Carlo estimates of variances can reasonably be trusted to about 3%—a reported value of 1.5 could represent a true value of 1.45 to 1.55. As the authors point out, one can assume slightly greater accuracy for comparisons within a given situation.

The choice of distributions seems to have been strongly influenced by the existence of efficient Monte Carlo techniques. At least one participant in our seminar felt this study had placed too much emphasis on long-tailed situations which had either infinite variance or unrealistically heavy contamination. In particular, justification for redescending ψ-functions came from such distributions, and it is not completely clear that the justification would remain if these long-tailed distributions were truncated moderately far out in the tails.

There was also comment in our seminar on the absence of seriously multivariate exploration of such a rich body of data. One could allocate a dimension for the variance or deficiency in each of a moderate number of situations, regard the estimators as points in this space, and try principal components, Andrews's plots for high-dimensional data, or Chernoff's faces. Rather clearly there are many more possibilities for this sort of exploration than any one group would choose to pursue. For this reason, among others, a computer "export tape" would be a desirable byproduct of any such study. As this is written, it seems likely that a tape for this study, containing such things as variances, pseudovariances (at least 2.5% and 10%), FORTRAN routines, covariances, and perhaps detailed sample configurations and estimator values, will be available in the fall of 1973.

In the space of this review it has not been possible to discuss such parts of the book as the appendices which extend Monte Carlo techniques for contaminated Gaussians. They, like the chapters on analysis of results, deserve much careful study.

While the Princeton Robustness Study considered exclusively the problem of point estimation of location, its results have sparked much research on such robustness problems as interval estimation of location (Gross [1]), estimation of scale and shape parameters (Bickel and Lehmann), and regression (Huber, Mallows).

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Nineteenth-Century Society,

E.A. Wrigley (ed.). New York: Cambridge University Press, 1972. vii+448 pp. $27.50.

The Wages of War 1816–1965: A Statistical Handbook,

J. David Singer and Melvin Small. New York: John Wiley and Sons, Inc., 1972. xii+419 pp. $13.95.

A social scientist who tells his colleagues that he is going to apply statistical methods to life before 1939 or 1945 or 1960 (standards differ) usually gets a glassy-eyed look and the remark, "Yes: that sounds interesting; but the data must be lousy." Political scientists, sociologists, and economists, especially those who think in statistical terms, are doggedly present-minded. Little wonder. When one-in-a-thousand samples from the latest census and elaborate surveys of current public opinion are available for the asking in tapes from authoritative agencies or in clean-looking columns from reference books, why bother to extract numbers from the distant past? And when the rewards to those who covet scientific prestige depend on adding a frill to factor analysis and when the rewards to those who covet social influence depend on beating the journalists to the latest social problem, why bother to use the numbers once extracted?

These two books provide persuasive answers. First, and most obvious, they demonstrate that historical statistics give us more degrees of freedom and that the accuracy of the additional observations is frequently no worse than those collected yesterday. As Singer and Small remark, one can estimate deaths in battle with more confidence for the first war they treat, the Franco-Spanish War of 1823, than for the last, the Indian-Pakistani War of 1965. In the introduction to *Nineteenth-Century Society* Wrigley points out that access to the detailed records of the British census is restricted by a hundred years' rule, making it impossible to do for 1971 what is possible for 1871. The present-minded may retort that the dimensions of life in 1871, not to mention 1823, are irrelevant to present concerns, yet here again the answer is plain. Historical statistics give perspective to the present and impose a discipline on the analysis of the short run. Singer and Small are able to reject, for example, the common assumption that the amount of war has increased markedly in the last century and a half. V.A.C. Gatrell and T.B. Hadden, in their essay on criminal statistics in the Wrigley volume, find that the positive correlation in the 20th century between economic prosperity and crimes against property, on which has been based so much criminological theorizing, is reversed in the 19th. Michael Anderson's essay on the use of census information for the study of the structure of British family life contributes to the on-going demolition of the myth of the pre-industrial extended family. To paraphrase a quotation that Anderson takes as a motto, social scientists have hitherto fabricated their statistical history to lend profile to their assertions about the present. These two books are impressive examples of how measurement can replace fabrication.

This is not to say that either in its field has finished the job of statistical inference. Their goal at present is merely descriptive: in the schedule of Scientific Method that Singer and Small invoke so frequently, both books are at the stage of data-collection. *The Wages of War* collects information on the dependent variable for a projected regression analysis of the causes of war, detailing for 93 international wars involving at least one acknowledged member of the "interstate system" (a definition that excludes, therefore, civil wars and wars between countries too small or too dependent to have an important foreign policy) the duration, participants, and battle deaths. As its subtitle declares, it is a "Statistical Handbook." Nearly two-thirds of the 378 pages of text are taken up by tables and graphs exhibiting these statistics in one or another form, and the accompanying prose brings the reader into the statistical workshop. *Nineteenth-Century Society* is a handbook at an earlier stage. The view into the workshop finds the craftsmen busily assessing their supplies of raw materials—official statistics on the age, occupation, family membership, school attendance, migration, and criminal record of the people of England and Wales from 1801 to 1891—but as yet, as Wringley remarks in the introduction, finds "very few results of research.... Where substantive results are mentioned it is usually to illustrate a point of technique" in the compilation of the statistics.

There is much in both books to provoke thought in the minds of applied statisticians. The statistics of deadly quarrels are inherently fascinating, much like percentage baseball, and the steps to producing such statistics share this quality: in part A, defining the sample, bringing measurement to bear on issues of the importance of nations and the definition of war; in part B, quantifying the blood spilled, revealing such surprising horrors as that the Chaco War between Paraguay and Bolivia, 1932–35, was nearly three times more bloody in terms of battle deaths per million population of the participants than the next contender, World War I; in part C, examining the amount of war underway from 1823 to 1965 and finding or failing to find the trends, cycles, and seasonalities about which observers have speculated for so long; and in part D, reclassifying the facts by nations, and reaching cautious conclusions on warmongers, allies, and the relationship between deaths and victory.

The subject of *Nineteenth-Century Society* has less sex appeal and the numbers collected on the subject are more conventional. Yet it too is good reading for an applied statistician who is curious about the historical frontier of his field. Michael Drake's essay on the history of the decennial census of population is both amusing and solid, exhibiting the birth in the nineteenth century of the public passion for official statistics. The quality of the statistics produced in response is the subject of a long and careful chapter by P.M. Tillott (on the clerical and other errors that crept into the 1851 and 1861 censuses) and their use to construct other statistics is the subject of Anderson's excellent chapter on kinship and cohabitation, and also of chapters by W.A. Armstrong on matching occupations in successive censuses, by Dudley Baines on the inferences that can

be made about migration, by B.I. Coleman on the difficulties faced if one goes outside the census for information on school attendance, and by Gatrell and Hadden on one large body of unexploited data outside the census, the statistics of crime. Together with R.S. Schofield's expository essay on "Sampling in Historical Research" they will give the statistician some feel for which tools of his trade historians find useful.

Both books, then, are Keplerian rather than Newtonian reports from social observatories located in Cambridge, England (the Cambridge Group for the History of Population and Social Structure) and in Ann Arbor, Michigan (a large group of social astronomers with a quantitative orientation). The statistics that the Cambridge group will be able to lay before some future Newton of social theory are very rich: statistics on every person alive in England and Wales during the 19th century, making possible delicate comparisons of groups in differing circumstances. Using a comparison between two otherwise similar towns, for example, Anderson is able to show that mere differences in the availability of housing for young married couples, rather than any difference in the strength of family ties, explains the difference in the extent to which families of successive generations lived together. The Ann Arbor group is less fortunate. Singer and Small, for all their wisdom in choosing a long period to deal with, have only 93 wars, compared with the millions of individuals and thousands of towns that can be measured from the books of the census enumerators in England. The reasons people migrate, take occupations, marry, and turn to crime are no doubt as complex as the reasons nations go to war, but a student of war has fewer cases with which to disentangle the reasons. The study of business cycles, like the study of many similar events, faces difficulties of this kind as well, although economists have the advantage which a student of international politics does not that a believable explanation of business cycles can be built up from tested and logically interconnected propositions about each part of the business world. The difficulty is aggravated by the enormous range in the size of what is called war. Fully 80 percent of the battle deaths in the 93 international wars between 1823 and 1965 occurred in World Wars I and II, and their share of deaths understates their share of impact. Many economists have felt that the Great Depression was different in kind from other depressions and requires its own special explanation. Yet the numbers of unemployed in 1933 were not different from other depressions by the factor of over 3,000 that separates battle deaths in the Second Schleswig-Holstein War of 1864 from those in World War II. The same could be said of revolutions: would one want to lump the French Revolution with a street riot in Rome in 1825 or a coup d'etat in Bolivia in 1935? Perhaps, but it seems likely that the causes of large revolutions were different from those of most trivial ones, in which case one is stuck with few examples of the important events. The Cambridge Group, although it too is dealing ultimately with large and unique events (industrialization, changes in the nature of the family, an increase in mobility, new levels of education), does not face this problem because it attacks the larger events from, as it were, the bottom up, by studying thousands of individual families or villages. Were the contributors to Nineteenth-Century Society forced, as Singer and Small have been on their own subject, to rely on a comparison of, say, the average experience of 93 separate nations undergoing industrialization or increases in mobility at different times over a century and a half, the probability of reaching credible results would be much smaller. War, unfortunately, is an act of a whole nation, not individuals, and important wars are acts of a handful of important nations. This fact can put tight limits on statistical ingenuity.

These, however, are difficulties with the application of statistics to social events in general, particularly when the relevant social science lacks the models of behavior in the small that could be fitted into models of behavior in the large, not difficulties with the application of statistics to past events alone. Both books give ample evidence that the methods of the social sciences are applicable to history and that the history produced with the methods will enrich the social sciences in turn. A statistician does not need to be told that statistics is applicable whenever there is something to count, to the past as well as to the present. It is historians, economists, sociologists, and political scientists who need sometimes to be reminded of this obvious fact, and these two books, distinguished examples of a rising flood in the last decade or so, will bring it home to them.

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The Working of Econometric Models,

M. Morishima, Y. Murata, T. Nouse, and M. Saito. New York: Cambridge University Press, 1972. ix+339 pp. $22.50.

This is a very good collection of econometric studies. Perhaps one reason is that it is the result of a collaboration of experienced econometric model builders and economic theorists. The book will be used for reference purposes; it probably will be most useful in graduate courses given, presumably, after the students have had a good introduction to econometric theory. It can, however, be used to a limited extent in undergraduate courses assuming that the students are familiar with multivariate calculus and some linear algebra. The book is arranged in four main parts, each of which is interesting.

Part I, by Professors Morishima and Saito, presents a simple Keynsian model of the U.S. economy. This model has only 7 behavioral equations and is estimated using data for the years 1900–52. Except for the income identities the model is estimated in log linear form. In spite of its small size, the model appears to be structurally quite similar (except for detail) to the much larger Brookings and Wharton models. The authors provide the data used for estimation; and because of its size, there is a potential for using the model for simulation experiments in undergraduate macroeconomics and econometrics courses. One troublesome feature of the model is that fixed investment and inventory investment are assumed exogenous. This will lead to seriously biased long run multipliers and corrective measures should be taken.

Part II by Professors Morishima and Nouse features an input-output analysis of the effectiveness of fiscal policies for the United Kingdom. The model used here is essentially a simple Keynsian structure estimated in current prices with no supply restrictions. Accordingly there is no mechanism for determination of prices and wages. Given a national income identity and consumption functions which relate consumption to disposable income for each final demand production category, and given an input-output table, the authors engage in a variety of multiplier experiments. One such experiment was a "balanced" budget multiplier experiment which demonstrated the usual result obtained from simple systems. (A tax increase accompanied by similar government expenditure increases is expansionary.) The use of the I-O table enables them to assess impacts on the sectoral level. Like the study of part I, fixed investment and inventory investment are exogenous. Accordingly, the model is of questionable quality for long term analysis.

Part III by Professor Saito is a general equilibrium model of prices and output in Japan for the years 1953–65. Like the study in part II, an input-output model is incorporated. This I-O model assumes a Cobb-Douglas production process for each industry. Supply equations and demands for the factors of production (labor and intermediate goods) are developed assuming profit maximizing behavior. This is combined with a standard Keynsian income identity (or market clearing conditions), an aggregate consumption function, and a device for generating the consumption final demand component. An import sector for each producing sector is also developed. All other final demand sectors (including investment) are taken as exogenous. The system is solved, and complete system error analysis performed. Multiplier results are reported for national aggregates, such as GNP, and relative prices in response to exogenous shifts in the final demand vector.

Part IV by Morishima and Murata takes a novel approach to identifying disguised unemployment (in Japan). They describe their approach as being in the spirit of Marx's surplus-population theory. As an oversimplification, they begin by defining an individual to be in a state of disguised unemployment if he is employed at a wage rate that is below the equilibrium wage rate for his skill or job category. The model they use is quite similar in structure to those previously reported; there is no need to go into details. A solution is obtained for equilibrium employment in sector i ($\hat{Z}i$) and compared with actual employment (Zi). Disguised unemployment is computed as $Zi - \hat{Z}i$ if $Zi - \hat{Z}i > 0$.

There are many other good things about this collection of papers that might be mentioned, but let's leave it this way: try it, you will like it. It is a pity that the U.S. price is so high; apparently the publishers were anticipating further devaluations of the dollar and used a cost markup rule.

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