Knight of the Iron Crown and Inspector of Waters and Roads, and was elected to the Italian Society of Sciences. He became Inspector General of Public Education of Italy.

Lucien Le Cam (1924 – 2000) was an applied statistician working at Electricité de France for five years, and he was a graduate student at the Sorbonne in 1948, when Jerzy Neyman brought him to the University of California, Berkeley. Le Cam promptly flunked his doctoral qualifying exam. This humble beginning masked achievements he was to obtain in a career spanning about a half century at the University. After completing the Ph. D. in 1952, he was hired as an Instructor, rose through the ranks to full Professor of Statistics in 1960, and served as the Chair from 1961 – 1965. He published about 90 articles on topics relating to maximum likelihood, statistical decision functions, stochastic processes, asymptotic normal distributions, and applied cancer research. He co-edited a number of publications with Neyman (e. g., *Bernoulli-Bayes-Laplace Anniversary Volume* in 1965, *Proceedings of the Berkeley Symposium on Mathematical Statistics and Probability* in 1967 and 1972), and was the Associate Editor of *Zeitschrift für Wahrscheinlichkeits-theorie u. v. Gebiete* and *Polish Journal of Probability and Mathematical Statistics*. Among his students were Grace Lo Yang, Stephen Mack Stigler, and Jane-Ling Wang. Le Cam was President of the Institute of Mathematical Statistics in 1973, and was elected to the American Academy of Arts and Sciences (1976) and the American Association for the Advancement of Science (1977).

Eugène Charles Catalan (1814 – 1894) was a student of Joseph Liouville at École Polytechnique, but was expelled in 1833. He returned in 1835, and after graduating, accepted a faculty position at the Châlons sur Marne. He returned to Polytechnique as a Lecturer in 1838. He assisted Liouville in producing the *Journal de Mathématiques*. His solution to dissecting a polygon into triangles led to the discovery of Catalan numbers.

Michel (Floréal) Chasles (1793 – 1880), following a failed attempt at becoming a stockbroker, published a book in 1837 on the history of geometry. He became a professor at École Polyte Académie in 1841, teaching astronomy, geodesy, and mechanics. Chasles obtained a simultaneous appointment as Chair of Higher Geometry at the Sorbonne in 1846. He published on projective geometry, conic sections, and synthetic geometry, emphasizing the history of mathematics. Hubert Anson Newton (1830 – 1896) was his student, whose student was E. H. (Eliakim Hastings) Moore (1862 – 1932), whose students were George David Birkhoff (1884 – 1944) and Oswald Veblen (1880 – 1960). Chasles was elected to the Académie des Sciences in 1851, a Fellow of the Royal Society of London in 1854, and to the London Mathematical Society in 1867. He was awarded the Copley Medal in 1865.

Pafnuty Lvovich Chebyshev (Tchebychev or **Tschebyshew)** (1821 – 1894) was lame and had a speech impediment. This was no obstacle to a brilliant career. He obtained his undergraduate degree in mathematics from Moscow University in 1841, his Master's in 1846, and his doctorate in 1849. His first two degrees were influenced by his mentor, Nikolai Brashman. His published on multiple integrals, Taylor series, law of large numbers, integration by logarithms, number theory, prime numbers, and orthogonal polynomials. He generalized the beta function, and his name is associated with Chebyshev polynomials and the Bienaymé-Chebyshev inequality, today referred to as the Chebyshev inequality. In addition to Andrei Markov, another of his students was Aleksandr Mikhailovich Lyapunov (1857 – 1918), whose student was Vladimir Andreevich Steklov (1864

– 1926). Cheybyshev was given the title of Extraordinary Academician by the St Petersburg Academy of Sciences in 1856, elected to the Société Royale des Sciences of Liège in 1856, the Société Philomathique in 1856, the Berlin Academy of Sciences in 1871, the Bologna Academy in 1873, the Royal Society of London in 1877, the Italian Royal Academy in 1880, and the Swedish Academy of Sciences in 1893. Among many other prizes and titles, Chebyshev was awarded the French Légion d'Honneur.

Jean Le Rond d'Alembert (1717 – 1783) came from the classic ignoble beginning, as he was an illegitimate child left on the doorsteps of an orphanage. Fortunately, his identity was not kept secret, and while his father was alive he supplied financial support, which was until d'Alembert was nine years old. He was educated at the Jansenist Collège des Quatre Nations. He was admitted to the Paris Academy of Science in 1741. One of d'Alembert's major achievements was co-editing the 28 volume *Encyclopédie Diderot et d'Alembert* with Denis Diderot (1713 – 1784).

Jean Gaston Darboux (1842 – 1917) received his Ph. D. in Mathematics from École Normale Supérieure in 1866. He held academic posts at Collège de France in 1866, Lycée Louis le Grand the following year, École Normale Supérieure in 1872, and at the Sorbonne beginning in 1873. He taught higher geometry, became the Chair in Geometry in 1880, and Dean of the Faculty of Science from 1889 – 1903. His primary area of research was in differential geometry, but he also published on topics in algebra, function theory, and kinematics and dynamics. The Darboux integral bears his name. In 1884, he was elected to the Académie des Sciences, and in 1902 to the Royal Society of London. He was awarded the Sylvester Medal (James Joseph Sylvester, 1814 – 1897, founder of the *American Journal of Mathematics*) in 1916.

Robert L. Ebel obtained his Master's and Ph. D. from the University of Iowa. He was a high school teacher for nine years, and a school principal for three years. His was on the faculty of the University of Iowa from 1947 – 1957. He was a Vice President at the Educational Testing Service in Princeton, New Jersey, from 1957 – 1963. He returned to academia in 1963 at the Michigan State University (MSU), with an appointment to the faculty of Educational Counseling and Psychology, and also served as Assistant Dean. He authored numerous articles and textbooks in educational measurement, testing, and psychometric theory. He was the Editor of the *Encyclopedia of Educational Research* published by the American Educational Research Association (AERA). He was elected President of the National Council on Measurement in Education in 1957, and President of Division 5 (Evaluation, Measurement and Statistics) of the American Psychological Association in 1971. He won the AERA – American College Testing Program (ACT) "E. F. Lindquist Award" in 1989. His name is associated with a $6,000 MSU College of Education Endowed Scholarship.

Leonhard Euler (1707 – 1783) obtained his doctorate at the Universität Basel in 1726 under Johann Bernoulli. The Euler and Bernoulli families were long time friends. Leonhard's father was a collegiate classmate of Johann Bernoulli; when Euler attended university at the age of 14, Johann provided him with reading lists. Later, when Leonhard accepted his first post at the St. Petersburg Academy in Russia (offered after the demise of Nicolaus Bernoulli, II, 1695 – 1726), he resided with Daniel Bernoulli (1700 – 1782). After a seven year stint in the Russian navy, Leonhard developed severe health problems, losing one eye and having poor vision in the other. Nevertheless, he won the 1738 and 1740 Grand Prize of the Paris Academy. Due to the Russian political climate, Euler left for the Berlin Academy of Sciences in 1741, where he published over 375 articles and books. He returned to St. Petersburg in 1766, by which time he was totally blind. This had little effect on his productivity, as he continued to publish almost as many manuscripts as he had prior to losing his vision, making him perhaps the most published mathematician in history. He wrote seminal articles on calculus, differential geometry, and number theory. He developed the discipline of mathematical analysis and laid the foundation of analytical mechanics. He discovered the beta and gamma functions. The notation of "f(x)" for a function, "*e*" for the base of natural logarithms, "i" for the imaginary number representing $\sqrt{-1}$, "π" for pi, "Σ" for summation, and many more were due to Euler. He also published important works in astronomy, cartography, mechanics, and fluid mechanics. In 1739, he published a delightful, but complex treatise on the relationship between mathematics and music.

Jacques Salomon Hadamard (1865 – 1963) received his Docteur ès Sciences in 1892 at the École Normale Supérieure. Emile Picard and Jules Tannery are indicated as his doctoral advisors, but he also took courses with Jean Gaston Darboux, Paul Emile Appell (1855 – 1930), and Edouard Jean-Baptiste Goursat (1858 – 1936). Hadamard was initially a school teacher, and later served on the mathematics faculty at Lycée Saint-Louis, Lycée Buffon, University of Bordeaux, Sorbonne, Collège de France, École Polytechnique, and finally, École Centrale des Arts et Manufactures. He published books on dimensional geometry, functional analysis, linear partial and hyperbolic differential equations, and about 300 scientific and pedagogy articles and books for general audiences. His research achievements included proving the famous prime number theorem, the most important result in number theory. In addition to Paul Lévy, his students included Maurice René Fréchet (1878 – 1973) and Szolem Mandelbrojt (1899 – 1983, who succeeded Hadamard at the Collège de France). Hadamard received the Bordin Prize of the Academy of Sciences in 1896, and the Prix Poncelet Prize in 1898. He was a member of the Academy of Sciences of the United States, the Royal Society of London, the Accademia dei Lincei, and the Soviet Accademy of Sciences. He was elected President of the French Mathematical Society in 1906, and the Academy of Sciences in 1912.

Bruce Wendell Hall is Professor Emeritus in Educational Measurement and Research in the College of Education at the University of South Florida. He obtained his Ed. D. from Florida State University in 1969. He was appointed to the faculty at USF later that year, and rose through the ranks to full Professor in 1979. He served as Chair of Educational Measurement and Research from 1976 to 1982 and again from 1990 to 2002. In addition to R. Clifford Blair, Hall chaired 30 students' doctoral dissertations. He published 34 articles, made 142 paper presentations, and wrote 73 technical reports on educational research methods, instrument development, test reliability and validation, teacher attitudes, teacher attributions, teacher efficacy beliefs, classroom assessment, and school violence. He co-edited a volume on school testing programs published by the National Council on Measurement in Education (NCME) in 1976. He was twice elected President of the Florida Educational Research Association (1987 and 2003). He won the USF Provost's Award in 1996, and the USF Professorial Excellence Award in 1998.

David Lee Hanson obtained the B. S. from the Massachusetts Institute of Technology, and the M. A. and Ph. D (1960) from Indiana University. His first position was with the IBM Research Center. Subsequently, he was employed at the Sandia Corporation until 1963, when he was appointed to the faculty of the Department of Statistics, University of Missouri – Columbia and the Department of Mathematics. He rose through the ranks to full Professor in 1967, and became Department Chair of Statistics in 1971. He joined the Department of Mathematical Sciences, State University of New York at Binghamton in 1973. He was Department Chair for 16 years, and currently is Professor of Probability and Mathematical Statistics. He was Program Director for Probability and Statistics at the National Science Foundation in 1979. In addition to James J. Higgins, his former doctoral students include Ralph P. Russo. Hanson's publications include work on ergodic theory, the behavior of sums of random variables, Wiener processes (Norbert Wiener, 1894 – 1964), stochastic approximation, the theory of risk aversion, concave and monotonic regression, and hazard rates. He was an Associate Editor of *Annals of Mathematical Statistics*, *Annals of Probability*, and *Annals of*

Statistics. Hanson was elected Fellow of the Institute of Mathematical Statistics in 1966.

Charles Hermite (1822 – 1901) was a student of Louis Paul Émile Richard from 1840 – 1841 at the Collège Louis-le-Grand, who called him "un petit Lagrange." Hermite was privately tutored by Eugene Catalan from 1841 – 1842. He was initially dismissed from École Polytechnique due to a physical disability that required him to walk with a cane, and graduated in 1847 elsewhere with a Baccalauréat. He returned to Polytechnique as a member of the faculty in 1848 where he remained until 1876. In 1856, he barely survived after having contracted small pox. He had a simultaneous appointment at the Sorbonne beginning in 1869. His primary contributions were in number theory, orthogonal polynomials, elliptics, and quadratic forms. In 1873, he proved *e* is a transcendental number. In addition to his doctoral students Jules Tannery and Henri Jules Poincaré, he taught Paul Emile Appell (1855 – 1930), Félix Edouard Justin Emile Borel (1871 – 1956), Marie Ennemond Camille Jordan (1838 – 1922), Paul Painlevé (1863 – 1933), as well as Darboux, Hadamard, and Picard. Hermite was elected to the Paris Academy in 1850, and to the Académy of Sciences in 1856. His name is associated with Hermite polynomials, Hermite differential equations, and Hermitian matrices.

Albert N. Hieronymus obtained his Master's (1946) and Ph. D. (1948) from the University of Iowa. He was a member of the Phi Delta Kappa honor society for over a half century. He became Professor Emeritus in 1987 at the University of Iowa, culminating his academic career that began at the College of Education in 1948. He became the second director of the Iowa Basic Skills Testing Program in 1948. He focused on infusing technology into standardized testing. He authored over 35 major standardized tests. His research areas were in learning theory, test development, and test validation. He was awarded the National Council on Measurement in Education (NCME) Career Award in 1991.

James J. Higgins obtained the Ph. D. in Statistics at the University of Missouri-Columbia, in 1970. His first academic post was at the University of Missouri-Rolla, followed by his appointment at the University of South Florida from 1974 – 1980. Subsequently, he joined the faculty at Kansas State University in 1980, and is a full Professor. He served as the Head of the Department of Statistics from 1990 – 1995. His areas of theoretical research include mathematical statistics, nonparametric statistics, and reliability and life-testing. He also has published applied work on statistical education, correlated single subject designs, visitation patterns of animal foraging, and stochastic models for the synthesis of chemical compounds in red blood cells. To date, he has published a textbook on stochastic modeling and probability, a textbook on nonparametric statistics, and about 85 articles. In addition to serving as doctoral advisor to R. Clifford Blair and doctoral cognate advisor to Shlomo S. Sawilowsky, one of his former doctoral students was Sallie Keller-McNulty, who is President-elect of the American Statistical Association Board of Directors. Higgins received the College of Arts and Sciences Teaching Award in 1989, and was elected Fellow of the American Statistical Association in 1999.

Joseph-Louis Lagrange (Giuseppe Lodovico Lagrangia or **Luigi De la Grange Tournier)** (1736 – 1813) never met Leonhard Euler. Lagrange was mostly self-taught. However, in 1754, he began a life-long correspondence regarding his mathematical development with Euler. The following year he was appointed Professor of Mathematics at the Royal Artillery School in Turin at the age of only 19. In 1756, on Euler's recommendation, Lagrange was elected to the Berlin Academy. He was appointed Director of Mathematics at the Berlin Academy in 1766, which was Euler's post, on the latter's return to the University of St. Petersburg. Lagrange published on astronomy, dynamics, fluid mechanics, mechanics, number theory, probability, and of course, on the foundations of the calculus. The Lagrange multiplier, Lagrange integral, and Euler-Lagrange differential equation bear his name. He became a member of the Académie des Sciences in 1790. He was the inaugural Professor of Analysis at the École Polytechnique in 1794, and was required to accept a joint appointment the following year at the newly established École Normale. His teaching skills did not reach the heights of his research skills, and Jean Baptiste Joseph Fourier (1768 – 1830) was assigned as his teaching assistant. He was awarded the Legion of Honour and Count of the Empire in 1808, and the Grand Croix of the Ordre Impérial de la Réunion in 1813.

Pierre-Simon Laplace's (1749 – 1827) advanced mathematical education was directed by Jean Le Rond d'Alembert, and through his efforts, Laplace obtained a position at the École Militaire. He quickly published over a dozen articles on minima and maxima, integral calculus, and differential equations, which led to his election to the Académie des Sciences in 1773. Laplace became an examiner at the Royal Artillery Corps in 1784, and in the following year he tested the 16 year old Napoleon Bonaparte (who passed). He was later (1812) to dedicate *Théorie Analytique des Probabilités* to Napoleon. He was appointed to the Bureau des Longitudes in 1795. Perhaps he was more scientist than mathematician; along with the chemist Antoine Lavoisier (1743 – 1794), he discovered the nature of respiration, then developed his nebular hypothesis, and subsequently published extensively on the most important physics topics of the time. He did considerable work in probability theory (including the sub-discipline due to Thomas Bayes, 1702 – 1761) and the theory of errors. The Laplace transform, Laplace integral, and Laplace operator bear his name. In 1806, he was elevated to Count of the Empire, and to Marquis in 1817.

Gottfried Wilhelm von Leibniz (1646 – 1716) obtained philosophy degrees from the University of Leipzig (undergraduate) in 1663 and the University of Jena (Master's) the following year. He studied mathematics under Erhard Weigal while at Jena. He completed his studies for the Doctoral degree in Law, but was denied, apparently, because he was too young. Therefore, he left for the University of Altdorf, where he received the Doctorate in Law in 1667. He studied mathematics with Christiaan Huygens (1629 – 1695) in Paris in 1672. He was elected Fellow of the Royal Society of London the following year on the promise of developing a calculating machine (called a Stepped Reckoner, which was completed in 1694). Within four years, Leibniz was to develop his version of the calculus, and he published most of its elementary concepts, rules, and symbols in *Acta Eruditorum* by 1684. Although Sir Issac Newton (1643 – 1727) previously discovered the principles of the calculus in 1671, for a variety of reasons he never published them. Charges of plagiarism were launched in both directions. The matter was heard before Newton's home court – the Royal Society – where he had been its President since 1703. The Society commissioned a committee consisting primarily of Newton's British colleagues, such as Edmond Halley (1656 – 1742), with the notable exception of the French Abraham de Moivre (1667 – 1754). Leibniz' seemingly sole support was from his disciple Johann Bernoulli, who was not on the committee. The Society's conclusion was political, not scientific, and does not bear repeating. Newton and Leibniz can be considered co-discovers of the calculus.

Paul Pierre Lévy (1886 – 1971) was a third generation mathematician. He matriculated at École des Mines in Paris, while simultaneously attending lectures from Jean Gaston Darboux and Charles Émile Picard at the Sorbonne. His doctoral advisor was Jacques Salomon Hadamard, who also served as examiner with Picard and Henri Jules Poincaré in 1912. The Mathematics Genealogy Project also lists Vito Volterra as his doctoral advisor, and indeed, functional analysis was Lévy's first research interest. (This concurs with Hadamard's work on Volterra's "line function calculus", which Hadamard renamed as Volterra's "functional calculus".) He served on the faculty of Écoles des Mines for a year, and then for 39 years at École Polytechnique. His former doctoral student, Michel Loève, stated Lévy had few students because he did not teach probability theory at Polytechnique. However, Lévy certainly had a generation of students who benefited from his 10 books and 278 articles, primarily written on probability. He also published on functional analysis, partial differential equations, Brownian motion, and geometry. Lévy was elected honorary member of the London Mathematical Society in 1963, and in 1964 to the Académy des Sciences.

E. F. Lindquist (1914 – 1998), a native of Gowrie, Iowa (population about 1,000), was a psychometrician and statistician. He was a research assistant at the University of Iowa's College of Education in 1925. He became concerned with the process of assigning student grades based on casual and informal observations, or on subjective and unreliable opinions. This led him to the position of Director of the Iowa Testing Programs from 1930 – 1969. He co-invented the first electronic test scoring machine in 1955. He was also the co-founder of the American College Testing program (ACT) in 1959. He was the original developer of the Iowa Test of Basic Skills, and its first Director. In 1973, the University of Iowa dedicated the E. F. Lindquist Center for Measurement. The American Educational Research Association (AERA) and ACT co-sponsor an annual award in his name for outstanding theoretical research in testing and measurement. He was awarded the 1967 Distinguished Contributions to Research in Education Award by AERA.

Joseph Liouville (1809 – 1882) obtained his doctorate in 1827 from École Polytechnique. His examiners were Siméon Denis Poisson and Gaspard Clair François Marie Riche de Prony. He took several courses from André Marie Ampère and Dominique François Jean Arago at Polytechnique. He taught at Collège de France and École Centrale. Liouville launched the *Journal de Mathématiques Pures et Appliquées* in 1836. It became known as the Journal de Liouville, and it was an alternative to the previously established *Crelle's Journal* (August Leopold Crelle, 1780 – 1855). Liouville was elected to the Académie des Sciences in 1839, and the Bureau des Longitudes in 1840. In 1846, he published Evariste Galois's (1811 – 1832) hastily written final expositions prior to his death by duel. In politics, Liouville was elected to the Constituting Assembly in 1848. His work on the boundary value problem in differential equations resulted in the Sturm-Liouville theory (Charles-François Sturm, 1803 – 1855), an approach used in solving integral equations. He published about 200 articles on fractional calculus, integration of algebraic functions, transcendental numbers, and quadratic reciprocity. His work in differential geometry provides some of the foundations of statistical mechanics and measure theory.

Joseph Johann von Littrow (1781 – 1840) was a Professor of Astronomy at the University of Crakow, and served as the director of the Crakow Observatory from 1808 – 1810. Due to the campaign of Napolean, Littrow hastily repaired to a Professorship in Astronomy at the University of Kazan in Russia. In 1816, he became the co-Director of the Pest Observatory in Hungary. He became Professor of Astronomy at the University of Vienna in 1819, and directed the Viennese Observatory. His areas of research were in astronomy, chronometry, geometry, optics, and physics. About 1840, he proposed digging ditches 20 miles in diameter in the Sahara, fueling them with kerosene, and igniting them to communicate with extraterrestrial life. On December 11, 1972, Apollo 17 landed at the southeastern rim of Mare Serenitatis in the Taurus – Littrow valley at 20.19080° N latitude, 30.77168° E longitude, a lunar surface named after Joseph von Littrow. He was knighted by the Emperor of Austria in 1837.

Michel Loève (1907 – 1979) was born in Yaffa, Israel, and eventually immigrated to France. He was naturalized as a United States citizen in 1953. While in France, he was awarded the title Actuaire I. S. F. A. (l'Institut de Science Financière et d'Assurances) by the Université de Lyon in 1936, and obtained his Doctorate in Mathematical Sciences from the Sorbonne in 1941. He held appointments at the Centre National de la Recherche Scientifique, was the Chargé de Recherches at the Institut Henri Poincaré of the Université de Paris, and briefly served on the faculty at the University of London. After completing a visiting Professorship at Columbia University, he became Professor of Mathematics at the University of California, Berkeley. He obtained appointments as Professor of Statistics in 1955 and Professor of Arts and Sciences in 1967. His lectures on probability theory were published in textbook form as a volume in The University Series in Higher Mathematics in 1954. It became one of the most popularly used textbooks on modern probability theory. In addition to Julius Rubin Blum, Emanuel Parzen was one of Loève's doctoral students, who also wrote a classic textbook on the same subject. Loève was named Professor Emeritus in 1974. His wife and the University of California established the $30,000 Line and Michel Loève International Prize in Probability.

Andrei Andreyevich Markov (1856 – 1922) graduated from St. Petersburg University, Russia, in 1878, and became a Professor in 1886. He published on analysis, approximation theory, number theory, limits, and converging series. He is noted for his work on stochastic processes and probability theory. His name is associated with Markov chains, a sequence of random variates wherein a predicted value is independent, but based on the current value.

Theodore Micceri obtained the Ph. D. in Measurement and Research from the University of South Florida (USF) in 1987. He was R. Clifford Blair's second and final doctoral student. Bruce W. Hall was co-advisor of his dissertation. He is a researcher in the USF Office of Institutional Effectiveness. He has 20 refereed publications on real data distributions, robustness of statistics, and instrument validation. Micceri has published over 375 technical reports on the evaluation of teacher practices, courseware design, and data base design. He is a Church Deacon and a Wood Badge trained Boy Scout leader.

Gaspard Monge, Comte de Péluse (1746 – 1818) graduated from the Collège de la Trinité in 1764. The following year he became a draftsman at École Royale du Génie, Mézières, where he came into contact with Charles Bossut. When Bossut took another post in 1769, Monge replaced him as Professor of Mathematics, and the following year he held a simultaneous position as Instructor in Physics at the École Royale du Génie. While at École Polytechnique, one of his teaching assistants was Jean Baptiste Joseph Fourier (1768 – 1830). Monge published frequently at the Académie des Sciences on calculus of variations, infinitesimal geometry, partial differential equations, and combinatorics. He played an important role in creating École Polytechnique, and eventually became its Director. His support of Napoleon Bonaparte, even after his defeat at Waterloo, made Monge persona non grata in his latter years.

Ottaviano Fabrizio Mossotti (1791 – 1863), a student and later research assistant of Vincenzo Brunacci, obtained his degree in Engineering and Architecture at the University of Pavia in 1811. There is some evidence he took courses, and was influenced by Louis Gaspard Brugnatelli (1761 – 1818) and Alessandro Volta (1745 – 1827). He interned under Francesco Carlini (1783 – 1862) at the Royal Astronomical Observatory of Brera in Milan. An offer as Chair in Algebra and Geometry at Pavia was withdrawn when the university decided not to hire foreigners. In 1822, he was elected to the Società Italiana delle Scienze residente in Modena. He went to England for political reasons, returning later to become a Professor of Celestial Physics at the University of Pisa. In 1848, he fought in the Battle of Tuscany at Curtatone and Montanara, successfully leading a battalion of university students. In 1863, he was elected Senator of the Kingdom of Italy.

Jerzy (Splawa-)Neyman (Yuri Czeslawovich) (1894 – 1981), suffering from poor eye sight and tuberculosis, obtained his undergraduate degree from Kharkov University in 1947 and remained there as a Lecturer of Mathematics. He was influenced by his coursework in statistics, taken under Sergei Natanovich Bernstein (1880 – 1968). He met Wacław Sierpiñski in Poland, and was motivated to study under him for his doctorate, which he received in 1924. He was examined by Sierpiñski and Stefan Mazurkiewicz (1888 – 1945). Neyman became a teacher at Warsaw University and the College of Agriculture. As is well known, Neyman won a Rockefeller Fellowship to work with (Carl) Karl Pearson (1857 – 1936) in London in 1925, but was disappointed with Pearson's training in mathematics. He took a second year's fellowship to study with Félix Edouard Justin Emile Borel

(1871 – 1956) and Henri Léon Lebesgue (1875 – 1941) in Paris. Neyman returned to Poland in 1928, and set up and became the Director of the Biometric Laboratory at the Nencki Institute for Experimental Biology in Warsaw. He then joined Egon Sharpe Pearson (1895 – 1980), Karl's son, as an Associate Professor at University College in London, who he had met in 1925. Neyman accepted a position as Professor of Mathematics at the University of California, Berkeley. In 1955, he founded and became the Director of the Department of Statistics. Neyman and Egon Pearson collaborated on a number of articles, and they modified Sir Ronald Aylmer Fisher's (1890 – 1962) fiducial theory of statistics into the frequentist approach known as the Neyman – Pearson or "Bernoullian" paradigm of statistics. Neyman published on experimental design, generalized chi-square, hypothesis testing, optimal asymptotic tests, probability, and survey sampling. One of Neyman's greatest achievements was the development of the confidence interval, making him the father of modern statistics. He published applied research in meteorology and carcinogenesis toward the end of his career. Among his students were Erich Leo Lehmann (whose students included Madan Lal Puri, Peter John Bickel, Kjell Andreas Doksum, Gouri Kanta Bhattacharyya, Frank Rudolf Hampel, Howard Joseph Michael D'Abrera), George Bernard Dantzig, Frank Jones Massey, Jr., and Joseph Lawson Hodges, Jr. (whose student was Jerome Hamilton Klotz). Neyman won the Royal Statistical Society Guy Medal in 1966, the United States Medal of Science in 1969, and the 1973 Medal of the Copernicus Society of America. He was elected Fellow of the Royal Society in 1979. Neyman's slogan was "Statistics is the servant to all sciences".

Pietro Paoli (1759 – 1839) taught mathematics at the University of Pavia. His two volume *Elements of Algebra* was a classic text used in Italy. His research was on analytic geometry, calculus, partial derivatives, and differential equations. In addition to Vincenzo Brunacci, his students included Giovanni Taddeo Farini (1778 – 1822).

Charles Émile Picard (1856 – 1941) obtained his Ph. D. in 1877 from École Normale Supérieure. He served on their faculty, and later at the University of Paris, Toulouse, and the Sorbonne. His areas of expertise were in analysis, function theory, differential equations, and analytic geometry. He discovered the Picard group transformations on a linear differential equation. He published numerous books, and served as an Editor of Liouville's journal from 1885 – 1941. He was elected to the Acaédemy des Sciences in 1889 and the Académe Française in 1924. He received the Poncelet Prize in 1886, Grand Prix des Sciences Mathématiques in 1888, Grande Croix de la Légion d'Honneur in 1932, and the Mittag-Leffler Gold Medal in 1938. He served as President of the International Congress of Mathematicians in 1920.

Henri Jules Poincaré (1854 – 1912) was a student of Charles Hermite. He was an influential instructor of Paul Pierre Lévy, and served on his examination committee. Although Poincaré suffered greatly from various childhood illnesses, leaving him with muscular dysfunctions and poor eye sight, he was able to graduate from the École Polytechnique in 1875. He received his Doctorate in Mathematics from the University of Paris in 1879. His dissertation defense was less than stellar: His "*thesis is a little confused and shows that the author was still unable to express his ideas in a clear and simple manner*." He accepted a professorship at the University of Caen, where it was revealed that his teaching skills were underdeveloped. Despite his disabilities, lackluster thesis, and sub par teaching skills, he is considered to be one of the greatest geniuses in history. The road to success began with an appointment in 1881 to the Faculty of Science, then as Chair of Mathematical Physics at the Sorbonne in 1886, and eventually to the École Polytechnique. He became the father of algebraic topology, analytic functions of several complex variables, and along with Magnus Gösta Mittag-Leffler (1846 – 1927), his work led to chaos theory. As impressive as were these accomplishments, they pale in comparison to his co-discovery of special relativity, along with Hendrik Antoon Lorentz (1853 – 1928) and Albert Einstein (1879 – 1955). He was elected to the Académie des

Sciences in 1887, and became its President in 1906. He is the only person elected to every division of the Académie (geography, geometry, mechanics, navigation, & physics). He was elected to the Académie Francaise in 1908.

Siméon Denis Poisson's (1781 – 1840) lack of fine motor coordination played a role in his decision not to pursue a career in medicine, and when he turned to mathematics, to avoid descriptive geometry that required drawing finely detailed charts. Nevertheless, under the tutelage of both Pierre-Simon Laplace and Joseph-Louis Lagrange, his work was considered so brilliant that his dissertation was accepted without the traditional examination. His was immediately offered his first position at École Polytechnique in 1800. His appointments blossomed as an astronomer at the Bureau des Longitudes in 1808 and inaugural Chair of Mechanics at the Faculté des Sciences in 1809. He published major treatises on astronomy, heat, electricity, physics, and nearly 400 tracts on mathematics. His name is associated with the Poisson integral, Poisson distributions, Poisson differential equation brackets, Poisson elasticity ratio, and the Poisson constant in electricity. In addition to Michel Chasles, another of his doctoral students was Johann Peter Gustav Lejeune Dirichlet (1805 – 1859) (with Jean-Baptiste Joseph Fourier, 1768 – 1830, serving as 2nd advisor). [Dirichlet had many notable academic descendents: Rudolf Otto Sigismund Lipschitz (1832 – 1903), followed by Felix C. Klein (1849 – 1925), and Wilhelm v. Behrens and Ludwig Bieberbach (1886 – 1982). Continuing through Bieberbach were Heinz Hopf (1894 – 1971), Beno Eckmann, and Peter Jost Huber.] Dominique François Jean Arago quoted Poisson to have said, "Life is good for two things: researching mathematics and teaching mathematics."

Gaspard Clair François Marie Riche de Prony (1755 – 1839) graduated in 1776 with a degree in engineering from the École des Ponts et Chaussés, where he was subsequently employed and eventually became its Director in 1798. His work on the Louis XVI Bridge (Pont de la Concorde) elevated him to the position of Engineer-in-Chief in 1790. The following year, working with Adrien-Marie Legendre (1752 – 1833), Lazare Nicolas Marguérite Carnot (1753 – 1823), and over six dozen assistants, he commenced producing the *Cadastre*, an exhaustive book of logarithms and trigonometric functions. He wrote several text books on mechanics. He was a member of the Bureau de Longitude. de Prony promoted reforming curriculum toward applied mathematics, but Augustin Louis Cauchy's (1789 – 1857) firm stance on pure mathematics prevailed.

Hermann Henry Remmers (1892 – 19??) obtained his Ph. D. from the University of Iowa. He was a Professor of Education and Psychology at Purdue University for about 30 years, and served as the Director of the Division of Educational References. In 1935, he co-founded what was to become the Indiana Student Financial Aid Association. He was the originator of the Purdue Opinion Panel, which led to his noted book, *The American Teenager*, in 1957. He authored textbooks on educational psychology, educational measurement and evaluation, and about 200 articles and monographs on teaching, survey methods, testing, and evaluation. He was elected President of the Division of Educational Psychology of the American Psychological Association in 1951, and the President of the American Educational Research Association. His name is associated with Purdue University's $1,000 H. H. Remmers Award for African American Studies.

Louis Paul Émile Richard (1795 – 1849) served on the faculty of the College de Pontivy, Collège Saint-Louis, and Collège Louis-le-Grand. In addition to Charles Hermite, his students included Urbain Jean Joseph Le Verrier (1811 – 1877), Joseph Alfred Serret (1819 – 1885), and Evariste Galois (1811 – 1832).

Shlomo Noach (Stephen Ram) Sawilowsky obtained the M. A. (Counselor Education, 1981) and Ph. D. (Measurement, Evaluation, and Research, 1985) from the University of South Florida (USF). He was R. Clifford Blair's first doctoral student, graduate teaching assistant for two years, and graduate research assistant for two years. James J.

Higgins was his dissertation 2nd advisor. Bruce W. Hall was his measurement instructor. Sawilowsky was a Visiting Assistant Professor at USF from 1985 – 1987, and accepted a position in the College of Education at Wayne State University (WSU) in 1987. He rose through the ranks to full Professor of Evaluation and Research in 1997, and has served as Department Chair since 1998. He accepted simultaneous teaching appointments with the faculty of Curriculum and Instruction in 1998 and Counselor Education in 2000. Sawilowsky and Blair's work on the rank transform won the 1986 Distinguished Researcher Award of the Florida Educational Research Association, and a 1987 Distinguished Paper Award, State and Regional Associations, of the American Educational Research Association (AERA). Sawilowsky has won many WSU teaching honors, including the 1995 University President's Award, 1997 College of Education Award, 1998 Graduate Mentor Award, and the 1999 Faculty Mentor Award. A list of his doctoral students, descendents in R. Clifford Blair's academic genealogy, is compiled in Table 2. He was awarded WSU Distinguished Faculty Fellow in 2000. Along with Sharon Field and Alan Hoffman, he obtained over $3.5 Million in extramural funding for research on self-determination for students with and without disabilities, and co-authored a battery of standardized tests on self-determination. He has published over 80 articles on nonparametric rank tests, permutation and robust methods, classical measurement theory, and construct validity. He co-authored a textbook on statistics via Monte Carlo methods with Gail Fahoome, a former doctoral student, and he is the Editor of a volume on real data analysis to be published by the AERA Educational Statisticians. He founded the *Journal of Modern Applied Statistical Methods* in 2000, and serves as Editor. In a simultaneous career, he obtained his undergraduate degree in 1979 at the Rabbinical College of America. He served as the emissary of the *Lubavitcher Rebbe AMv"R* Menachem Mendel Schneerson, *ZTzVKLLH"H*, in Tampa (1980 – 1985) and St. Petersburg, Florida (1985 – 1987). Since 1987, he has taught *Talmud* (Rabbinical jurisprudence), *Halacha* (Jewish law), and *Chassidut* (philosophy) at various synagogues in Michigan. In 2004, Sawilowsky obtained a second Rabbinical ordination in Jerusalem, Israel, from *HaRav* Dovid Ostroff of *Pirchei Shoshanim*.

Wacław F. Sierpiñski (1882 – 1969) obtained his undergraduate degree in 1904 from the Department of Mathematics and Physics at the University of Warsaw while it was under Russian occupation. He was a student of Georgy Voronoy at that time. He won a prestigious university prize for his work on number theory. In his memoirs, Sierpiñski revealed that he deliberately left the answers to his final examinations blank to protest the Russian occupation of Poland and the University. This put the University in the position of denying the degree to a prize-winning student. Ultimately, however, he received the degree. He became a student of Zaremba (who was Voronoy's student) at the Jagiellonian University, Crakow. He obtained the doctorate in 1908, and in the same year accepted an appointment at the University of Lvov in 1908. Later, he served as the Dean of the Faculty of the University of Warsaw. One of his students was Stefan Mazurkiewicz (1888 – 1945). He published many books and articles, primarily on set theory, theory of irrational numbers, and point set topology. The Sierpiñski curve bears his name. He founded the journal *Fundamenta Mathematicae*. He was elected to the Polish Academy, Vice Chair of the Warsaw Scientific Society, and the Polish Mathematical Society.

Howard Stoker obtained his Master's degree in 1950 at the University of Iowa. Albert N. Hieronymus was his thesis advisor, and Robert L. Ebel served on the Master's committee. Stoker received his Ph. D., as a student of Hermann Henry Remmers, in 1957, from Purdue University. He obtained his first academic appointment at Florida State University, where he taught from 1957 – 1984. He was awarded Professor Emeritus in 1985. From 1984 – 1988 he was the Head of Instructional Development and Evaluation in the Department of Education at the University of Tennessee, Memphis. From 1988 – 1992 he held his third professorship, this time at the University of Tennessee, Knoxville. He was awarded Professor Emeritus from the University of Tennessee in 1992. He co-authored a two volume edited text on educational measurement in 1996. His research focused on standardized testing, test validity, and measurement theory.

Jules Tannery (1848 – 1910) obtained his Ph. D. in 1874 at École Normale Supériur. He served as a member of the mathematics faculty at Lycée Saint-Louis, Sorbonne, École Normale Supériur, École Normale – Sèvres, and Faculty of Sciences – Paris. He authored books on the history and philosophy of mathematics, and was an Editor of the *Bulletin des Sciences Mathématics* from 1876 – 1910. He played an important role in the revising of mathematics curriculum in France. He was elected *member libre* of the Académie des Sciences in 1907.

Vito Volterra obtained his Doctorate in Physics at the University of Pisa under Enrico Betti in 1882. His initial appointment, the following year, was Professor of Mechanics. He assumed the Chair of Mathematical Physics after Betti's demise. Subsequently, he served on the faculty at the University of Turin and the University of Rome. He published on partial differential equations, celestial mechanics, elasticity, and biometrics. His name is associated with Volterra functional calculus or Volterra type integrals. He became a Senator of the Kingdom of Italy in 1905. He was decorated with the War Cross for his services as a veteran of the air forces group in the corps of engineers in World War I, and was credited with developing mounted guns in airplanes. He was the first to propose replacing hydrogen with helium in airships. He received honorary knighthood from King George V of England in 1921. Volterra fought against the Fascist take-over of the Italian Parliament in 1930, resulting in his dismissal the following year from the University of Rome. He was President of the Academia dei Lincei, and after his dismissals from Italian scientific societies by the Fascist government, he was elected to the Pontifical Academy of Sciences by Pope Pius XI in 1936.

Georgy Fedoseevich Voronoy (1868 – 1908) was a member of the Faculty of Physics and Mathematics at the University of St. Petersburg, and Warsaw University, even while working on his undergraduate and Master's degrees. He obtained his Doctorate in Mathematics at the University of St. Petersburg. His dissertation won the Bunyakovsky Prize (Viktor Yakovlevich Bunyakovsky, 1804 – 1889). His area of research was number theory: algebraic numbers and the geometry of numbers. There is a discipline of art referred to as "Voronoi Paintings", where the design is based on cells interacting directly with its neighbor and indirectly elsewhere. Samples may be viewed at the Trayecto Gallery, Vitoria, and currently at: http://www.lxxl.pt/veado.html.

Erhard Weigel (1625 – 1699) was Professor of Mathematics at Jena University, where he taught from 1653 - 1699. He was an inventor, educator, and advocate of the Gregorian calendar. His aim was to meld mathematics with philosophy. He sought to teach the sciences to the public, and in that effort, created a celestial instruction globe made of copper, brass and wood that is held at the National Maritime Museum in London. Gottfried Wilhelm von Leibniz was his student. Gottfried Kirch (1639 – 1710) and Samuel Pufendorf (1632 – 1694) are indicated as his students, but their ages suggest they may have studied under Weigel prior to his tenure at Jena. The Mathematics Genealogy Project lists Christoph Vogel (Doctorate of Philosophy in 1652) and Theophilus Wildius (Ratisbonensis, Doctorate of Philosophy in 1654) as Weigel's students, but the dates of their doctorates are similarly problematic.

Stanislaw Zaremba (1863 – 1942) attended the Sorbonne, where he obtained his doctoral degree in 1889. After teaching in France for a decade, he returned to his native Poland to accept a Chair at Jagiellonian University, Crakow. He was elected as the inaugural President of the Mathematical Society of Crakow, and was the Editor of the *Annals of the Polish Mathematical Society* for many years. His primary areas of research were in partial differential equations and potential theory, but he also published articles on mathematical physics and crystallography. He was elected to the Soviet Academy in 1925.

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