Past, Present, and Future of Statistical Science

Edited by
Xihong Lin, Christian Genest,
David L. Banks, Geert Molenberghs,
David W. Scott, and Jane-Ling Wang

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A CHAPMAN & HALL BOOK

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Committee of Presidents
of Statistical Societies

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Cover images **Top row:** Andrew Gelman, Alice S. Whittemore, Tze Leung Lai, Marie Davidian, Peter Hall, Michael A. Newton, Jianqing Fan. **Second row:** Mary Gray, Bruce G. Lindsay, C. F. Jeff Wu, R. Dennis Cook, Nancy Reid, Noel A. Cressie, Donald A. S. Fraser. **Third row:** Ingram Olkin, Lynne Billard, Donald B. Rubin, Bradley Efron, Iain M. Johnstone, Juliet Popper Shaffer, Herman Chernoff. **Fourth row:** James O. Berger, Xiao-Li Meng, Elizabeth A. Thompson, Peter J. Bickel, Louise Ryan, Raymond J. Carroll, Xihong Lin. **Fifth row:** David B. Dunson, Kathryn Roeder, Ross L. Prentice, Jeffrey S. Rosenthal, Robert J. Tibshirani, Nancy Flournoy, Arthur P. Dempster. **Sixth row:** Terence P. Speed, Roderick J. Little, Norman E. Breslow, Grace Wahba, Mark J. van der Laan, Stephen E. Fienberg, Mary E. Thompson. **Seventh row:** Donna Brogan, Pascal Massart, Nilanjan Chatterjee, Larry A. Wasserman, Theodore W. Anderson, Nan M. Laird, Rafael A. Irizarry.

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Preface

Statistics is the science of data collection, analysis, and interpretation. It plays a pivotal role in many disciplines, including environmental, health, economic, social, physical, and information sciences. Statistics not only helps advance scientific discovery in many fields but also influences the development of humanity and society. In an increasingly data-driven and information-rich world, statistics is ever more critical in formulating scientific problems in quantitative terms, accounting for and communicating uncertainty, analyzing and learning from data, transforming numbers into knowledge and policy, and navigating the challenges for making data-driven decisions. The emergence of data science is also presenting statisticians with extraordinary opportunities for increasing the impact of the field in the real world.

This volume was commissioned in 2013 by the *Committee of Presidents of Statistical Societies* (COPSS) to celebrate its 50th anniversary and the International Year of Statistics. COPSS consists of five charter member societies: the American Statistical Association (ASA), the Institute of Mathematical Statistics (IMS), the Statistical Society of Canada (SSC), and the Eastern and Western North American Regions of the International Biometric Society (ENAR and WNAR). COPSS is best known for sponsoring prestigious awards given each year at the Joint Statistical Meetings, the largest annual gathering of statisticians in North America. Through the contributions of a distinguished group of statisticians, this volume aims to showcase the breadth and vibrancy of statistics, to describe current challenges and new opportunities, to highlight the exciting future of statistical science, and to provide guidance for future generations of statisticians.

The 50 contributors to this volume are all past winners of at least one of the awards sponsored by COPSS: the R.A. Fisher Lectureship, the Presidents' Award, the George W. Snedecor Award, the Elizabeth L. Scott Award, and the F.N. David Award. Established in 1964, the Fisher Lectureship honors both the contributions of Sir Ronald A. Fisher and a present-day statistician for their advancement of statistical theory and applications. The COPSS Presidents' Award, like the Fields Medal in mathematics or the John Bates Clark Medal in economics, is an early career award. It was created in 1979 to honor a statistician for outstanding contributions to statistics. The G.W. Snedecor Award, founded in 1976 and bestowed biennially, recognizes instrumental theoretical work in biometry. The E.L. Scott Award and F.N. David Award are also given biennially to commend efforts in promoting the role of women in

statistics and to female statisticians who are leading exemplary careers; these awards were set up in 1992 and 2001, respectively.

This volume is not only about statistics and science, but also about people and their passion for discovery. It contains expository articles by distinguished authors on a broad spectrum of topics of interest in statistical education, research, and applications. Many of these articles are accessible not only to professional statisticians and graduate students, but also to undergraduates interested in pursuing statistics as a career, and to all those who use statistics in solving real-world problems. Topics include reminiscences and personal reflections on statistical careers, perspectives on the field and profession, thoughts on the discipline and the future of statistical science, as well as advice for young statisticians. A consistent theme of all the articles is the passion for statistics enthusiastically shared by the authors. Their success stories inspire, give a sense of statistics as a discipline, and provide a taste of the exhilaration of discovery, success, and professional accomplishment.

This volume has five parts. In Part I, Ingram Olkin gives a brief overview of the 50-year history of COPSS. Part II consists of 11 articles by authors who reflect on their own careers (Ingram Olkin, Herman Chernoff, Peter Bickel), share the wisdom they gained (Dennis Cook, Kathryn Roeder) and the lessons they learned (David Brillinger), describe their journeys into statistics and biostatistics (Juliet Popper Schaffer, Donna Brogan), and trace their path to success (Bruce Lindsay, Jeff Rosenthal). Mary Gray also gives an account of her lifetime efforts to promote equity.

Part III comprises nine articles devoted to the impact of statistical science on society (Steve Fienberg), statistical education (Iain Johnstone), the role of statisticians in the interplay between statistics and science (Rafael Irizarry and Nilanjan Chatterjee), equity and diversity in statistics (Mary Thompson, Nancy Reid, and Louise Ryan), and the challenges of statistical science as we enter the era of big data (Peter Hall and Xihong Lin).

Part IV consists of 24 articles, in which authors provide insight on past developments, current challenges, and future opportunities in statistical science. A broad spectrum of issues is addressed, including the foundations and principles of statistical inference (Don Fraser, Jim Berger, Art Dempster), nonparametric statistics (David Dunson), model fitting (Andrew Gelman), time series analysis (Ted Anderson), non-asymptotic probability and statistics (Pascal Massart), symbolic data analysis (Lynne Billard), statistics in medicine and public health (Norman Breslow, Nancy Flournoy, Ross Prentice, Nan Laird, Alice Whittemore), environmental statistics (Noel Cressie), health care and finance (Tze Leung Lai), statistical genetics and genomics (Elizabeth Thompson, Michael Newton), survey sampling (Rod Little), targeted learning (Mark van der Laan), statistical techniques for big data analysis, machine learning, and statistical learning (Jianqing Fan, Rob Tibshirani, Grace Wahba, Larry Wasserman). This part concludes with "a trio of inference problems that could win you a Nobel Prize in statistics" offered by Xiao-Li Meng.

Part V comprises seven articles, in which six senior statisticians share their experience and provide career advice. Jeff Wu talks about inspiration, aspiration, and ambition as sources of motivation; Ray Carroll and Marie Davidian give tips for success in research and publishing related to the choice of research topics and collaborators, familiarity with the publication process, and effective communication; and Terry Speed speaks of the necessity to follow one's own path and to be enthusiastic. Don Rubin proposed two possible topics: learning from failure and learning from mentors. As they seemed equally attractive, we asked him to do both. The book closes with Brad Efron's "thirteen rules for giving a really bad talk."

We are grateful to COPSS and its five charter member societies for supporting this book project. Our gratitude extends to Bhramar Mukherjee, former secretary and treasurer of COPSS; and to Jane Pendergast and Maura Stokes, current chair and secretary/treasurer of COPSS for their efforts in support of this book. For their help in planning, we are also indebted to the members of COPSS' 50th anniversary celebration planning committee, Joel Greenhouse, John Kittelson, Christian Léger, Xihong Lin, Bob Rodriguez, and Jeff Wu.

Additional funding for this book was provided by the International Chinese Statistical Society, the International Indian Statistical Association, and the Korean International Statistical Society. We thank them for their sponsorship and further acknowledge the substantial in-kind support provided by the Institut des sciences mathématiques du Québec.

Last but not least, we would like to express our deep appreciation to Heidi Sestrich from Carnegie Mellon University for her technical assistance, dedication, and effort in compiling this volume. Thanks also to Taylor and Francis, and especially senior editor Rob Calver, for their help and support. With the publisher's authorization, this book's content is freely available at www.copss.org so that it can benefit as many people as possible.

We hope that this volume will inspire you and help you develop the same passion for statistics that we share with the authors. Happy reading!

The editors

Xihong Lin
Harvard University
Boston, MA

David L. Banks
Duke University
Durham, NC

David W. Scott
Rice University
Houston, TX

Christian Genest
McGill University
Montréal, QC

Geert Molenberghs
Universiteit Hasselt and KU Leuven
Belgium

Jane-Ling Wang
University of California
Davis, CA

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Contributors

Theodore W. Anderson
Stanford University
Stanford, CA

James O. Berger
Duke University
Durham, NC

Peter J. Bickel
University of California
Berkeley, CA

Lynne Billard
University of Georgia
Athens, GA

Norman E. Breslow
University of Washington
Seattle, WA

David R. Brillinger
University of California
Berkeley, CA

Donna J. Brogan
Emory University
Atlanta, GA

Raymond J. Carroll
Texas A&M University
College Station, TX

Nilanjan Chatterjee
National Cancer Institute
Bethesda, MD

Herman Chernoff
Harvard University
Cambridge, MA

R. Dennis Cook
University of Minnesota
Minneapolis, MN

Noel Cressie
University of Wollongong
Wollongong, NSW, Australia

Marie Davidian
North Carolina State University
Raleigh, NC

Arthur P. Dempster
Harvard University
Cambridge, MA

David B. Dunson
Duke University
Durham, NC

Bradley Efron
Stanford University
Stanford, CA

Jianquing Fan
Princeton University
Princeton, NJ

Stephen E. Fienberg
Carnegie Mellon University
Pittsburgh, PA

Nancy Flournoy
University of Missouri
Columbia, MO

Donald A.S. Fraser
University of Toronto
Toronto, ON

Andrew Gelman
Columbia University
New York, NY

Mary W. Gray
American University
Washington, DC

Peter G. Hall
University of Melbourne, Australia
University of California, Davis, CA

Rafael A. Irizarry
Dana-Farber Cancer Institute and
Harvard School of Public Health
Boston, MA

Iain M. Johnstone
Stanford University
Stanford, CA

Tze Leung Lai
Stanford University
Stanford, CA

Nan M. Laird
Harvard School of Public Health
Boston, MA

Xihong Lin
Harvard School of Public Health
Boston, MA

Bruce G. Lindsay
Pennsylvania State University
University Park, PA

Roderick J. Little
University of Michigan
Ann Arbor, MI

Pascal Massart
Université de Paris-Sud
Orsay, France

Xiao-Li Meng
Harvard School of Public Health
Boston, MA

Michael A. Newton
University of Wisconsin
Madison, WI

Ingram Olkin
Stanford University
Stanford, CA

Ross Prentice
Fred Hutchinson
Cancer Research Center
Seattle, WA

Nancy M. Reid
University of Toronto
Toronto, ON

Kathryn Roeder
Carnegie Mellon University
Pittsburgh, PA

Jeffrey S. Rosenthal
University of Toronto
Toronto, ON

Donald B. Rubin
Harvard University
Cambridge, MA

Louise M. Ryan
University of Technology Sydney
Sydney, Australia

Juliet Popper Shaffer
University of California
Berkeley, CA

Terry Speed
University of California
Berkeley, CA
Walter and Eliza Hall Institute of
Medical Research
Melbourne, Australia

Elizabeth A. Thompson
University of Washington
Seattle, WA

Mary E. Thompson
University of Waterloo
Waterloo, ON

Robert J. Tibshirani
Stanford University
Stanford, CA

Mark van der Laan
University of California
Berkeley, CA

Grace Wahba
University of Wisconsin
Madison, WI

Larry A. Wasserman
Carnegie Mellon University
Pittsburgh, PA

Alice S. Whittemore
Stanford University
Stanford, CA

C.F. Jeff Wu
Georgia Institute of Technology
Atlanta, GA

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Part I

The history of COPSS

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A brief history of the Committee of Presidents of Statistical Societies (COPSS)

Ingram Olkin

Department of Statistics
Stanford University, Stanford, CA

Shortly after becoming Chair of COPSS in 1992, I collated some of the organization's archival history. At that time there was already a 1972 document prepared by Walter T. Federer who was Chair starting in 1965. The following is a composite of Federer's history coupled with my update in 1994, together with a review of recent activities.

1.1 Introduction

In 1958–59, the American Statistical Association (ASA), the Biometric Society (ENAR), and the Institute of Mathematical Statistics (IMS) initiated discussions to study relationships among statistical societies. Each of the three organizations often appointed a committee to perform similar or even identical duties, and communication among these and other groups was not always what was desired. Thus, in order to eliminate duplication of work, to improve communication among statistical societies, and to strengthen the scientific voice of statistics, the ASA, under the leadership of Rensis Likert, Morris H. Hansen, and Donald C. Riley, appointed a Committee to Study Relationships Among Statistical Societies (CONTRASTS). This committee was chaired by Frederick Mosteller. A series of campus discussions was initiated by members of CONTRASTS in order to obtain a broad base of opinion for a possible organization of statistical societies.

A grant of $9,000 was obtained from the Rockefeller Foundation by ASA to finance a series of discussions on the organizational needs of North American statisticians. Subsequently, an inter-society meeting to discuss relationships among statistical societies was held from Friday evening of September 16 through Sunday morning of September 18, 1960, at the Sterling Forest

Onchiota Conference Center in Tuxedo, New York. An attempt was made at inclusiveness, and 23 cognate societies sent representatives in addition to Wallace O. Fenn of the American Institute of Biological Sciences, G. Baley Price of the Conference Board of Mathematical Sciences, and May Robinson of the Brookings Institute. See *The American Statistician*, 1960, vol. 14, no. 4, pp. 2–3, for a complete listing of the representatives.

Mr. Fenn described the origin of the American Institute of Biological Sciences (AIBS) and pointed out their accomplishments after organization. Mr. Price discussed the background and importance of the Conference Board of Mathematical Sciences. The main motion for future action that was passed was to the effect that a federation of societies concerned with statistics should be organized to consider some or all of the following items: (1) publicity; (2) publication; (3) bulletin; (4) newsletter; (5) subscription exchange; (6) abstracts; (7) translations; (8) directors; (9) national roster; (10) recruitment; (11) symposia; (12) visiting lecturer program and summer institutes; (13) joint studies; (14) films and TV; (15) Washington office; (16) nominations for national committees; (17) fellowships at national levels; (18) cooperative apprentice training program; (19) international cooperation. It is satisfying to note that many of these activities came to fruition. A committee was appointed to draft a proposal for a federation of statistical societies by January 1, 1961.

1.1.1 The birth of COPSS

It was not until December 9, 1961, that a meeting was held in New York to discuss the proposed federation and the roles of ASA, ENAR–WNAR, and IMS in such an organization. A more formal meeting of the presidents, secretaries, and other members of the ASA, IMS, and the Biometric Society (ENAR) was held at the Annual Statistics Meetings in December of 1961. At this meeting the Committee of Presidents of Statistical Societies (COPSS) was essentially born. It was agreed that the president, the secretary, and one society-designated officer of ASA and IMS, the president and secretary of ENAR, the president of WNAR, and one member-at-large would form the COPSS Committee (*Amstat News*, October 1962, p. 1). The Executive Committee of COPSS was to be composed of the presidents of ASA, ENAR, and IMS and the secretary of ASA, with each president to serve as Chairman of the Committee for four months of the year.

Philip Hauser, then President of the ASA, reported on the deliberation of COPSS at the Annual Meeting. Six joint committees were established with representatives from ASA, IMS, ENAR, and WNAR. The charges are described in his report as follows.

1. *The Joint Committee on Professional Standards* is charged with considering the problems relating to standards for professional statisticians and with recommending means to maintain such standards.

2. *The Joint Committee on the Career Brochure* will be charged generally with considerations relating to statistics as a career. A specific continuing task will be the preparation and revision of a Career Brochure.

3. *The Joint Committee on Educational Opportunities* will be charged with the preparation of a brochure designed to inform students and others interested in statistics as a career of the appropriate available training facilities, both graduate and undergraduate.

4. *The Joint Committee for Liaison with the American Association for the Advancement of Science* (AAAS) will represent the statistics profession in a new section within the AAAS.

5. *The Joint Committee on Organizational Changes* is charged with the study of constitutional changes among the societies and with recommending those which may facilitate more effective and efficient collaboration between them and between members of the profession generally.

6. *The Joint Committee on News and Notes* has the task of working out arrangements for *The American Statistician* to carry news and notes for the Institute of Mathematical Statistics and the Biometric Society as well as for ASA.

As the report continued, creation of these six joint committees was to be hailed as a major step forward toward more effective collaboration between statistical societies in pursuing common ends.

The joint publication of a directory of members was part of the initial thrust for avoiding duplication, and for a cooperative venture on the part of statistical societies. However, the needs of the member societies did not always coincide, and directories have been published jointly in 1973, 1978, 1987, and 1991, and separately by IMS in 1981. In 1996 it was generally agreed to plan for a joint directory to appear no less frequently than every three years. With the advent of computer technology, an up-to-date member directory became available at any time. However, the joint directory was concrete evidence of the benefits of collaboration between the statistical societies.

A meeting of the full COPSS Committee was held on Monday, September 10, 1962, to discuss a memorandum prepared by H.L. Lucas on (7) COPSS and its activities; (8) membership of COPSS Committees; (9) charges to COPSS Committees; and (10) Committee reports. The revised version of this report became the official document of COPSS. In addition to discussing the topics listed, COPSS established the following committees: (11) Standardization of Symbols and Notations; and (12) Memorial Session in Honor of Sir Ronald A. Fisher, who had died in Adelaide, Australia, on July 29.

The COPSS Committee on Organizational Changes and COPSS Executive Committee held a meeting on Friday, March 1, 1963, to discuss a number of items dealing with cooperative arrangements among societies in COPSS, including certain possible structural changes within the societies.

The items discussed were: (1) a national committee on statistics; (2) financial needs of COPSS; (3) liaison with related societies; (4) improvement of inter-society arrangements with respect to *The American Statistician*; (5) Mathematical Statistics Section of ASA; (6) implications of reorganization to IMS; (7) progress in coordination of ENAR and WNAR; (8) recommendations on Sections of ASA, other societies affiliated with ASA, and for improvement in structure and activities of COPSS; and (9) joint billings of ASA, ENAR, IMS, and WNAR.

Two meetings of COPSS on August 26, 1963, and September 6, 1963, were held to consider a national committee on statistics, action on a report on availability of new statisticians, review of reports from COPSS Committees, a committee on liaison with related societies, the problem of recruiting census enumerators, and a proposal for publishing statistical tables. The 13th COPSS Committee, Liaison with Statistical Societies, was appointed during the summer of 1963.

At their meeting on Thursday, January 16, 1964, the COPSS Executive Committee considered the distribution of the new edition of *Careers in Statistics*, a national committee on statistics, recommendations of the Committee on Standardization of Symbols and Notation, suggestions regarding the Liaison Committee, availability of statisticians, and other items.

At the meeting of COPSS held on Tuesday, December 29, 1964, the member-at-large, Walter T. Federer, was elected as Chairman and Executive Secretary of COPSS for a three-year term, 1965–67. Federer was subsequently reappointed for a second three-year term, 1968–70, a one-year term in 1971, and a second one-year term in 1972. After this change the Executive Committee no longer met, so the ASA and IMS both designated the President-Elect as the other officer on COPSS, and some committees were added and others disbanded.

1.2 COPSS activities in the early years

It was decided that the minutes of COPSS meetings were of sufficient general interest to be published in *The American Statistician*. Meeting minutes are found in the following issues: February 1964, p. 2 (meeting of January 16, 1964); April 1969, p. 37 (meeting of August 21, 1968); June 1970, p. 2 (meeting of August 21, 1969); April 1972, p. 38 (meeting of December 27, 1970); April 1972, p. 40 (meeting of August 23, 1971). In addition, the minutes of the Onchiota Conference Center meeting appear on p. 2 of the October 1960 issue (see also p. 41).

Membership lists of COPSS Committees were published in *Amstat News* as follows: February 1963, p. 31; April 1965, p. 61; April 1966, p. 44; April

1967, p. 48; April 1968, p. 40; February 1969, p. 39; April 1970, p. 48; April 1971, p. 47; April 1972, p. 55.

Other *Amstat News* citations relating to work on COPSS Committees are the "Brochure on Statistics as a Career" (April 1962, p. 4), the "AAAS Elects Statisticians as Vice Presidents" (April 1962, p. 4), "Statistics Section (U) of the AAAS, " by M.B. Ullman (February 1964, p. 9), and "Recommended Standards for Statistical Symbols and Notation," by M. Halperin, H.O. Hartley (Chairman), and P.G. Hoel (June 1965, p. 12).

The Academic Programs Committee was most beneficial, especially through the work of Franklin Graybill in several publications of the Committee on the Undergraduate Program in Mathematics (CUPM) relating to statistics, and of Paul Minton in preparing a list for *Amstat News* (October 1970, December 1971) of US and Canadian schools that offer degrees in statistics. The work of the Bernard Greenberg committee on preparing the "Careers in Statistics" brochure was particularly commendable as evidenced by the fact that several hundred thousand copies of the brochure were distributed. The work of other Committees as well helped COPSS to achieve its goals. To further their efforts, it was suggested that the committee members be placed on a rotating basis of three-year terms whenever possible, that certain members be considered ex-officio members of the committees, that the Committee for Liaison with Other Statistical Societies be studied to find more effective means of communication between societies, and that the Executive Committee of COPSS consider holding additional meetings to consider ways of strengthening statistics in the scientific community and nationally.

In 1965 the Conference Board of the Mathematical Sciences (CBMS) began conducting a survey of the state of undergraduate mathematical and statistical sciences in the nation. This survey is conducted every five years and ten full reports have been issued, the latest being in 2010. With its statistical expertise, COPSS participated in the 1990 and 1995 surveys.

COPSS was represented at meetings of the Conference Board of Mathematical Sciences, and this served to bring concerns of the statistical community to the attention of the cognate mathematics societies.

A chronic concern related to the annual meetings of the statistical sciences. For many years the IMS alternated meeting with the ASA in one year and with the American Mathematical Association and Mathematical Association of America in another year. Somehow schedules did not always mesh geographically. This led to a considerable amount of negotiation. At one point having Joint Statistical Meetings (JSM) with all societies included solved this problem.

In the early days of COPSS the position of Chair rotated among the Societies. Later a Chair and Secretary/Treasurer were chosen by the chairs of the member societies. These are listed in Table 1.1.

TABLE 1.1
List of COPSS Chairs and Secretary/Treasurers.

Period	Chair	Secretary/Treasurer
1994–96	Ingram Olkin	Lisa Weissfeld
1997–00	Marvin Zelen	Vickie Hertzberg
2001–03	Sallie Keller	Aparna Huzurbazar
2004–06	Linda Young	Karen Bandeen-Roche
2007–09	Jessica Utts	Madhuri Mulekar
2010–12	Xihong Lin	Bhramar Mukherjee
2013–15	Jane Pendergast	Maura Stokes

1.3 COPSS activities in recent times

From the beginning of the 1990s, COPSS has been an established organization with a host of activities. The main program is described below, along with a number of notable projects.

The joint "Careers" brochure was revised in 1991 and again in 1993, and was mailed to over 100,000 individuals, schools, and institutions. It has been subsequently revised by a committee consisting of Donald Bentley (Chair), Judith O'Fallon (ASA), Jeffrey Witner (IMS), Keith Soyer (ENAR), Kevin Cain (WNAR), and Cyntha Struthers representing the Statistical Society of Canada (SSC). The latter society joined COPSS as a member in 1981.

A task force was formed to recommend a revision of the 1991 Mathematics Subject Classification in Mathematical Reviews (MR). A committee (David Aldous, Wayne Fuller, Robb Muirhead, Ingram Olkin, Emanuel Parzen, Bruce Trumbo) met at the University of Michigan with Executive Editor Ronald Babbit and also with editors of the *Zentralblatt für Mathematik*.

In 2011 the US Centers for Medicare and Medicaid Services (CMS) asked COPSS to prepare a white paper on "Statistical Issues on Assessing Hospital Performance." Then Chair, Xihong Lin formed a committee consisting of Arlene Ash, Stephen Fienberg, Thomas Louis, Sharon-Lise Normand, Thérèse Stukel, and Jessica Utts to undertake this task. The report was to provide evaluation and guidance on statistical approaches used to estimate hospital performance metrics, with specific attention to statistical issues identified by the CMS and stakeholders (hospitals, consumers, and insurers). Issues related to modeling hospital quality based on outcomes using hierarchical generalized linear models. The committee prepared a very thoughtful and comprehensive report, which was submitted to CMS on November 30, 2011, and was posted on the CMS website.

1.3.1 The Visiting Lecturer Program in Statistics

The Visiting Lecturer Program (VLP) in Statistics was a major undertaking by COPSS. At the Annual Meeting of the IMS held at Stanford University in August 1960, discussion in the Council of the Institute, program presentations, and general comment forcibly re-emphasized the need to attract many more competent young people to professional careers in statistics. Governmental, educational, and industrial requirements for trained statisticians were not being met, and new positions were being created at a faster rate than the output of new statisticians could address. The difficulty was compounded by a paucity of information about careers in statistics, by the loss of instructional personnel to higher-paying, non-academic employment, and by competition from other sciences for students with mathematical skills. A proposal for a program of visiting scientists in statistics covering the years 1962–67 was drawn up and presented to the National Science Foundation. The program was funded in 1962 by means of a three-year NSF grant to set up the Visiting Lecturer Program in Statistics for 1963–64 and 1964–65 under the Chairmanship of Jack C. Kiefer. The VLP was administered by the IMS but became a COPSS Committee because of the nature of its activities.

The original rationale for the VLP was described as follows.

> "Statistics is a very broad and exciting field of work. The main purpose of this program is to convey this excitement to students and others who may be interested. Specifically, we hope that this program will:
>
> 1. Provide information on the nature of modern statistics.
> 2. Illustrate the importance of statistics in all fields of scientific endeavor, particularly those involving experimental research, and to encourage instruction in statistics to students in all academic areas and at all levels.
> 3. Create an awareness of the opportunities for careers in statistics for students with high quantitative and problem-solving abilities and to encourage them to seek advanced training in statistics.
> 4. Provide information and advice to university and college faculties and students on the present availability of advanced training in statistics.
> 5. Encourage the development of new courses and programs in statistics."

Over the years the objectives changed somewhat, and by 1995 the Program had five similar main objectives: (1) to provide education and information on the nature and scope of modern statistics and to correct misconceptions held in regard to the science; (2) to establish and emphasize the role that statistics plays in research and practice in all fields of scientific endeavor, particularly those involving experimental research, and to encourage instruction in statistical theory and application to students in all academic areas; (3) to create an awareness of the opportunities for careers in statistics among young men and

women of high potential mathematical ability and to encourage them to seek advanced training in statistics; (4) to provide information and advice to students, student counselors, and university and college faculties on the present availability of advanced training in statistics; (5) to encourage and stimulate new programs in statistics both to supplement programs in other curricula and to develop the further training of statisticians.

The 1963–64 VLP was highly successful. There were about 100 requests for lectures and over 70 visits were made by 31 lecturers. Almost every school which was assigned a speaker received the first of its three choices. Lecturers were urged to give two talks, one at a technical level and one on statistics as a career. The program continued into the 1990s. A 1991–93 report by then-Chair Lynne Billard noted that in the two-year period there were over 40 visits. These were mainly to universities in which there was no statistics department. Examples are Allegheny College, Carson Newman College, Bucknell University, Furman University, Moorhead State University, Memphis State University, University of Puerto Rico, and Wabash University, to name but a few. In general the program was not designed for universities with an active statistics department. The chairs of the VLP are listed in Table 1.2.

TABLE 1.2
List of Chairs of the Visiting Lecturer Program (VLP).

Period	Chair
1962–63	Jack C. Kiefer
1964–65	W. Jackson Hall
1966–67	Shanti S. Gupta
1970–71	Don B. Owen
1974–75	Herbert T. David
1984–86	Jon Kettenring
1987–89	Fred Leysieffer
1990–94	Lynne Billard

For a list of the lecturers and number of their visits, see *The American Statistician*: 1964, no. 4, p. 6; 1965, no. 4, p. 5; 1966, no 4, p. 12; 1967, no. 4, p. 8; 1968, no. 4, p. 3; 1970, no. 5, p. 2; 1971, no. 4, p. 2.

1.4 Awards

COPSS initiated a number of awards which have become prestigious hallmarks of achievement. These are the Presidents' Award, the R.A. Fisher Lectureship, the George W. Snedecor Award, the Elizabeth L. Scott Award, and the Florence Nightingale David Award. The COPSS Awards Ceremony usually takes

place on Wednesdays of the Joint Statistical Meetings. The Presidents' Award, the Snedecor or Scott Award, and the David Award are announced first, and are followed by the Fisher Lecture. All awards now include a plaque and a cash honorarium of $1,000.

The George W. Snedecor and Elizabeth L. Scott Awards receive support from an endowment fund. In the early days of these awards, financing was in a precarious state. This led to a solicitation for funds in August 1995 that raised $12,100. Subsequently a fiscal policy was established for the support of awards.

This history of COPSS provides us with an opportunity to thank the following donors for their assistance at that time: Abbott Laboratories ($500); Biopharmaceutical Research Consultants ($100); Bristol-Myers Squibb Pharmaceutical Research Institute ($500); Chapman & Hall ($1,000); COPSS ($4,250); Duxbury Press ($500); Institute of Mathematical Statistics ($500); Institute for Social Research Survey Research Center, University of Michigan ($500); Iowa State University ($750); Procter & Gamble ($500); Springer-Verlag ($500); Section on Statistical Graphics ($500); SYSTAT ($500); Trilogy Consulting Corporation ($500); John Wiley and Sons ($1,000).

1.4.1 Presidents' Award

COPSS sponsors the Presidents' Award and presents it to a young member of the statistical community in recognition of an outstanding contribution to the profession of statistics. The Presidents' Award was established in 1976 and is jointly sponsored by the American Statistical Association, the Institute of Mathematical Statistics, the Biometric Society ENAR, the Biometric Society WNAR, and the Statistical Society of Canada operating through COPSS. (In 1994 the Biometric Society became the International Biometric Society.) The first award was given in 1979, and it is presented annually.

According to the award description, "The recipient of the Presidents' Award shall be a member of at least one of the participating societies. The Presidents' Award is granted to an individual who has not yet reached his or her 40th birthday at the time of the award's presentation. The candidate may be chosen for a single contribution of extraordinary merit, or an outstanding aggregate of contributions, to the profession of statistics."

The Presidents' Award Committee consists of seven members, including one representative appointed by each of the five member societies plus the COPSS Chair, and a past awardee as an additional member. The Chair of the Award Committee is appointed by the Chair of COPSS.

Prior to 1988 the COPSS Presidents' Award was funded by the ASA. When the 1987 award depleted this account, COPSS voted to reallocate a fraction of membership dues to fund future awards. Recipients are listed in Table 1.3 with their affiliation at the time of the award.

TABLE 1.3
List of COPSS Presidents' Award winners.

Year	Winner	Affiliation (at the Time of Award)
1979	Peter J. Bickel	University of California, Berkeley
1981	Stephen E. Fienberg	Carnegie Mellon University
1983	Tze Leung Lai	Columbia University
1984	David V. Hinkley	University of Texas, Austin
1985	James O. Berger	Purdue University
1986	Ross L. Prentice	Fred Hutchinson Cancer Research Center
1987	Chien-Fu Jeff Wu	University of Wisconsin, Madison
1988	Raymond J. Carroll	Texas A&M University
1989	Peter Hall	Australian National University
1990	Peter McCullagh	University of Chicago
1991	Bernard W. Silverman	University of Bristol, UK
1992	Nancy M. Reid	University of Toronto, Canada
1993	Wing Hung Wong	University of Chicago
1994	David L. Donoho	Stanford University
1995	Iain M. Johnstone	Stanford University
1996	Robert J. Tibshirani	University of Toronto, Canada
1997	Kathryn Roeder	Carnegie Mellon University
1998	Pascal Massart	Université de Paris-Sud, France
1999	Larry A. Wasserman	Carnegie Mellon University
2000	Jianqing Fan	University of North Carolina, Chapel Hill
2001	Xiao-Li Meng	Harvard University
2002	Jun Liu	Harvard University
2003	Andrew Gelman	Columbia University
2004	Michael A. Newton	University of Wisconsin, Madison
2005	Mark J. van der Laan	University of California, Berkeley
2006	Xihong Lin	Harvard University
2007	Jeffrey S. Rosenthal	University of Toronto, Canada
2008	T. Tony Cai	University of Pennsylvania
2009	Rafael Irizarry	Johns Hopkins University
2010	David B. Dunson	Duke University
2011	Nilanjan Chatterjee	National Cancer Institute
2012	Samuel Kou	Harvard University
2013	Marc Suchard	University of California, Los Angeles

1.4.2 R.A. Fisher Lectureship

The R.A. Fisher Lectureship was established in 1963 to honor both the contributions of Sir Ronald Aylmer Fisher and a present-day statistician for their advancement of statistical theory and applications. The list of past Fisher Lectures well reflects the prestige that COPSS and its constituent societies place on this award. Awarded each year, the Fisher Lectureship represents meritorious achievement and scholarship in statistical science, and recognizes highly significant impacts of statistical methods on scientific investigations. The Lecturer is selected by the R.A. Fisher Lecture and Award Committee which is chosen to reflect the interests of the member societies. The Lecture has become an integral part of the COPSS program, and is given at the Joint Statistical Meeting.

In the early days of COPSS, the award of the Lectureship was governed by the following conditions: (1) The Fisher Lectureship is awarded annually to an eminent statistician for outstanding contributions to the theory and application of statistics; (2) the Fisher Lecture shall be presented at a designated Annual Meeting of the COPSS societies; (3) the Lecture shall be broadly based and emphasize those aspects of statistics and probability which bear close relationship to the scientific collection and interpretation of data, areas in which Fisher himself made outstanding contributions; (4) the Lecture shall be scheduled so as to have no conflict with any other session at the Annual Meeting; (5) the Chair of the Lecture shall be the Chair of the R.A. Fisher Lecture and Award Committee or the Chair's designee: the Chair shall present a short statement on the life and works of R.A. Fisher, not to exceed five minutes in duration, and an appropriate introduction for the Fisher Lecturer; (6) the Lecturer is expected to prepare a manuscript based on the Lecture and to submit it to an appropriate statistical journal. There is an additional honorarium of $1,000 upon publication of the Fisher Lecture.

The recipients of the R.A. Fisher Lectureship are listed in Table 1.4, together with the titles of their lectures and their affiliations at the time of the award.

TABLE 1.4
Recipients of the R.A. Fisher Lectureship and titles of their lectures.

Year	Winner and Affiliation (*Title of the Talk*)
1964	Maurice S. Bartlett, University of Chicago and University College London, UK *R.A. Fisher and the last fifty years of statistical methodology*
1965	Oscar Kempthorne, Iowa State University *Some aspects of experimental inference*
1966	(none)
1967	John W. Tukey, Princeton University and Bell Labs *Some perspectives in data analysis*

TABLE 1.4
Recipients of the R.A. Fisher Lectureship (cont'd).

Year	Winner and Affiliation (*Title of the Talk*)
1968	Leo A. Goodman, University of Chicago *The analysis of cross-classified data: Independence, quasi-independence, and interactions in contingency tables with or without missing entries*
1970	Leonard J. Savage, Princeton University *On rereading R.A. Fisher*
1971	Cuthbert Daniel, Private Consultant *One-at-a-time plans*
1972	William G. Cochran, Harvard University *Experiments for nonlinear functions*
1973	Jerome Cornfield, George Washington University *On making sense of data*
1974	George E.P. Box, University of Wisconsin, Madison *Science and statistics*
1975	Herman Chernoff, Massachusetts Institute of Technology *Identifying an unknown member of a large population*
1976	George A. Barnard, University of Waterloo, Canada *Robustness and the logic of pivotal inference*
1977	R.C. Bose, University of North Carolina *R.A. Fisher's contribution to multivariate analysis and design of experiments*
1978	William H. Kruskal, University of Chicago *Statistics in society: Problems unsolved and unformulated*
1979	C.R. Rao, The Pennsylvania State University *Fisher efficiency and estimation of several parameters*
1980	(none)
1981	(none)
1982	Frank J. Anscombe, Yale University *How much to look at the data*
1983	I. Richard Savage, University of Minnesota *Nonparametric statistics and a microcosm*
1984	(none)
1985	T.W. Anderson, Stanford University *R.A. Fisher and multivariate analysis*
1986	David H. Blackwell, University of California, Berkeley *Likelihood and sufficiency*
1987	Frederick Mosteller, Harvard University *Methods for studying coincidences* (with P. Diaconis)
1988	Erich L. Lehmann, University of California, Berkeley *Model specification: Fisher's views and some later strategies*
1989	Sir David R. Cox, Nuffield College, Oxford *Probability models: Their role in statistical analysis*
1990	Donald A.S. Fraser, York University, Canada *Statistical inference: Likelihood to significance*
1991	David R. Brillinger, University of California, Berkeley *Nerve cell spike train data analysis: A progression of technique*

TABLE 1.4
Recipients of the R.A. Fisher Lectureship (cont'd).

Year	Winner and Affiliation (*Title of the Talk*)
1992	Paul Meier, Columbia University
	The scope of general estimation
1993	Herbert E. Robbins, Columbia University
	N and n: Sequential choice between two treatments
1994	Elizabeth A. Thompson, University of Washington
	Likelihood and linkage: From Fisher to the future
1995	Norman E. Breslow, University of Washington
	Statistics in epidemiology: The case-control study
1996	Bradley Efron, Stanford University
	R.A. Fisher in the 21st century
1997	Colin L. Mallows, AT&T Bell Laboratories
	The zeroth problem
1998	Arthur P. Dempster, Harvard University
	Logistic statistics: Modeling and inference
1999	John D. Kalbfleisch, University of Waterloo, Canada
	The estimating function bootstrap
2000	Ingram Olkin, Stanford University
	R.A. Fisher and the combining of evidence
2001	James O. Berger, Duke University
	Could Fisher, Jeffreys, and Neyman have agreed on testing?
2002	Raymond J. Carroll, Texas A&M University
	Variability is not always a nuisance parameter
2003	Adrian F.M. Smith, University of London, UK
	On rereading L.J. Savage rereading R.A. Fisher
2004	Donald B. Rubin, Harvard University
	Causal inference using potential outcomes:
	Design, modeling, decisions
2005	R. Dennis Cook, University of Minnesota
	Dimension reduction in regression
2006	Terence P. Speed, University of California, Berkeley
	Recombination and linkage
2007	Marvin Zelen, Harvard School of Public Health
	The early detection of disease: Statistical challenges
2008	Ross L. Prentice, Fred Hutchinson Cancer Research Center
	The population science research agenda:
	Multivariate failure time data analysis methods
2009	Noel Cressie, The Ohio State University
	Where, when, and then why
2010	Bruce G. Lindsay, Pennsylvania State University
	Likelihood: Efficiency and deficiency
2011	C.F. Jeff Wu, Georgia Institute of Technology
	Post-Fisherian experimentation: From physical to virtual
2012	Roderick J. Little, University of Michigan
	In praise of simplicity not mathematistry!
	Simple, powerful ideas for the statistical scientist
2013	Peter J. Bickel, University of California, Berkeley
	From Fisher to "Big Data": Continuities and discontinuities

1.4.3 George W. Snedecor Award

Established in 1976, this award honors George W. Snedecor who was instrumental in the development of statistical theory in biometry. It recognizes a noteworthy publication in biometry appearing within three years of the date of the award. Since 1991 it has been given every other year in odd years.

George W. Snedecor was born on October 20, 1881, in Memphis, TN, and was educated at the Alabama Polytechnic Institute, the University of Alabama, and the University of Michigan. He joined the faculty of Iowa State College (University) in 1913 and taught there for 45 years. In 1924, he and his colleague Henry Wallace (who became Secretary of Agriculture, 1933–40, 33rd Vice President of the United States, 1941–45, and Secretary of Commerce, 1945–46) organized a seminar to study regression and data analysis. He formed the Iowa State Statistics Laboratory in 1933 and served as Director. His book *Statistical Methods* was published in 1937, and later, with William G. Cochran as co-author, went through seven editions. Iowa State's Department of Statistics separated from the Mathematics Department in 1939; it offered a Master's in statistics, the first of which was given to Gertrude Cox.

The F distribution, which is central to the analysis of variance, was obtained by Snedecor and called F after Fisher. Snedecor served as president of the American Statistical Association in 1948, was named an Honorary Fellow of the Royal Statistical Society in 1954, and received an honorary Doctorate of Science from North Carolina State University in 1956. Further details about Snedecor are contained in "Tales of Statisticians" and "Statisticians in History" (*Amstat News*, September 2009, pp. 10–11).

The recipients of the George W. Snedecor Award are listed in Table 1.5, along with references for the awarded publications.

TABLE 1.5
Recipients of the George W. Snedecor Award and publication(s).

1977	A. Philip Dawid Properties of diagnostic data distribution. *Biometrics*, 32:647–658.
1978	Bruce W. Turnbull and Toby J. Mitchell Exploratory analysis of disease prevalence data from survival/sacrifice experiments. *Biometrics*, 34:555–570.
1979	Ethel S. Gilbert The assessment of risks from occupational exposure to ionizing radiation. In *Energy and Health*, SIAM–SIMS Conference Series No. 6 (N. Breslow, Ed.), SIAM, Philadelphia, PA, pp. 209–225.
1981	Barry H. Margolin, Norman Kaplan, and Errol Zeiger Statistical analysis of the *Ames salmonella*/microsome test. *Proceedings of the National Academy of Science*, 78:3779–3783.

TABLE 1.5
Recipients of the George W. Snedecor Award (cont'd).

1982	Byron J.T. Morgan Modeling polyspermy. *Biometrics*, 38:885–898.
1983	Cavell Brownie and Douglas S. Robson Estimation of time-specific survival rates from tag-resighting samples: A generalization of the Jolly–Seber model. *Biometrics*, 39:437–453; and
1983	R.A. Maller, E.S. DeBoer, L.M. Joll, D.A. Anderson, and J.P. Hinde Determination of the maximum foregut volume of Western Rock Lobsters (*Panulirus cygnus*) from field data. *Biometrics*, 39:543–551.
1984	Stuart H. Hurlbert Pseudoreplication and the design of ecological field experiments. *Ecological Monographs*, 54:187–211; and
1984	John A. Anderson Regression and ordered categorical variables. *Journal of the Royal Statistical Society, Series B*, 46:1–30.
1985	Mitchell H. Gail and Richard Simon Testing for qualitative interactions between treatment effects and patients subsets. *Biometrics*, 41:361–372.
1986	Kung-Yee Liang and Scott L. Zeger Longitudinal data analysis using generalized linear models. *Biometrika*, 73:13–22; and Longitudinal data analysis for discrete and continuous outcomes. *Biometrics*, 42:121–130.
1987	George E. Bonney Regressive logistic models for familial disease and other binary traits. *Biometrics*, 42:611–625; Logistic regression for dependent binary observations. *Biometrics*, 43:951–973.
1988	Karim F. Hirji, Cyrus R. Mehta, and Nitin R. Patel Exact inference for matched case-control studies. *Biometrics*, 44:803–814.
1989	Barry I. Graubard, Thomas R. Fears, and Mitchell H. Gail Effects of cluster sampling on epidemiologic analysis in population-based case-control studies. *Biometrics*, 45:1053–1071.
1990	Kenneth H. Pollack, James D. Nichols, Cavell Brownie, and James E. Hines Statistical inference for capture-recapture experiments. *Wildlife Monographs*, The Wildlife Society 107.
1993	Kenneth L. Lange and Michael L. Boehnke Bayesian methods and optimal experimental design for gene mapping by radiation hybrid. *Annals of Human Genetics*, 56:119–144.

TABLE 1.5
Recipients of the George W. Snedecor Award (cont'd).

1995	Norman E. Breslow and David Clayton Approximate inference in generalized linear models. *Journal of the American Statistical Association*, 88:9–25.
1997	Michael A. Newton Bootstrapping phylogenies: Large deviations and dispersion effects. *Biometrika*, 83:315–328; and
1997	Kathryn Roeder, Raymond J. Carroll, and Bruce G. Lindsay A semiparametric mixture approach to case-control studies with errors in covariables. *Journal of the American Statistical Association*, 91:722–732.
1999	Daniel Scharfstein, Anastasios Butch Tsiatis, and Jamie Robins Semiparametric efficiency and its implications on the design and analysis of group sequential studies. *Journal of the American Statistical Association*, 92:1342–1350.
2001	Patrick J. Heagerty Marginally specified logistic-normal models for longitudinal binary data. *Biometrics*, 5:688–698.
2003	Paul R. Rosenbaum Effects attributable to treatment: Inference in experiments and observational studies with a discrete pivot. *Biometrika*, 88:219–231; and Attributing effects to treatment in matched observational studies. *Journal of the American Statistical Association*, 97:183–192.
2005	Nicholas P. Jewell and Mark J. van der Laan Case-control current status data. *Biometrika*, 91:529–541.
2007	Donald B. Rubin The design versus the analysis of observational studies for causal effects: Parallels with the design of randomized trials. *Statistics in Medicine*, 26:20–36.
2009	Marie Davidian Improving efficiency of inferences in randomized clinical trials using auxiliary covariates. *Biometrics*, 64:707–715 (by M. Zhang, A.A. Tsiatis, and M. Davidian).
2011	Nilanjan Chatterjee Shrinkage estimators for robust and efficient inference in haplotype-based case-control studies. *Journal of the American Statistical Association*, 104:220–233 (by Y.H. Chen, N. Chatterjee, and R.J. Carroll).
2013	John D. Kalbfleisch Pointwise nonparametric maximum likelihood estimator of stochastically ordered survival functions. *Biometrika*, 99:327–343 (by Y. Park, J.M.G. Taylor, and J.D. Kalbfleisch).

1.4.4 Elizabeth L. Scott Award

In recognition of Elizabeth L. Scott's lifelong efforts to further the careers of women, this award is presented to an individual who has helped foster opportunities in statistics for women by developing programs to encourage women to seek careers in statistics; by consistently and successfully mentoring women students or new researchers; by working to identify gender-based inequities in employment; or by serving in a variety of capacities as a role model. First awarded in 1992, it is given every other year in even-numbered years.

Elizabeth Scott was born in Fort Sill, Oklahoma, on November 23, 1917. Her family moved to Berkeley, where she remained for the rest of her life. She was in the UC Berkeley astronomy program and published more than ten papers on comet positions. She received her PhD in 1949. Her dissertation was part astronomy and part statistics: "(a) Contribution to the problem of selective identifiability of spectroscopic binaries; (b) Note on consistent estimates of the linear structural relation between two variables." She collaborated with Jerzy Neyman on astronomical problems as well as weather modification.

In 1970 Elizabeth Scott co-chaired a university sub-committee which published a comprehensive study on the status of women in academia. Subsequently she led follow-up studies concerning gender-related issues such as salary discrepancies and tenure and promotion. She developed a toolkit for evaluating salaries that was distributed by the American Association of University Professors and used by many academic women to argue successfully for salary adjustments. She often told of her history in the Astronomy Department which provided a telescope to every male faculty, but not to her. She received many honors and awards, and served as president of the IMS, 1977–78, and of the Bernoulli Society, 1983–85. She was Chair of the Statistics Department from 1968 to 1973. She was a role model for many of the women who are our current leaders. She died on December 20, 1988.

TABLE 1.6
Recipients of the Elizabeth L. Scott Award.

Year	Winner	Affiliation (at the Time of the Award)
1992	Florence N. David	University of California, Riverside
1994	Donna Brogan	University of North Carolina, Chapel Hill
1996	Grace Wahba	University of Wisconsin, Madison
1998	Ingram Olkin	Stanford University
2000	Nancy Flournoy	University of Missouri, Columbia
2002	Janet Norwood	Bureau of Labor Statistics
2004	Gladys Reynolds	Centers for Disease Control and Prevention
2006	Louise Ryan	Harvard University
2008	Lynne Billard	University of Georgia
2010	Mary E. Thompson	University of Waterloo, Canada
2012	Mary W. Gray	American University

For more details of her life and accomplishments, the web site "Biographies of Women Mathematicians" (http://www.agnesscott.edu/lriddle/women) recommends: (1) "Elizabeth Scott: Scholar, Teacher, Administrator," *Statistical Science*, 6:206–216; (2) "Obituary: Elizabeth Scott, 1917–1988," *Journal of the Royal Statistical Society, Series A*, 153:100; (3) "In memory of Elizabeth Scott," *Newsletter of the Caucus for Women in Statistics*, 19:5–6. The recipients of the Elizabeth L. Scott Award are listed in Table 1.6.

1.4.5 Florence Nightingale David Award

This award recognizes a female statistician who exemplifies the contributions of Florence Nightingale David, an accomplished researcher in combinatorial probability theory, author or editor of numerous books including a classic on the history of probability theory, *Games, Gods, and Gambling*, and first recipient of the Elizabeth L. Scott Award. Sponsored jointly by COPSS and the Caucus for Women in Statistics, the award was established in 2001 and consists of a plaque, a citation, and a cash honorarium. It is presented every other year in odd-numbered years if, in the opinion of the Award Committee, an eligible and worthy nominee is found. The Award Committee has the option of not giving an award for any given year.

F.N. David was born in the village of Irvington in Herefordshire, England, on August 23, 1909. She graduated from Bedford College for Women in 1931 with a mathematics degree. She sought advice from Karl Pearson about obtaining an actuarial position, but instead was offered a research position at University College, London. David collaborated with Pearson and Sam Stouffer (a sociological statistician) on her first paper, which appeared in 1932. Neyman was a visitor at this time and urged her to complete her PhD, which she did in 1938. During the war, she served as a statistician in military agencies. She remained at University College until 1967 when she joined the University of California at Riverside, serving as Chair of Biostatistics which was later renamed the Department of Statistics. Her research output was varied and included both theory and applications. She published *Probability Theory for Statistical Methods* in 1949, and jointly with D.E. Barton, *Combinatorial Chance* in 1962. David died in 1993 at the age of 83. The recipients of the F.N. David Award are listed in Table 1.7.

TABLE 1.7
Recipients of the Florence Nightingale David Award.

Year	Recipient	Affiliation
2001	Nan M. Laird	Harvard University
2003	Juliet Popper Shaffer	University of California, Berkeley
2005	Alice S. Whittemore	Stanford University
2007	Nancy Flournoy	University of Missouri, Columbia
2009	Nancy M. Reid	University of Toronto, Canada
2011	Marie Davidian	North Carolina State University
2013	Lynne Billard	University of Georgia

Part II

Reminiscences and personal reflections on career paths

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2

Reminiscences of the Columbia University Department of Mathematical Statistics in the late 1940s

Ingram Olkin

Department of Statistics
Stanford University, Stanford, CA

2.1 Introduction: Pre-Columbia

Every once in a while in a dinner conversation, I have recalled my student days at Columbia, and have met with the suggestion that I write up these recollections. Although present-day students may recognize some of the famous names such as Hotelling, Wald, and Wolfowitz, they won't meet many faculty who were their students. The following is the result, and I hope the reader finds these reminiscences interesting. Because recollections of 60 years ago are often inaccurate, I urge readers to add to my recollections.

I started City College (CCNY) in 1941 and in 1943 enlisted in the US Army Air Force meteorology program. After completion of the program, I served as a meteorologist at various airports until I was discharged in 1946. I returned to CCNY and graduated in 1947, at which time I enrolled at Columbia University. As an aside, the professor at CCNY was Selby Robinson. Although not a great teacher, he somehow inspired a number of students to continue their study of statistics. Kenneth Arrow, Herman Chernoff, Milton Sobel, and Herbert Solomon are several who continued their studies at Columbia after graduating from CCNY.

Harold Hotelling was a key figure in my career. After receiving a doctorate at Princeton, Hotelling was at Stanford from 1924 to 1931, at the Food Research Institute and the Mathematics Department. In 1927 he taught three courses at Stanford: mathematical statistics (among the very early faculty to teach a rigorous course in statistics), differential geometry, and topology (who would tackle this today?). In 1931 he moved to Columbia, where he wrote his most famous papers in economics and in statistics (principal components, canonical correlations, T^2, to mention but a few). His 1941 paper on

the teaching of statistics had a phenomenal impact. Jerzy Neyman stated that it was one of the most influential papers in statistics. Faculty attempting to convince university administrators to form a Department of Statistics often used this paper as an argument why the teaching of statistics should be done by statisticians and not by faculty in substantive fields that use statistics. To read more about Hotelling, see Olkin and Sampson (2001a,b).

2.2 Columbia days

At Columbia, Hotelling had invited Abraham Wald to Columbia in 1938, and when Hotelling left in 1946 to be Head of the Statistics Department at Chapel Hill, Wald became Chair of the newly formed department at Columbia. The department was in the Faculty of Political Economy because the Mathematics Department objected to statistics being in the same division. The two economists F.E. Croxton and F.C. Mills taught statistics in the Economics Department and insisted that the new department be the Department of Mathematical Statistics to avoid any competition with their program. The other faculty were Ted Anderson, Jack Wolfowitz, later joined by Howard Levene and Henry Scheffé; Helen Walker was in the School of Education. (Helen was one of a few well-known, influential female statisticians. One source states that she was the first woman to teach statistics.) For a detailed history of the department, see Anderson (1955).

In the late 1940s Columbia, Chapel Hill, and Berkeley were statistical centers that attracted many visitors. There were other universities that had an impact in statistics such as Princeton, Iowa State, Iowa, Chicago, Stanford, and Michigan, but conferences were mostly held at the top three. The first two Berkeley Symposia were in 1946 and 1950, and these brought many visitors from around the world.

The Second Berkeley Symposium brought a galaxy of foreign statisticians to the US: Paul Lévy, Bruno de Finetti, Michel Loève, Harold Cramér, Aryeh Dvoretzky, and Robert Fortet. Domestic faculty were present as well, such as Richard Feynman, Kenneth Arrow, Jacob Marshak, Harold Kuhn, and Albert Tucker. Because some of the participants came from distant lands, they often visited other universities as part of the trip. During the 1946–48 academic years the visitors were Neyman, P.L Hsu, J.L. Doob, M.M. Loève, E.J.G. Pitman, R.C. Bose, each teaching special-topic courses. Later Bose and Hsu joined Hotelling at Chapel Hill.

With the GI Bill, I did not have to worry about tuition, and enrolled at Columbia in two classes in the summer of 1947. The classes were crowded with post-war returnees. One class was a first course in mathematical statistics that was taught by Wolfowitz. Some of the students at Columbia during the 1947–50 period were Raj Bahadur and Thelma Clark (later his wife), Bob

Bechhofer, Allan Birnbaum, Al Bowker, Herman Chernoff (he was officially at Brown University, but worked with Wald), Herb T. David, Cyrus Derman, Sylvan Ehrenfeld, Harry Eisenpress, Peter Frank, Leon Herbach, Stanley Isaacson, Seymour Jablon, Jack Kiefer, Bill Kruskal, Gerry Lieberman, Gottfried Noether, Rosedith Sitgreaves, Milton Sobel, Herbert Solomon, Charles Stein, Henry Teicher, Lionel Weiss, and many others. Columbia Statistics was an exciting place, and almost all of the students continued their career in statistics. There was a feeling that we were in on the ground floor of a new field, and in many respects we were. From 1950 to 1970 *The Annals of Mathematical Statistics* grew from 625 to 2200 pages, with many articles from the students of this era.

Some statistics classes were held at night starting at 5:40 and 7:30 so that students who worked during the day could get to class. However, math classes took place during the day. I took sequential analysis and analysis of variance from Wald, core probability from Wolfowitz, finite differences from B.O. Koopman, linear algebra from Howard Levi, differential equations from Ritt, a computer science course at the Columbia Watson Lab, and a course on analysis of variance from Helen Walker. Anderson taught multivariate analysis the year before I arrived. Govind Seth and Charles Stein took notes from this course, which later became Anderson's book on multivariate analysis.

Wald had a classic European lecture style. He started at the upper left corner of the blackboard and finished at the lower right. The lectures were smooth and the delivery was a uniform distribution. Though I had a lovely set of notes, Wald treated difficult and easy parts equally, so one did not recognize pitfalls when doing homework. The notion of an application in its current use did not exist. I don't recall the origin of the following quotation, but it is attributed to Wald: "Consider an application. Let $X_1, \ldots, X_n$ be i.i.d. random variables." In contrast to Wald's style, Wolfowitz's lectures were definitely not smooth, but he attempted to emphasize the essence of the topic. He struggled to try to explain what made the theorem "tick," a word he often used: "Let's see what makes this tick." However, as a novice in the field the gems of insight that he presented were not always appreciated. It was only years later as a researcher that they resurfaced, and were found to be illuminating.

Wolfowitz had a number of other pet phrases such as "It doesn't cut any ice," and "stripped of all baloney." It was a surprise to hear Columbia graduates years later using the same phrase. In a regression class with Wolfowitz we learned the Gauss–Seidel method. Wolfowitz was upset that the Doolittle method had a name attached to it, and he would exclaim, "Who is this Doolittle?" Many years later when Wolfowitz visited Stanford a name might arise in a conversation. If Wolfowitz did not recognize the name he would say "Jones, Jones, what theorem did he prove?"

In 1947–48 the only serious general textbooks were Cramér, Kendall, and Wilks' soft-covered notes. This was a time when drafts of books were being written. Feller's Volume I appeared in 1950, Doob's book on stochastic pro-

cesses in 1953, Lehmann's notes on estimation and testing of hypotheses in 1950, Scheffé's book on analysis of variance in 1953. The graduate students at Columbia formed an organization that duplicated lecture notes, especially those of visitors. Two that I remember are Doob's lectures on stochastic processes and Loève's on probability.

The Master's degree program required a thesis and mine was written with Wolfowitz. The topic was on a sequential procedure that Leon Herbach (he was ahead of me) had worked on. Wolfowitz had very brief office hours, so there usually was a queue to see him. When I did see him in his office he asked me to explain my question at the blackboard. While talking at the blackboard Wolfowitz was multi-tasking (even in 1947) by reading his mail and talking on the telephone. I often think of this as an operatic trio in which each singer is on a different wavelength. This had the desired effect in that I never went back. However, I did manage to see him after class. He once said "Walk me to the subway while we are talking," so I did. We did not finish our discussion by the time we reached the subway (only a few blocks away) so I went into the subway where we continued our conversation. This was not my subway line so it cost me a nickel to talk to him. One of my students at Stanford 30 years later told me that I suggested that he walk with me while discussing a problem. There is a moral here for faculty.

Wald liked to take walks. Milton Sobel was one of Wald's students and he occasionally accompanied Wald on these walks. Later I learned that Milton took his students on walks. I wonder what is the 21st century current version of faculty-student interaction?

2.3 Courses

The Collyer brothers became famous for their compulsive collecting. I am not in their league, but I have saved my notes from some of the courses that I took. The following is an excerpt from the Columbia course catalog.

> Mathematical Statistics 111a — Probability. 3 points Winter Session. Professor Wolfowitz.
>
> Tu. Th. 5:40–6:30 and 7:30–8:20 p.m. 602 Hamilton.
>
> Fundamentals. Combinatorial problems. Distribution functions in one or more dimensions. The binomial, normal, and Poisson laws. Moments and characteristic functions. Stochastic convergence and the law of large numbers. Addition of chance variables and limit theorems.
>
> This course terminates on Nov. 18. A thorough knowledge of calculus is an essential prerequisite. Students are advised to study higher algebra simultaneously to obtain a knowledge of matrix algebra for use in more advanced mathematical statistics courses.

Milton Sobel was the teaching assistant for the 111a course; Robert Bechofer and Allan Birnbaum were also TAs. I remember that Milton Sobel sat in on Wald's class on analysis of variance. Because he was at least one year ahead of me I thought that he would have taken this course earlier. He said he did take it earlier but the course was totally different depending on who was teaching it. It was depressing to think that I would have to take every course twice! As the course progressed Wald ran out of subscripts and superscripts on the right-hand side, e.g., $x_{ij}^{k\ell}$, and subsequently added some subscripts on the left-hand side.

Wolfowitz recommended three books, and assigned homework from them:

(a) H. Cramér (1945): *Mathematical Methods of Statistics*

(b) J.V. Uspensky (1937): *Introduction to Mathematical Probability*

(c) S.S. Wilks (1943): *Mathematical Statistics*

He mentioned references to Kolmogorov's *Foundation of Probability* and the Lévy and Roth book *Elements of Probability.*

Wolfowitz used the term "chance variables" and commented that the Law of Small Numbers should have been called the Law of Small Probabilities. As I look through the notes it is funny to see the old-fashioned factorial symbols $\lfloor n$ instead of $n!$. As I reread my notes it seems to me that this course was a rather simplified first course in probability. Some of the topics touched upon the use of independence, Markov chains, joint distributions, conditional distributions, Chebychev's inequality, stochastic convergence (Slutsky's theorem), Law of Large Numbers, convolutions, characteristic functions, Central Limit Theorem (with discussion of Lyapunov and Lindeberg conditions). I have a comment in which Wolfowitz notes an error in Cramér (p. 343): (a) if $y_1, y_2, \ldots$ is a sequence with $\sum y_i = c_i$ for all c and $\sigma^2(y_i) \to 0$ as $i \to \infty$, then $p\lim_{i\to\infty}(y_i - c_i) = 0$; (b) the converse is not true in that it may be that $\sigma^2(y_i) \to \infty$ and yet $p\lim(y_i - c_i) = 0$.

The second basic course was 111b, taught by Wald. The topics included point estimation, consistency, unbiasedness, asymptotic variance, maximum likelihood, likelihood ratio tests, efficiency. This course was more mathematical than 111a in that there was more asymptotics. In terms of mathematical background I note that he used Lagrange multipliers to show that, for $w_1, \ldots, w_n \in [0, 1]$, $\sum_{i=1}^n w_i^2/(\sum_{i=1}^n w_i)^2$ is minimized when $w_i = 1/n$ for all $i \in \{1, \ldots, n\}$. Apparently, convexity was not discussed.

There is a derivation of the chi-square distribution that includes a discussion of orthogonal matrices. This is one of the standard proofs. Other topics include Schwarz inequality (but not used for the above minimization), and sufficiency. The second part of the course dealt with tests of hypotheses, with emphasis on the power function (Wald used the term "power curve"), acceptance sampling, and the OC curve.

My Columbia days are now over 65 years ago, but I still remember them as exciting and an incubator for many friendships and collaborations.

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3

A career in statistics

Herman Chernoff
Department of Statistics
Harvard University, Cambridge, MA

3.1 Education

At the early age of 15, I graduated from Townsend Harris high school in New York and made the daring decision to study mathematics at the City College of New York (CCNY) during the depression, rather than some practical subject like accounting. The Mathematics faculty of CCNY was of mixed quality, but the mathematics majors were exceptionally good. Years later, one of the graduate students in statistics at Stanford found a copy of the 1939 yearbook with a picture of the Math Club. He posted it with a sign "Know your Faculty." At CCNY we had an excellent training in undergraduate mathematics, but since there was no graduate program, there was no opportunity to take courses in the advanced subjects of modern research. I was too immature to understand whether my innocent attempts to do original research were meaningful or not. This gave me an appetite for applied research where successfully confronting a real problem that was not trivial had to be useful.

While at CCNY, I took a statistics course in the Mathematics Department, which did not seem very interesting or exciting, until Professor Selby Robinson distributed some papers for us to read during the week that he had to be away. My paper was by Neyman and Pearson (1933). It struck me as mathematically trivial and exceptionally deep, requiring a reorganization of my brain cells to confront statistical issues. At that time I had not heard of R.A. Fisher, who had succeeded in converting statistics to a theoretical subject in which mathematicians could work. Of course, he had little use for mathematicians in statistics, on the grounds that they confronted the wrong problems and he was opposed to Neyman–Pearson theory (NP).

Once when asked how he could find the appropriate test statistic without recourse to NP, his reply was "I have no trouble." In short, NP made explicit the consideration of the alternative hypotheses necessary to construct good tests. This consideration was implicit for statisticians who understood their

problem, but often unclear to outsiders and students. Fisher viewed it as an unnecessary mathematization, but the philosophical issue was important. Years later Neyman gave a talk in which he pointed out that the NP Lemma was highly touted but trivial. On the other hand it took him years of thinking to understand and state the issue.

Just before graduation I received a telegram offering me a position, which I accepted, as Junior Physicist at the Dahlgren Naval Proving Grounds in Virginia. After a year and a half I left Dahlgren to study applied mathematics at Brown University in Providence, Rhode Island. Dean Richardson had set up a program in applied mathematics where he could use many of the distinguished European mathematician émigrés to teach and get involved in research for the Defense Department, while many of the regular faculty were away working at Defense establishments. There was a good deal of coming and going of faculty, students and interesting visitors during this program.

During the following year I worked very hard as a Research Assistant for Professor Stefan Bergman and took many courses and audited a couple. I wrote a Master's thesis under Bergman on the growth of solutions of partial differential equations generated by his method, and received an ScM degree. One of the courses I audited was given by Professor Willy Feller, in which his lectures were a preliminary to the first volume of his two volume outstanding books on probability.

During the following summer, I took a reading course in statistics from Professor Henry Mann, a number theorist who had become interested in statistics because some number theory issues were predominant in some of the work going on in experimental design. In fact, he had coauthored a paper with Abraham Wald (Mann and Wald, 1943) on how the o and O notation could be extended to o_p and O_p. This paper also proved that if X_n has as its limiting distribution that of Y and g is a continuous function, then $g(X_n)$ has as its limiting distribution that of $g(Y)$.

Mann gave me a paper by Wald (1939) on a generalization of inference which handled that of estimation and testing simultaneously. Once more I found this paper revolutionary. This was apparently Wald's first paper on decision theory. Although it did not resemble the later version of a game against nature, it clearly indicated the importance of cost considerations in statistical philosophy. Later discussions with Allen Wallis indicated that Wald had been aware of von Neumann's ideas about game theory. My theory is that in this first paper, he had not recognized the relevance, but as his work in this field grew, the formulation gradually changed to make the relationship with game theory clearer. Certainly the role of mixed strategies in both fields made the relation apparent.

At the end of the summer, I received two letters. One offered me a predoctoral NSF fellowship and the other an invitation I could not decline, to join the US Army. It was 1945, the war had ended, and the draft boards did not see much advantage in deferring young men engaged in research on the war effort. I was ordered to appear at Fort Devens, Massachusetts, where I was

given three-day basic training and assigned to work as a clerk in the separation center busy discharging veterans. My hours were from 5 PM to midnight, and I considered this my first vacation in many years. I could visit Brown on weekends and on one of these visits Professor Prager suggested that I might prefer to do technical work for the army. He arranged for me to be transferred to Camp Lee, Virginia, where I was designated to get real basic training and end up, based on my previous experience, as a clerk in the quartermaster corps in Germany. I decided that I would prefer to return to my studies and had the nerve to apply for discharge on the grounds that I was a scientist, a profession in good repute at the end of the war. Much to everyone's surprise my application was approved and I returned to Brown, where much had changed during my brief absence. All the old European professors were gone, Prager had been put in charge of the new Division of Applied Mathematics, and a new group of applied mathematicians had replaced the émigrés.

I spent some months reading in probability and statistics. In particular Wald's papers on sequential analysis, a topic classified secret during the war, was of special interest.

During the summer of 1946 there was a six-week meeting in Raleigh, North Carolina, to open up the Research Triangle. Courses were offered by R.A. Fisher, J. Wolfowitz, and W. Cochran. Many prominent statisticians attended the meeting, and I had a chance to meet some of them and young students interested in statistics, and to attend the courses. Wolfowitz taught sequential analysis, Cochran taught sampling, and R.A. Fisher taught something.

Hotelling had moved to North Carolina because Columbia University had refused to start a Statistics Department. Columbia realized that they had made a mistake, and started a department with Wald as Chair and funds to attract visiting professors and faculty. Wolfowitz, who had gone to North Carolina, returned to Columbia. I returned to Brown to prepare for my preliminary exams. Since Brown no longer had any statisticians, I asked Wald to permit me to attend Columbia to write my dissertation in absentia under his direction. He insisted that I take some courses in statistics. In January 1947, I attended Columbia and took courses from T.W. Anderson, Wolfowitz, J.L. Doob, R.C. Bose and Wald.

My contact with Anderson led to a connection with the Cowles Commission for Research in Economics at the University of Chicago, where I was charged with investigating the possible use of computing machines for the extensive calculations that had to be done with their techniques for characterizing the economy. Those calculations were being done on electric calculating machines by human "computers" who had to spend hours carrying 10 digits inverting matrices of order as much as 12. Herman Rubin, who had received his PhD working under T. Koopmans at Cowles and was spending a postdoctoral year at the Institute for Advanced Study at Princeton, often came up to New York to help me wrestle with the clumsy IBM machines at the Watson

Laboratories of IBM, then located near Columbia. At the time, the engineers at Watson were working on developing a modern computer.

3.2 Postdoc at University of Chicago

I completed my dissertation on an approach of Wald to an asymptotic approximation to the (nonexistent) solution of the Behrens–Fisher problem, and in May, 1948, I went to Chicago for a postdoc appointment at Cowles with my new bride, Judith Ullman. I was in charge of computing. Among my new colleagues were Kenneth Arrow, a former CCNY mathematics major, who was regarded as a brilliant economist, and Herman Rubin and several graduate students in economics, one of whom, Don Patinkin, went to Israel where he introduced the ideas of the Cowles Commission and later became President of the Hebrew University.

Arrow had not yet written a dissertation, but was invited to visit Rand Corporation that summer and returned with two outstanding accomplishments. One was the basis for his thesis and Nobel Prize, a proof that there was no sensible way a group could derive a preference ordering among alternatives from the preference orderings of the individual members of the group. The other was a proof of the optimum character of the sequential probability ratio test for deciding between two alternatives. The latter proof, with D. Blackwell and A. Girshick (Arrow et al., 1949), was derived after Wald presented a proof which had some measure theoretic problems. The backward induction proof of ABG was the basis for the development of a large literature on dynamic programming. The basic idea of Wald and Wolfowitz, which was essential to the proof, had been to use a clever Bayesian argument.

I had always been interested in the philosophical issues in statistics, and Jimmie Savage claimed to have resolved one. Wald had proposed the minimax criterion for deciding how to select one among the many "admissible" strategies. Some students at Columbia had wondered why Wald was so tentative in proposing this criterion. The criterion made a good deal of sense in dealing with two-person zero-sum games, but the rationalization seemed weak for games against nature. In fact, a naive use of this criterion would suggest suicide if there was a possibility of a horrible death otherwise. Savage pointed out that in all the examples Wald used, his loss was not an absolute loss, but a regret for not doing the best possible under the actual state of nature. He proposed that minimax regret would resolve the problem. At first I bought his claim, but later discovered a simple example where minimax regret had a similar problem to that of minimax expected loss. For another example the criterion led to selecting the strategy A, but if B was forbidden, it led to C and not A. This was one of the characteristics forbidden in Arrow's thesis.

Savage tried to defend his method, but soon gave in with the remark that perhaps we should examine the work of de Finetti on the Bayesian approach to inference. He later became a sort of high priest in the ensuing controversy between the Bayesians and the misnamed frequentists. I posed a list of properties that an objective scientist should require of a criterion for decision theory problems. There was no criterion satisfying that list in a problem with a finite number of states of nature, unless we canceled one of the requirements. In that case the only criterion was one of all states being equally likely. To me that meant that there could be no objective way of doing science. I held back publishing those results for a few years hoping that time would resolve the issue (Chernoff, 1954).

In the controversy, I remained a frequentist. My main objection to Bayesian philosophy and practice was based on the choice of the prior probability. In principle, it should come from the initial belief. Does that come from birth? If we use instead a non-informative prior, the choice of one may carry hidden assumptions in complicated problems. Besides, the necessary calculation was very forbidding at that time. The fact that randomized strategies are not needed for Bayes procedures is disconcerting, considering the important role of random sampling. On the other hand, frequentist criteria lead to the contradiction of the reasonable criteria of rationality demanded by the derivation of Bayesian theory, and thus statisticians have to be very careful about the use of frequentist methods.

In recent years, my reasoning has been that one does not understand a problem unless it can be stated in terms of a Bayesian decision problem. If one does not understand the problem, the attempts to solve it are like shooting in the dark. If one understands the problem, it is not necessary to attack it using Bayesian analysis. My thoughts on inference have not grown much since then in spite of my initial attraction to statistics that came from the philosophical impact of Neyman–Pearson and decision theory.

One slightly amusing correspondence with de Finetti came from a problem from the principal of a local school that had been teaching third graders Spanish. He brought me some data on a multiple choice exam given to the children to evaluate how successful the teaching had been. It was clear from the results that many of the children were guessing on some of the questions. A traditional way to compensate for guessing is to subtract a penalty for each wrong answer. But when the students are required to make a choice, this method simply applies a linear transformation to the score and does not provide any more information than the number of correct answers. I proposed a method (Chernoff, 1962) which turned out to be an early application of empirical Bayes. For each question, the proportion of correct answers in the class provides an estimate of how many guessed and what proportion of the correct answers were guesses. The appropriate reward for a correct answer should take this estimate into account. Students who hear of this approach are usually shocked because if they are smart, they will suffer if they are in a class with students who are not bright.

Bruno de Finetti heard of this method and he wrote to me suggesting that the student should be encouraged to state their probability for each of the possible choices. The appropriate score should be a simple function of the probability distribution and the correct answer. An appropriate function would encourage students to reply with their actual distribution rather than attempt to bluff. I responded that it would be difficult to get third graders to list probabilities. He answered that we should give the students five gold stars and let them distribute the stars among the possible answers.

3.3 University of Illinois and Stanford

In 1949, Arrow and Rubin went to Stanford, and I went to the Mathematics Department of the University of Illinois at Urbana. During my second year at Urbana, I received a call from Arrow suggesting that I visit the young Statistics Department at Stanford for the summer and the first quarter of 1951. That offer was attractive because I had spent the previous summer, supplementing my $4,500 annual salary with a stint at the Operations Research Office of Johns Hopkins located at Fort Lesley J. McNair in Washington, DC. I had enjoyed the visit there, and learned about the Liapounoff theorem about the (convex) range of a vector measure, a powerful theorem that I had occasion to make use of and generalize slightly (Chernoff, 1951). I needed some summer salary. The opportunity to visit Stanford with my child and pregnant wife was attractive.

The head of the department was A. Bowker, a protégé of the provost F. Terman. Terman was a radio engineer, returned from working on radar in Cambridge, MA during the war, where he had learned about the advantages of having contracts with US Government agencies and had planned to exploit such opportunities. Essentially, he was the father of Silicon Valley. The Statistics Department had an applied contract with the Office of Naval Research (ONR) and I discovered, shortly after arriving, that as part of the contract, the personnel of the department supported by that contract were expected to engage in research with relevance to the ONR and to address problems posed to them on annual visits by scientists from the NSA. We distributed the problems posed in mathematical form without much background. I was given the problem of how best to decide between two alternative distributions of a random variable X when the test statistic must be a sum of integers Y with $1 \leq Y \leq k$ for some specified value of k and Y must be some unspecified function of X. It was clear that the problem involves partitioning the space of X into k subsets and applying the likelihood ratio. The Liapounoff theorem was relevant and the Central Limit Theorem gave error probabilities to use to select the best procedure.

In working on an artificial example, I discovered that I was using the Central Limit Theorem for large deviations where it did not apply. This led me to derive the asymptotic upper and lower bounds that were needed for the tail probabilities. Rubin claimed he could get these bounds with much less work and I challenged him. He produced a rather simple argument, using the Markov inequality, for the upper bound. Since that seemed to be a minor lemma in the ensuing paper I published (Chernoff, 1952), I neglected to give him credit. I now consider it a serious error in judgment, especially because his result is stronger, for the upper bound, than the asymptotic result I had derived.

I should mention that Cramér (1938) had derived much more elegant and general results on large deviations. I discovered this after I derived my results. However, Cramér did require a condition that was not satisfied by the integer-valued random variables in my problem. Shannon had published a paper using the Central Limit Theorem as an approximation for large deviations and had been criticized for that. My paper permitted him to modify his results and led to a great deal of publicity in the computer science literature for the so-called Chernoff bound which was really Rubin's result.

A second vaguely stated problem was misinterpreted by Girshick and myself. I interpreted it as follows: There exists a class of experiments, the data from which depend on two parameters, one of which is to be estimated. Independent observations with repetitions may be made on some of these experiments. The Fisher information matrix is additive and we wish to minimize the asymptotic variance, or equivalently the upper left corner of the inverse of the sum of the informations. We may as well minimize the same element of the inverse of the average of the informations. But this average lies in the convex set generated by the individual informations of the available experiments. Since each information matrix has three distinct elements, we have the problem of minimizing a function on a convex set in three dimensions. It is immediately obvious that we need at most four of the original available experiments to match the information for any design. By monotonicity it is also obvious that the optimum corresponds to a point on the boundary, and we need at most three of the experiments, and a more complicated argument shows that a mixture of at most two of the experiments will provide an asymptotically optimal experiment. This result (Chernoff, 1953) easily generalizes to the case of estimating a function of r of the k parameters involved in the available experiments.

The lively environment at Stanford persuaded me to accept a position there and I returned to settle in during the next academic year. Up to then I had regarded myself as a "theoretical statistical gun for hire" with no long-term special field to explore. But both of the problems described above have optimal design implications. I also felt that the nature of scientific study was to use experiments to learn about issues so that better experiments could be performed until a final decision was to be made. This led me to have sequential design of experiments as a major background goal.

At Stanford, I worked on many research projects which involved optimization and asymptotic results. Many seemed to come easily with the use of Taylor's theorem, the Central Limit Theorem and the Mann–Wald results. A more difficult case was in the theorem of Chernoff and Savage (1958) where we established the Hodges–Lehmann conjecture about the efficiency of the nonparametric normal scores test. I knew very little about nonparametrics, but when Richard Savage and M. Dwass mentioned the conjecture, I thought that the variational argument would not be difficult, and it was easy. What surprised me was that the asymptotic normality, when the hypothesis of the equality of the two distributions is false, had not been established. Our argument approximating the relevant cumulative distribution function by a Gaussian process was tedious but successful. The result apparently opened up a side industry in nonparametric research which was a surprise to Jimmie Savage, the older brother of Richard.

One side issue is the relevance of optimality and asymptotic results. In real problems the asymptotic result may be a poor approximation to what is needed. But, especially in complicated cases, it provides a guide for tabulating finite-sample results in a reasonable way with a minimum of relevant variables. Also, for technical reasons optimality methods are not always available, but what is optimal can reveal how much is lost by using practical methods and when one should search for substantially better ones, and often how to do so.

Around 1958, I proved that for the case of a finite number of states of nature and a finite number of experiments, an asymptotically optimal sequential design consists of solving a game where the payoff for the statistician using the experiment e against nature using θ is $I(\hat{\theta}, \theta, e)$ and I is the Kullback–Leibler information, assuming the current estimate $\hat{\theta}$ is the true value of the unknown state (Chernoff, 1959). This result was generalized to infinitely many experiments and states by Bessler (1960) and Albert (1961) but Albert's result required that the states corresponding to different terminal decisions be separated.

This raised the simpler non-design problem of how to handle the test that the mean of a Normal distribution with known variance is positive or negative. Until then the closest approach to this had been to treat the case of three states of nature $a, 0, -a$ for the means and to minimize the expected sample size for 0 when the error probabilities for the other states were given. This appeared to me to be an incorrect statement of the relevant decision problem which I asked G. Schwarz to attack. There the cost was a loss for the wrong decision and a cost per observation (no loss when the mean is 0). Although the techniques in my paper would work, Schwarz (1962) did a beautiful job using a Bayesian approach. But the problem where the mean could vary over the entire real line was still not done.

I devoted much of the next three years to dealing with the non-design problem of sequentially testing whether the mean of a Normal distribution with known variance is positive or negative. On the assumption that the payoff for each decision is a smooth function of the mean μ, it seems natural to

measure the loss as the difference which must be proportional to $|\mu|$ in the neighborhood of 0. To facilitate analysis, this problem was posed in terms of the drift of a Wiener process, and using Bayesian analysis, was reduced to a free boundary problem involving the heat equation. The two dimensions are Y, the current posterior estimate of the mean, and t, the precision of the estimate. Starting at (t_0, Y_0), determined by the prior distribution, Y moves like a standard Wiener process as sampling continues and the optimal sequential procedure is to stop when Y leaves the region determined by the symmetric boundary.

The research resulted in four papers; see Chernoff (1961), Breakwell and Chernoff (1964), Chernoff (1965a), and Chernoff (1965b). The first was preliminary with some minor results and bounds and conjectures about the boundary near $t = 0$ and large t. Before I went off on sabbatical in London and Rome, J.V. Breakwell, an engineer at Lockheed, agreed to collaborate on an approach to large t and I planned to concentrate on small t. In London I finally made a breakthrough and gave a presentation at Cambridge where I met J. Bather, a graduate student who had been working on the same problem. He had just developed a clever method for obtaining inner and outer bounds on the boundary.

Breakwell had used hypergeometric functions to get good asymptotic approximations for large t, but was unhappy because the calculations based on the discrete time problem seemed to indicate that his approximations were poor. His letter to that effect arrived just as I had derived the corrections relating the continuous time and discrete time problems, and these corrections indicated that the apparently poor approximations were in fact excellent.

Bather had impressed me so that I invited him to visit Stanford for a postdoc period. Let me digress briefly to mention that one of the most valuable functions of the department was to use the contracts to support excellent postdocs who could do research without teaching responsibilities and appreciate courses by Stein and Rubin that were often too difficult for many of our own students.

Breakwell introduced Bather and me to the midcourse correction problem for sending a rocket to the moon. The instruments measure the estimated miss distance continuously, and corrections early are cheap but depend on poor estimates, while corrections later involve good estimates but are expensive in the use of fuel. We found that our methods for the sequential problem work in this problem, yielding a region where nothing is done. But when the estimated miss distance leaves that region, fuel must be used to return. Shortly after we derived our results (Bather and Chernoff, 1967), a rocket was sent to the moon and about half way there, a correction was made and it went to the desired spot. The instrumentation was so excellent (and expensive) that our refined method was unnecessary. Bather declined to stay at Stanford as Assistant Professor and returned with his family to England to teach at Suffolk University. Later I summarized many of these results in a SIAM monograph on sequential analysis and optimal design (Chernoff, 1972).

A trip to a modern factory in Italy during my sabbatical gave me the impression that automation still had far to go, and the study of pattern recognition and cluster analysis could be useful. There are many methods available for clustering, but it seemed that an appropriate method should depend on the nature of the data. This raised the problem of how to observe multidimensional data. It occurred to me that presenting each n-dimensional data point by a cartoon of a face, where each of the components of the data point controlled a feature of the face, might be effective in some cases. A presentation of this idea with a couple of examples was received enthusiastically by the audience, many of whom went home and wrote their own version of what are popularly called "Chernoff faces." This took place at a time when the computer was just about ready to handle the technology, and I am reasonably sure that if I had not done it, someone else would soon have thought of the idea. Apparently I was lucky in having thought of using caricatures of faces, because faces are processed in the brain differently than other visual objects and caricatures have a larger impact than real faces; see Chernoff (1973).

3.4 MIT and Harvard

At the age of 50, I decided to leave Stanford and start a statistics program at MIT in the Applied Mathematics Section of the Mathematics Department. For several years, we had a vital but small group, but the School of Science was not a healthy place for recognizing and promoting excellent applied statisticians, and so I retired from MIT to accept a position at Harvard University, from which I retired in 1997, but where I have an office that I visit regularly even though they don't pay me.

I am currently involved in a collaboration with Professor Shaw-Hwa Lo at Columbia University, who was inspired by a seminar course I offered at Harvard on statistical issues in molecular biology. We have been working on variable selection methods for large data sets with applications to biology and medicine; see Chernoff (2009).

In review, I feel that I lacked some of the abilities that are important for an applied statistician who has to handle problems on a daily basis. I lacked the library of rough and ready techniques to produce usable results. However, I found that dealing with real applied problems, no matter how unimportant, without this library, required serious consideration of the issues and was often a source of theoretical insight and innovation.

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