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WILLIAM GEMMELL COCHRAN

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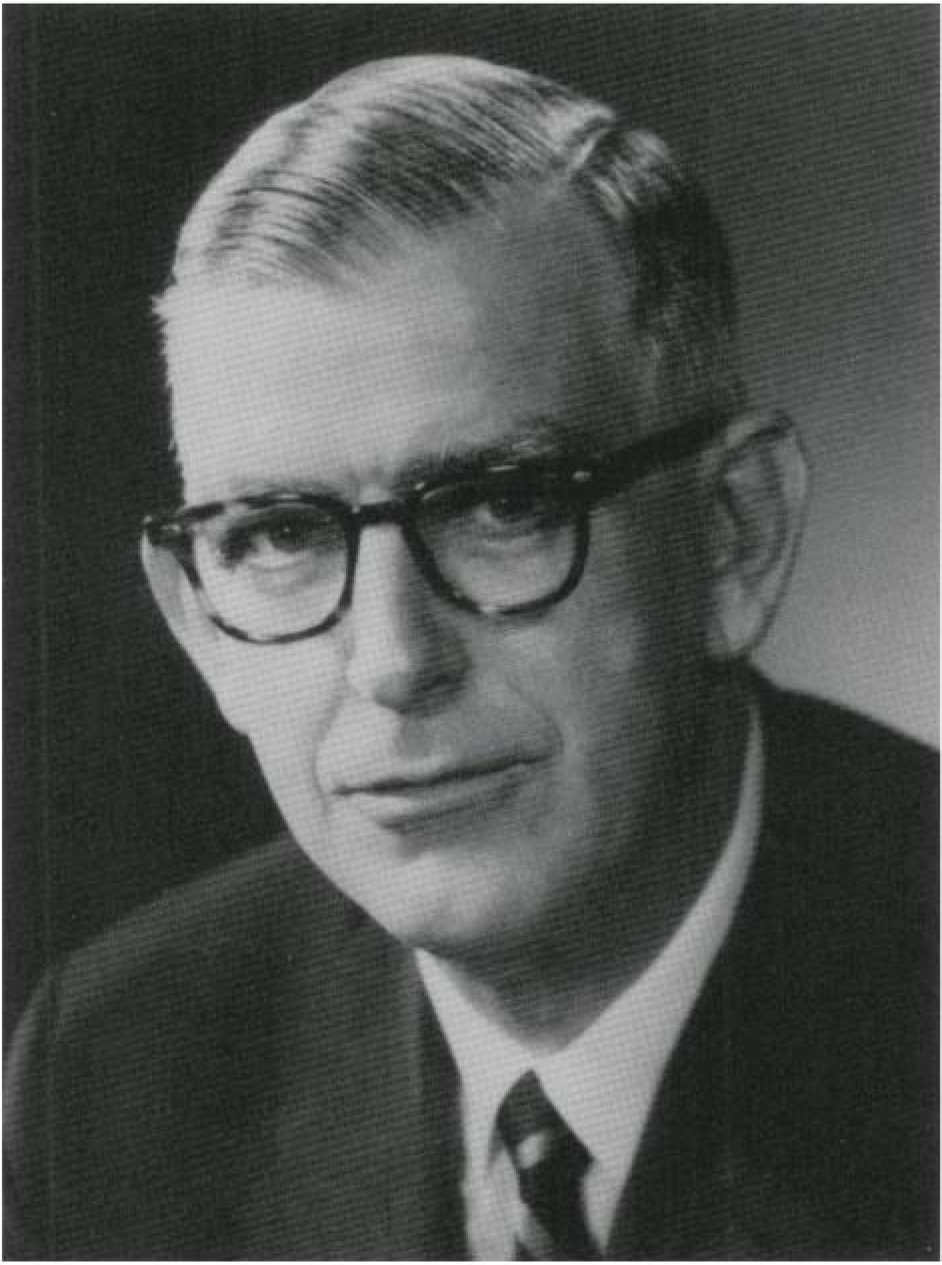
A Biographical Memoir by
MORRIS HANSEN AND FREDERICK MOSTELLER



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July 15, 1909–March 29, 1980

BY MORRIS HANSEN AND FREDERICK MOSTELLER

WILLIAM GEMMELL COCHRAN was born into modest circumstances on July 15, 1909, in Rutherglen, Scotland. His father, Thomas, the eldest of seven children, had begun his lifetime employment with the railroad at the age of thirteen. The family, consisting of Thomas, his wife Jeannie, and sons Oliver and William, moved to Gourock, a holiday resort town on the Firth of Clyde, when William was six, and to Glasgow ten years later.

Oliver has colorful recollections of their childhood. At age five, Willie (pronounced Wully), as he was known to family and friends, was hospitalized for a burst appendix, and his life hung in the balance for a day. But soon he was home, wearying his family with snatches of German taught him by a German patient in his nursing-home ward. Willie had a knack for hearing or reading something and remembering it. Oliver recalls that throughout his life, Willie would walk or sit around reciting poems, speeches, advertisements, music hall songs, and in later life oratorios and choral works he was learning.

Until Willie was sixteen, the family lived in an apartment known in Scotland as a "two room and kitchen"—a parlor-cum-dining room (used on posh occasions, about twelve times a year), a bedroom used by the parents, and a kitchen.

In the kitchen food was prepared, cooked, served, and eaten; dishes were washed, laundry done, friends entertained, and homework accomplished. It was also the boys' bedroom in the form of an alcove-with-bed known as "the-hole-in-the-wall." The boys had a happy childhood, with mile-long walks to and from public school twice a day (lunch was eaten at home) and play at the oceanside.

Willie was a great achiever in school, usually coming in first. Oliver feels he had an irresistible urge to be first, often calculating closely just how much he would have to do to gain that end. Oliver recalls being worried about passing a professional exam and having Willie say to him: "I don't know what on earth you're worrying about; you only have to pass, I have to be first." He was referring to the Bursary Competition, open to all scholars in Scotland. And he was first, winning his fees to Glasgow University. Later he was in an even larger competition for the George A. Clark Scholarship, which provided support for four years and paid his Cambridge fees. Without winning these competitions, he almost assuredly would not have been able to attend either Glasgow or Cambridge.

Willie had no absorbing hobbies as a boy, although he dabbled in many things. Cycling, hiking, and walking in the hills were his chief physical activities. Later, studying and reading became primary. His scholastic prowess won him many books as prizes and created an extensive home library.

Cochran graduated with the M.A. from Glasgow in 1931 with first class honors in mathematics and natural philosophy (physics) and shared the Logan Medal for the most distinguished graduate in the Arts Faculty. That same year he entered St. John's College, Cambridge, and studied for the mathematics tripos (mathematics major) as a prelude to becoming a research student. As an elective, he chose a new course, Mathematical Statistics, taught by John Wishart. A fellow student believes that the Great Depression had interested him in the work of a Dr. Mess, who advocated thorough mathematical investigation of economic problems. He was doubtless also influenced by R. A. Fisher's work at this time. By now he had dropped the use of Willie, and among his colleagues he was known as Bill.

Bill was persuaded by Frank Yates to leave Cambridge without his doctorate to accept a position, a rare opportunity in the depression year of 1934, to do practical research at Rothamsted Experimental Station. Cochran never did receive an earned doctorate, although he received honorary degrees from The University of Glasgow (1970) and The Johns Hopkins University (1975).

During his six years at Rothamsted, Cochran pioneered with Yates in developing techniques for analyzing replicated and long-term agricultural experiments and for assessing the effects of weather patterns on crop yields. They also studied selection effects in non-random sampling.

At Rothamsted, Cochran gained a great deal of practical experience and became well known in his field. In 1937 he married Betty I. M. Mitchell, a plant pathologist.

After visiting Iowa State College (now University) in Ames in 1938, Cochran agreed to return there the following year to teach. The imminence of war in 1939 made him hesitate to leave Europe, but he felt he must keep his word. Under George Snedecor, in 1939 Iowa State was a center for statistical treatment of experimental work—at a time when modern applied statistics had little foothold in America. The emphasis in applied statistics at Iowa was then on sample surveys and experimental design. Cochran lectured on both topics in his first quarter, and these lecture notes matured over the next ten years into his two well-known texts on these topics.

Two of Cochran's three children were born in Ames, Elizabeth in 1940 and Alexander Charles in 1942.

In 1943–44 Cochran took leave to join the Princeton Statistical Research Group at Princeton University as a research mathematician. He was to work on Army–Navy research problems, including naval warfare and a survey of bomb efficiency, for the Office of Scientific Research and Development.

At Iowa State, Cochran and Gertrude Cox initiated their collaboration, which culminated in their book *Experimental Design*, published in 1950. In 1946, at Cox's instigation, Cochran left Iowa to organize and head the graduate program in experimental statistics at North Carolina State College at Raleigh. Cox envisioned this program as half of the Institute of Statistics, the second part consisting of a Department of Mathematical Statistics at the University of North Carolina at Chapel Hill, headed by Harold Hotelling. The Cochran's third child, Theresa, was born in North Carolina in 1946.

In January 1949 the Cochrans moved to Baltimore, where Bill became head of the Department of Biostatistics in the School of Hygiene and Public Health at The Johns Hopkins University. Here his interest in medical and health problems increased. Bill published a second book, *Sampling Techniques* (1953). His two books—along with his 1967 revision, at Snedecor's request, of Snedecor's *Statistical Methods*—became important reference texts and were widely translated. *Statistical Methods* is one of the most widely cited books in the scientific literature.

In 1957 the Department of Statistics was organized at Harvard University, and Cochran joined the staff, remaining nineteen years until he became professor emeritus in 1976. During his time at Harvard, his continued interest in biostatistics was reflected in his interaction with the Department of Biostatistics in the Harvard School of Public Health.

COCHRAN'S WORK

In discussing Cochran's scientific work, we open with his most famous theorem, and follow with selections of his work on the design and analysis of comparative investigations, with both experiments and comparative observational studies. After an overview of his work on counted data, we present some of his contributions to the theory and practice of sample surveys, followed by brief mention of other areas of work. With a few exceptions, we emphasize his advice and philosophy rather than the details of his technical work.

Cochran's first paper (1934), a mixture of algebra and analysis, brought into mathematical statistics an extremely valuable and widely used result, now called Cochran's Theorem:[1] Let X_j, $j = 1,2, \ldots ,p$, be independent standard normal random variables with sum of squares Q. Let Q be decomposed into the sum of k quadratic forms Q_i, where Q_i has rank r_i, $i = 1,2, \ldots ,k$. Then if one of the following three conditions holds, so do the other two: (a) $\sum_{i=1}^{k} r_i = p$, (b) each Q_i has a chi-squared distribution, and (c) each Q_i is independent of every other.

Cochran (1934) himself exploited this result to show that analysis of variance can be extended to a variety of situations requiring adjustment for covariates.

DESIGN AND ANALYSIS OF COMPARATIVE INVESTIGATIONS

Agriculture. Over the years, sets of Cochran's papers focused on methods of value to many applied areas, including agriculture and biomedical research. At Rothamsted he ex-

[1] The form cited is suggested by Maurice G. Kendall and Alan Stuart, *The Advanced Theory of Statistics*, 2d ed., vol. 1 (New York: Hafner Publishing Company, 1963), 360–61.

posited new developments in lattice designs, attributing the general method to Frank Yates. These designs help breeders of wheat, soybeans, corn, and small grains by permitting comparisons of large numbers of varieties (squares being preferable, such as 49, 64, 81, 100, . . .). He compares the performance of these designs with that of others (1941a, 1941b, 1943b).

Along with the descriptions of the methods and their strengths and weaknesses, Cochran continually emphasized the computational effort required in the analysis and the importance of being able to communicate the ideas to the investigator. Why should the half-day or day of calculation required for the analysis be of much concern when an agricultural investigation has already required considerable land for much of a season and several workers to carry it out? Perhaps Cochran realized that a computation that took him half a day might leave a breeder helpless. He was therefore eager to reassure the breeder of its feasibility. Indeed, he said (1941a, p. 355), "Extra complication in the statistical analysis may be a drawback to the widespread use of a design in other respects. If the experimenter does not clearly understand the assumptions involved in the statistical manipulations, or the reasons for them, he loses confidence in the final results of the calculations."

In several papers, Cochran gave substantial reviews intended to guide experimentation in specialized subject matters. For example, just before leaving the United Kingdom for the U.S.A., he presented a major review paper (1939a) on the design and analysis of long-term agricultural experiments that won plaudits during discussion from Sir John Russell, R. A. Fisher, J. Wishart, F. Yates, M. S. Bartlett, M. G. Kendall, and H. O. Hartley. Cochran dealt not only with formal design and analysis considerations but also with important features of the practical execution of these trials in the field: size and shape of plots, numbers of replications, choices of stratification or blocks, headlands and guard rows between plots, and the value of a year or two of a uniformity trial prior to a long-term field experiment, especially for a new crop. And he warned the statistician, "It is not sufficient for him [the statistician] to provide the best possible design to suit the size of the experiment; it is also his duty to advise whether he thinks the experiment as designed is worth doing, or whether it should be postponed until more resources are available" (1939a, p. 106).

With Gertrude Cox (1946a), he summarized the principal sources of variation in greenhouse experimentation (temperature, moisture, and shading gradients) and major designs that could control for such sometimes nearly overwhelming variables. Curiously, in 1946 they reported that they had no information about the possible benefits of moving pots around, although this is one advantage of the greenhouse over field conditions.

His article in the *International Encyclopedia of Statistics* (1978b) on experimental design contains an instructive postscript on the rise of the use of experiments in the social sciences and the encouragement given to this movement by the Social Science Research Council. That postscript relates more generally to his study (1976) of the history of experimentation. After introducing us to Arthur Young's total intolerance for any method but comparative experiments, Cochran notes (1976, p. 5), "This issue persists today. In reviewing the present state of knowledge about the relative merits of two therapies for hospitalized patients, we may find a few well-controlled experiments and a larger number of doctor's observations on their experiences with one or the other therapy. Young would seem to suggest that to consider the latter group is a waste of time."

Cochran used the history article to include a little instruction on experimental design, as well as to get in a few licks about some consulting problems he had suffered. He suggested that most consulting statisticians will have had experience with an investigator who begins "'I want to do an experiment to show that. . . .' He knows the answer." Cochran used this remark as a springboard to discuss double-blind experiments. In a similar aside, Cochran used James Johnston's book on agriculture[2] to make an additional point. After describing Johnston's position that a bad investigation wastes money and leads to incorrect results in standard textbooks, as well as to the neglect of further research, Cochran said (1976, p. 9), "I have heard this point made recently with regard to medical experiments on seriously ill patients, where there is often a question for the doctor if it is ethical to conduct an experiment, but from the broader view-point [it is] a question of whether it is ethical *not* to conduct experiments."

Cochran used history to console the young scholar. Upon recalling that after Student's *t* tables had been available for fourteen years and practically no one used them, he said, "Young research workers who feel that the world is very slow to appreciate their results might be heartened by this example. The world is indeed a little slow at times to realize how brilliant we are" (1976, pp. 13–14). He sums up the history of statistics in agriculture by saying that it took a century to take two major steps: (1) to begin applying probability theory (already available in astronomy) to interpret quantitative experiments and (2) to establish efficient practical methods for the conduct of field experiments.

Bioassay. A sequence of papers (three with Miles Davis: 1963 1964, 1965a; and 1973) reported on Cochran and Dav-

[2] J. F. W. Johnston, *Experimental Agriculture, Being the Results of Past and Suggestions for Future Experiments in Scientific and Practical Agriculture* (Edinburgh: W. Blackwood and Sons, 1849).

is's studies of bioassay, where the investigator wants to find the LD50, the dosage that kills 50 percent of the animals or insects. They studied sequential approaches using a grid of dosages. Animals are tested at an initial dose, and the outcome at that dose guides the choice of the next dose—up or down. In one version, if the first dose kills, the second dose is one step smaller; if it does not, the next animal gets a dose one step higher. This process continues. They recommended a two-stage approach. The first stage uses few animals with large steps until it locates a reversal, and the second stage uses the Robbins-Munro method with smaller steps.

Clinical Trials. His papers on the design of clinical trials (1961a, 1977b) had a rather general nature. In the first (1961a), he emphasized heavily the value of precise protocol, power, blindness, randomization, and design. The biostatistician of the 1980s—with special survival analyses, sequential designs, and balancing approaches—might be surprised, even affronted, to read (1961a, p. 71): "The planning and conduct of a clinical trial does not involve any difficult or esoteric intellectual principles. It is mainly a matter of hard work and attention to detail."

The second paper (1977b) was a group effort focused on surgical experiments in duodenal ulcer. Although Cochran had suffered a substantial illness, he was essentially recovered, but he did not want to take on any extra tasks. Consequently he refused to take part in a working group in the Faculty Seminar on Health and Medicine at the Harvard School of Public Health. But students and friends pleaded with him to change his mind, and in the end he chaired the Working Group on Protocol Issues. After two years of discussions in depth of the principal experiments in surgery for duodenal ulcer, the group produced a comprehensive list of medical and statistical criteria for consideration in further experiments. Most of the criteria have value for design, analysis, and reporting of comparative medical investigations generally, not just for surgery for duodenal ulcer. Again, care and precision in protocol were emphasized. The lists cannot be reproduced here, but a remark on follow-up to obtain information on nearly 100 percent of patients treated is worth quoting (1977b, p. 191): "A search produced few references to available techniques for guarding against follow-up losses. There seems to be no substitute for determination." They reported that at the Mayo Clinic high rates of follow-up have been "achieved by writing letters directly to patients and not going through their doctors; if no reply is forthcoming, the telephone is used. If the patient is not found, a vigorous search is undertaken, including use of bill-collecting agencies, who apparently have experience with similar problems" (1977b, p. 191).

Observational Studies. Program arrangers often asked Cochran to provide a substantial general paper on the conduct of comparative studies intended to decide causation. In discussing the advantages of matching subjects or materials as compared with the use of covariance adjustment in observational studies, he first noted that the methods perform almost equally well. "A difficulty which I have occasionally encountered with covariance is that some scientists have an inborn suspicion of adjustments to the data, and although the adjustments made in the covariance analysis are entirely objective, they may find a rather grudging acceptance" (1953, p. 687). (Although Cochran correctly stated that, given least squares, the adjustment itself is objective, the decision to make it usually is not; when many covariables are available, many subsets can be selected. The suspicious scientist has a right to some skepticism because an investigator could adjust for the subset that gave results most pleasing to him or her. Nevertheless, when the covariables for adjustment are chosen in advance of the investigation, the method is objective.)

Possibly his *Journal of the Royal Statistical Society, Series A* paper on observational studies (1965b), followed by papers in 1967 and 1972, formed the basis for his program to prepare a book on the planning and analysis of comparative observational studies. At his death, he left six and one-half of seven planned chapters completed. Moses and Mosteller edited it for posthumous publication (Cochran, 1983). The 1965b paper itself offers a substantial introduction to such investigations. Some quotations may be appropriate. The opening reminds us of Harold Dorn's[3] dictum to ask "How would the study be conducted if it were possible to do it by controlled experimentation?" (1965b, p. 236). In reviewing the dangers of loading a study with so many research questions that it may fall of its own weight, he confessed, "But when dealing with an imaginative investigator I do not find it easy to determine at what point one should adamantly oppose all further questions, however ingenious and interesting" (1965b, p. 240). In before-after studies—of health, for example—some investigators note that the initial questionnaire may alert participants to behavior they should beware of, and thus bias the study. Cochran said (1965b, p. 249), "My own view is that an educational programme that cannot improve health practices more than can a single questionnaire is not wrongly considered a failure. . . ."

When faced with a collection of studies yielding contradictory results, the applied scientist "cannot avoid an attempt to weigh the evidence for and against, since some results are so vulnerable to bias that they should be given low weight. . . . He should state such judgements forthrightly, remembering his duty to maintain even standards and, if possible, an air of calm detachment" (1965b, pp. 253–54). This last remark is a bit of tongue-in-cheek humor; Cochran was about to sug-

[3] H. F. Dorn, "Philosophy of inferences from retrospective studies," *American Journal of Public Health*, 43(1953):677–83.

gest that someone else, while doing a good job, may have sometimes strayed just a bit from even standards.

COUNTED DATA

Among Cochran's several systematic research programs, the analysis of counted data stands out (1936a, 1936c, 1937, 1938a, 1940a, 1942b, 1943a, 1950, 1952, 1954b). Maxwell, in his introduction to the first organized text on counted data, *Analysing Qualitative Data,*[4] said "I am indebted to . . . Professor W. G. Cochran from whose work I borrowed freely" (p. 9).

In studying both the distribution of diseased plants in rows of a field (1936a) and the persistence of one kind of weather (1938a), Cochran had occasion to derive and use the distribution of the number of runs in a binomial sequence where the probability of success on a single trial differed from ½, thus generalizing the work of Marbe and others. He also investigated the power of the sign test (1937).

The problem of chi-squared tests and the correction for continuity (1942b) come up in various ways. How small shall the observed counts in cells be before we abandon the attempt to use chi-squared, or pool cells, or find some corrective device? Repeatedly Cochran returns to this question (1936c, 1942b, 1952, 1954b). In the 1942b paper he gives a special formula and tables for handling the problem, tables still not widely used, we believe. In addition to these, the use of transformations (1940a) and the analysis of variance for data that come as percentages (successes divided by totals; 1943a) and data from matched samples (1950) produced major contributions to the field. The large papers concerning goodness-of-fit tests (1952) and strengthening the common chi-squared tests (1954b) offer a small education in them-

[4] A. E. Maxwell, *Analysing Qualitative Data* (London: Methuen and Company, 1961).

selves. The 1952 paper (p. 324) lists rules for handling chi-squared with small numbers in the cells, and the 1954b paper (p. 420) offers some slight revision of these rules based on further research. Indeed, these ten papers would form a small textbook on the analysis of counted data. The 1954b paper presents a large number of methods for strengthening the chi-squared tests and includes the essentials, together with a derivation in the appendix of the now-popular technique, sometimes called the Mantel-Haenszel method for combining results of several contingency tables.

One difficulty in reading Cochran's papers is that it is hard to know what may be original with him and what he regards as helpful exposition of known results. He often said of statistical research workers, "we all deserve more credit than we get for results others publish, and a little less for those we ourselves publish." His grounds for this remark were that many ideas in statistics float around for a long time before someone actually sets them down in good order and publishes them. Often we cannot nail down just exactly who had the original idea.

The utility of the common chi-squared test for goodness of fit has been much debated, partly because most statisticians including Cochran (1952, p. 336) agree with Joseph Berkson. He argued that, given enough observations, we would be sure to reject the normal distribution (and presumably any other distribution) as a model in any particular situation. (Amusingly enough, when Berkson gathered an enormous body of data to check whether radiation counts followed a Poisson process, theory and data agreed extremely well. On the other hand, Berkson's work on counting blood corpuscles showed that no standard distribution applied.) Cochran pointed out that Karl Pearson was aware of this difficulty, even when he invented the chi-squared test. Cochran struggled to suggest new approaches in these situations. He proposed that perhaps instead of testing a point null hypothesis we should be testing whether a quantity falls into an interval; or that we should consider as the null hypothesis a broader family, near the one being assumed.

SAMPLE SURVEYS

Cochran's initiation into sample-survey theory and practice came when he joined Frank Yates at Rothamsted. R. A. Fisher, with Yates and other colleagues at Rothamsted, had made remarkable advances in the theory of statistics as a tool of applied research in agricultural experiments. Modern theory and methods for sample surveys were substantially advanced by these developments, including the use of randomization in sample selection, already used to some extent in sample surveys.

Cochran's first paper directly related to sample surveys (1936b) demonstrated the importance of randomization in the selection of samples as distinguished from purposive or judgmental selection. Yates had earlier done an experimental demonstration of biases that resulted by allowing a judgmental selection of a "random sample" of plants. At a conference of the observers of the crop-weather scheme (for crop forecasting) in 1935, an experiment was planned to see to what extent the kinds of biases observed by Yates are common to all observers who make deliberate selections. Cochran analyzed the results of the experiment and concluded (1936b, pp. 74–75):

> It is obvious that samples that are picked by a process of randomization which gives every sample in the population an equal chance of being picked, must be representative of the population from which they are drawn and give an unbiased estimate of the quantity which it is desired to measure. Those who have little experience of the technique of sampling might, however, be unwilling to admit that they could not do as well, or better, by choosing the samples themselves. In this experiment, out of

twelve observers, all of whom have had some training in sampling, not one managed to pick a sample that could be called representative of the material from which they were sampling. . . . What is even more serious and striking is that the individual observers were not consistent throughout the experiment; the positive bias in selection increased regularly as the mean height of the sampling-unit decreased.

This work helped establish the importance of randomization in both sample surveys and experiments.

In his work at Rothamsted, Cochran took advantage of the opportunities to be involved in practical studies in design and analysis of experiments and sample surveys. The sample-survey experience included, for example, evaluation of crop-forecasting methods based jointly on sample-survey information on the crop and on weather data (1938c). It also resulted in empirical analyses of survey data to evaluate the efficiency of alternative sample designs for agricultural studies (1938b). As was his usual practice, this paper included a rather exhaustive analysis, including one of the early efforts to balance the amount of work involved against statistical efficiency. He also developed procedures for making approximate advance speculations on sampling variances before results are available for analysis, as is essential in practical work on sample-survey design. In another study (1940b) he evaluated the gains that would result in estimating cereal yields by estimating the ratio of grain to total produce from the sample and applying the ratio to known information on the total produce.

In 1939 he published a paper entitled "The Use of the Analysis of Variance in Enumeration by Sampling," based primarily on his work at Rothamsted, but published after he had moved to the Statistical Laboratory at Iowa State College in 1939. In this pioneering paper he applied the analysis of variance to finite-population sampling by regarding the finite population as a sample from an infinite superpopulation. He conditions on the finite population and obtains estimators appropriate to the finite population that—with minor exceptions—agree exactly with those arrived at by direct application of probability-sampling theory. He illustrated the great convenience and power of the application of the analysis of variance to data available from a particular sample in evaluating the appropriate use of subdivision (now generally referred to as stratification), subsampling, choice of sampling units, and double sampling. He concluded:

> The results of a properly planned sampling investigation, in addition to providing an estimate of the accuracy of the sample, often provide estimates of the accuracy of various alternative methods of sampling which might have been used. These estimates are helpful in increasing the efficiency of sampling in future studies on similar material. . . . The estimate of the relative accuracy of two methods of sampling is shown to be in most cases a simple function of the variance-ratio, so that its sampling limits are easily obtainable. (p. 510)

In 1942 Cochran contributed an especially interesting result for sample-survey applications concerned with "Sampling Theory When the Sampling-Units Are of Unequal Sizes." The procedure is applicable in estimating a population average, $\bar{y}_p$, or total for a variable y where information on a correlated variable, x, is available for the total population and for each unit in the sample. Among others he considered a linear regression estimator of $\bar{y}_p$ of the form $\bar{y}_l = \bar{y}_s + b(\bar{x}_p - \bar{x}_s)$, where $\bar{y}_s$ and $\bar{x}_s$ are the sample means, b is the usual estimate from the sample of the linear regression coefficient, and $\bar{x}_p$ is the known population mean of the x characteristic. It was well known that this estimator is the minimum variance estimator of $\bar{y}_p$ if the population regression of y on x is linear and if the conditional variance of y given x is constant. Cochran, however, showed the exceedingly useful result that $V(\bar{y}_s)(1 - r^2)$, the well-known estimator of the variance of $\bar{y}_l$ for this particular case, is asymptotically valid in large samples for any population; that is, it is a consistent estimator of the variance no matter what the form of the regression of y on x. He considered weighted as well as unweighted regression estimators and compared these and other alternative estimators for varying sampling designs, as well as discussing the conditions under which each estimator is most efficient. As he pointed out, the regression estimator is relatively difficult to compute. While the regression estimator has been extensively used, its applications are limited by the difficulty of computing. In addition, in sample surveys that measure many characteristics the results for multiple characteristics are not additive; that is, an estimate for males plus an estimate for females will not necessarily be equal to the estimate for both sexes combined. Nevertheless, it has proved highly useful in many applications. It has also contributed to understanding the principles of estimation from sample surveys.

Systematic sampling, of which the simplest form is selecting every k^{th} unit from some kind of an ordered sequence, has long had intuitive appeal and has been widely used as a sample-selection procedure. The estimation of summary measures from such a sample, such as means, ratios, or regressions, is straightforward, but theory is not available for making consistent estimates of variances. Often variances are estimated by treating a systematic sample as equivalent to a stratified random sample. Some empirical studies have shown this to provide a reasonable approximation in many circumstances, but far from a satisfactory approximation in others.

In 1944 W. G. and L. H. Madow identified systematic sampling as a special case of cluster sampling, and provided theory and examined its characteristics under some alternative models.[5] Cochran extended these results in a paper entitled "Relative Accuracy of Systematic and Stratified Ran-

[5] William G. and Lillian H. Madow, "On the theory of systematic sampling," *Annals of Mathematical Statistics*, 15(1944):1–24.

dom Samples for a Certain Class of Populations," published in 1946. He observed that numerous studies of real populations had revealed that the variance among the elements in any group of contiguous elements increases steadily as the size of the group increases, and he constructed a model appropriate to such populations. In formulating the model, he regarded the observed finite population as a sample from a superpopulation in which (in what follows, E is the expectation operator):

$$E(x_i) = \mu,\ E(x_i - \mu)^2 = \sigma^2,\ E(x_i - \mu)(x_{i+u} - \mu) = \rho_u \sigma^2,$$

where $\rho_u \geq \rho_v \geq 0$ whenever $u < v$. He obtained average variances for samples from the possible finite populations from such a superpopulation.

For this class of populations he showed that:

> The stratified random sample is always at least as accurate on the average as the random sample and its relative efficiency is a monotone increasing function of the size of the sample. No general result is valid for the relative efficiency of the systematic sample. In fact, there are populations in the class in which the systematic sample is more accurate than the stratified sample for one sampling rate, but is less accurate than the random sample for another sampling rate. If, however, the correlogram is in addition concave upwards, the systematic sample is on the average more accurate than the stratified sample for any size of sample. (1946b, p. 164)

He pointed out that while no unbiased or consistent estimate of the variance of the estimated mean is available from a systematic sample, an unbiased estimator can be obtained if one can properly make an assumption concerning the form of the population being sampled. Its validity will depend, of course, on the validity of the assumed population model.

Cochran published numerous other papers concerned with various aspects of sample surveys as he encountered them in consulting or otherwise became interested in them. For example, in a 1961(b) paper he examined alternative rules for establishing strata boundaries by comparing them empirically for several different forms of populations with varying amounts of skewness. In 1962 he jointly authored, with J. N. K. Rao and H. O. Hartley, a paper that proposed a simple procedure for unequal probability sampling without replacement. This approach had the advantages of simplicity of calculation and the ability to provide unbiased estimates of the variance of the estimators. This was a topic that received considerable attention at the time, and a number of different procedures were proposed by various authors.

The problem of nonsampling errors in surveys is one that has received extensive attention, and in 1968 Cochran prepared a review paper and extended some of the earlier work that had been done in this area. He concluded, as do others, that errors in measurement can sometimes seriously vitiate most standard statistical techniques and at other times have only trivial effects—depending on the size of the relevant response variances and covariances. He added that what seems needed at the present state of development of this area are many studies that permit the estimation of these variances and covariances, and that most of these studies should be embedded in ongoing surveys. "When an 'errors of measurement' study has to be conducted separately, as will sometimes be necessary because of the complexity of such studies, it is always difficult to reproduce the working conditions of an actual survey" (1968, p. 665).

In "Laplace's Ratio Estimator" (1978a), Cochran took an engaging historical tour. He reviewed the well-known estimate made by Laplace in 1802 of the total population of France. Laplace took a sample (by purposive sampling procedures) of communes in France and persuaded the government to have a population census taken in each of these. Births were registered throughout France, and therefore were known for each commune as well as the country as a whole. He then estimated the total population of France with the ratio estimator $\hat{Y} = Xy/x$ where X is the known total registered births, x is registered births for the sample communes, and y is the total population for the sample communes. The estimate was 28.4 million. Laplace then estimated the standard error of this estimate to be 108,000. In computing the estimated sampling error, Laplace assumed that the birth rate in each commune (and of course in all of France) was the consequence of sampling births and population at random with equal probability from the same urn, a finite superpopulation.

Cochran reported: "He found the large-sample distribution of his error of estimate to be approximately normal, with a small bias and a variance that he calculates" (1978a, p. 3). Cochran then points out that in computing the sampling error Laplace failed to recognize that the birth rates in the sample and in all of France were not independent, and states in a summary remark:

> It is unfortunate that Laplace should have made a mistake in probability in a book on the theory of probabilities. In his application, however, the mistake was of little consequence. His working out of the large-sample distribution of the ratio estimator and his concept of the superpopulation as a tool in studying estimates from samples are pioneering achievements. (1978a, p. 10)

Cochran wrote a number of review papers related to sample-survey topics (1938b, 1947, 1951, 1956) that provided lucid summaries of the state of the art at the time the papers were written and gave additional interpretations. Of course his textbook, *Sampling Techniques,* is a substantially comprehensive summary, with extensions of theory to round out topics and with reporting of empirical results to provide better guidance on practical implications of some of the methods. It is undoubtedly the most widely used textbook in teaching sample surveys, as is attested by the printing of second and third editions in 1963 and 1977.

COCHRAN'S OTHER CONTRIBUTIONS TO STATISTICS AND TO SOCIETY

Cochran suggested that statisticians might profitably conduct a survey to find out how scientists use statistical techniques and how they are helped by them. He thinks it "might be very illuminating to statisticians if it could be carried out despite the obvious difficulties. Statisticians are, I think, rather quick to jump to conclusions about the kinds of problems which scientists in other fields are supposed to face, and about their presumed uses and misuses of statistical methods and ideas" (1952, pp. 334–35). Because he was writing in the *Annals of Mathematical Statistics,* he probably felt he was speaking only to the statisticians.

Having illustrated Cochran's propensity for returning to problems repeatedly, we shall not review all the topics where he carried on such a program. Instead we merely mention that these included: (a) the problem of weighting to combine results from several comparable experiments (for example, when the effects in the different experiments did not necessarily have the same true means or precisions and when precisions needed to be estimated); (b) the problems associated with both qualitative and quantitative discriminant functions; (c) the use of covariates in experiments and observational studies; (d) the effect of errors of measurement on regression, analysis of variance, and the analysis of counted data; and (e) special analyses for detecting outliers, for handling missing observations, for adding or removing a variable in regression, or for comparing scales of measurement.

Cochran was an exceptional teacher, beloved by his students. He directed four dissertations at North Carolina, fifteen at Johns Hopkins, and nineteen at Harvard. In addition he greatly influenced a large number of other students. They recall his clarity, wit, willingness to help, and use of practical examples culled from his experience. As one said, Bill "pulled it all together in a way that made it fun to calculate coefficients and to invert matrices. We wanted to do it because Bill would have been disappointed if we failed."

Bill had a great ability to get to the heart of any statistics problem with virtually no time lost. He was succinct and clear in his teaching and writing. He worked with his graduate students to try to make them understand where the problem formulation and inductive statistics ended and the deductive mathematics began. Bill displayed the great knack for linking the theoretical and the applied that Americans associate with statisticians trained in the United Kingdom, and he was able to explain complicated statistical information to investigators in language they could understand. Consequently he was a much sought-after consultant and an excellent committee member or head. His calm fairness and down-to-earth attitude assured attention to dealing with the core problem.

Cochran limited his committee participation to the amount of work he could handle. He chaired the committee appointed by the American Statistical Association at the request of the National Academy of Sciences to review the Kinsey, Pomeroy, and Martin study of sexual behavior in the human male, work that resulted in a book (1954a). He served as chairman of the Panel of Statistical Consultants, U.S. Bureau of the Census. He served on the committee to consider the effect of battery additives on the life of batteries, on the Academy Committee to the Atomic Bomb Casualty Commission, and on the Committee on Epidemiology and Biometry at the National Institutes of Health. The Subcommittee on National Morbidity Survey of the U.S. National Committee on Health Statistics, of which he was a member, submitted a report to the Surgeon General that was the basis, with little change, of the National Health Survey Act. A smoker, Bill was the only statistician on the Surgeon General's Committee on Smoking and Health.

Bill received many honors. He was at various times president of four major statistical organizations: the Institute of Mathematical Statistics in 1946, the American Statistical Association in 1953, the Biometric Society (which he helped found as a member of the organizing committee) in 1954–55, and the International Statistical Institute in 1967–71. He was elected to the American Academy of Arts and Sciences in 1971 and to the National Academy of Sciences in 1974. He was a fellow of the American Association for the Advancement of Science; honorary fellow of the Royal Statistical Society; and Guggenheim fellow, 1964–65. He received the Guy Medal of the Royal Statistical Society in 1936, the S. S. Wilks Memorial Medal (American Statistical Association) in 1967, and the "Outstanding Statistician" Award (Chicago Chapter, American Statistical Association) in 1974. He was editor of the *Journal of the American Statistical Association* from 1945 to 1950.

Personally, Bill was an unpretentious man with Scottish wit and humor. He was a believer in the fellowship of man, and one of the few things sure to elicit his anger was a bigoted comment. Although he preferred to work by himself rather than to collaborate with others, he was friendly to everyone and liked by all. He and his wife Betty, to the delight of colleagues and students, entertained frequently, and enjoyed square dancing, theater, music, and travel. Hundreds of statisticians from far-flung places attended Bill's retirement dinner in 1976.

The last several years of Bill's life were plagued with a series of medical problems. Nonetheless, after his retirement and his move to his Cape Cod home, he continued to travel, to teach, and to write. He died in Orleans, Massachusetts, on March 29, 1980.

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