COWLES COMMISSION
FOR RESEARCH IN ECONOMICS

REPORT OF FOURTH ANNUAL
RESEARCH CONFERENCE ON
ECONOMICS AND STATISTICS

July 5 to 29, 1938

COLORADO SPRINGS · COLORADO

COWLES COMMISSION
FOR RESEARCH IN ECONOMICS

THE COWLES COMMISSION FOR RESEARCH IN ECONOMICS is a not-for-profit corporation, founded in 1932 for the purpose of conducting and encouraging investigations into economic problems. A function of the COMMISSION is to issue from time to time papers and monographs of an econometric or economic-statistical nature without, however, assuming responsibility for theories or opinions expressed therein. The COMMISSION is affiliated with the ECONOMETRIC SOCIETY, an international society for the advancement of economic theory in its relation to statistics and mathematics.

COLORADO SPRINGS · COLORADO

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THE 1938 CONFERENCE

The Fourth Annual Research Conference was held at Colorado College from Monday, July 5, to Friday, July 29, 1938. Two lectures, followed by discussion periods, were scheduled each morning, the sessions beginning at 9:30 and closing about 12:30. Saturdays and Sundays were left free for recreation.

Since the year 1938 marked the hundredth anniversary of the publication by Antoine-Augustin Cournot of *Recherches sur les principes mathématiques de la théorie des richesses,* it was decided to feature this centenary in the program. The opening lecture by Professor Harold T. Davis was devoted to "Cournot—Pioneer in Scientific Economics." On the invitation of the Commission, the Government of the French Republic sent as its official representative to the Conference Professor René Roy, Chief Engineer of the Department of Bridges and Roads, Professor at the National School of Bridges and Roads and at the Institute of Statistics of the University of Paris. Professor Roy gave a public lecture in French on "La vie d'Augustin Cournot, sa pensée, ses tendances philosophiques et son oeuvre," and two lectures before the Conference: "A propos d'un centenaire: l'oeuvre économique d'Augustin Cournot," and "Étude particulière d'une loi de demande: le trafic postal en France de 1873 à 1936."

In addition to Professor and Mrs. Roy from France, there were present at the Conference participants from seven other foreign countries, Denmark, England, Greece, Italy, Java, Rumania, and Switzerland, and from all sections of the United States.

The scientific program was supplemented by recreation which included a picnic supper for visitors in attendance on July 5. Teas were held at the residences of Professor and Mrs. Harold T. Davis, Mr. and Mrs. Dickson H. Leavens, Professor and Mrs. Francis McIntyre, and President Alfred Cowles 3rd of the Commission, and members of the staff were entertained at a dinner by the out-of-town guests. Swimming and

horseback riding were enjoyed by many, and a number of hikes and picnics were arranged. Points of interest visited on afternoon or week-end trips included the summits of Pikes Peak and Cheyenne Mountain, the Royal Gorge of the Arkansas River, Rocky Mountain National Park (Estes Park), Cripple Creek gold camp, and Mesa Verde cliff dwellings.

The number of people participating in part or all of the sessions at the Conference was 192, including 93 from out of town.

PARTICIPANTS FROM OUT OF TOWN

ALLEN, MR. AND MRS. R. G. D., London School of Economics, London, England.
BERG, MISS MATILDA, 405 West 12th St., Pueblo, Colo.
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BOYD, DR., University of Minnesota, Minneapolis, Minn.
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DODD, PROFESSOR AND MRS. EDWARD L., University of Texas, Austin, Texas.
DUNCAN, PROFESSOR AND MRS. KENNETH, Pomona College, Claremont, Cal.
ELLIS, PROFESSOR AND MRS. HOWARD S., University of California, Berkeley, Cal.
ELVEBACK, MISS MARY L., University of Minnesota, Minneapolis, Minn.
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GONZALES, DR. AND MRS. RICHARD J., Humble Oil and Refining Co., Houston, Texas.
GREEN, MR. L. A., Carbondale, Kan.

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HOOS, MR. SIDNEY S., Food Research Institute, Stanford University, Cal.
JESSEN, MR. RAY, Iowa State College, Ames, Iowa.
JOHNSON, PROFESSOR ERNEST A., Lake Forest College, Lake Forest, Ill.
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KENNEDY, MISS CLAY, Smith College, Northampton, Mass.
LEDERER, PROFESSOR AND MRS. EMIL, New School for Social Research, New York, N. Y.
LERNER, MR. AND MRS. A. P., London School of Economics, London, England.
LINDAUER, MISS ROSE, Trinidad, Colo.
LOTTMAN, MR. AND MRS. VICTOR, Purina Mills, St. Louis, Mo.
LOVERDOS, DR. ALEX, National Bank of Greece, Athens, Greece.
MAYER, DR. JOSEPH, Library of Congress, Washington, D. C.
MENDERSHAUSEN, DR. AND MRS. HORST, University of Geneva, Geneva, Switzerland (now at Cowles Commission).
MINDRUP, MISS JULIA, Laton, Cal.
MOORE, PROFESSOR T. W., United States Naval Academy, Annapolis, Md.
NEWBURY, MR. F. D., Westinghouse Electric and Manufacturing Co., Pittsburgh, Penn.
NORDIN, MR. JOHN A., JR., University of Minnesota, Minneapolis, Minn.
PAPI, PROFESSOR UGO, University of Rome, Rome, Italy.
PEERY, MISS CLARA M., Omaha Technical High School, Omaha, Neb.
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SCHULTZ, PROFESSOR AND MRS. HENRY, University of Chicago, Chicago, Ill.
SCHWIEGER, PROFESSOR AND MRS. ALBERT J., Worcester Polytechnic Institute, Worcester, Mass.
SCOTT, PROFESSOR E. C., Sweet Briar College, Sweet Briar, Va.
SECRIST, PROFESSOR HORACE, School of Commerce, Northwestern University, Chicago, Ill.
SIMMONS, MRS. HAVEN Y., Cedar Rapids, Iowa.
SIMPSON, MR. FLOYD R., University of Minnesota, Minneapolis, Minn.
SINGLETON, MR. ROBERT R., Princeton University, Princeton, N. J.
SMITH, MR. V. E., Holt, Mich.

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HISTORY OF THE CONFERENCES

The Annual Research Conferences on Economics and Statistics, held at Colorado Springs under the auspices of the Cowles Commission for Research in Economics, originated in a series of informal meetings during the summer of 1935 following the sessions of the Econometric Society at Colorado Springs on June 22-24 of that year. At these gatherings various papers were presented and discussed by economists who remained in the vicinity. The meetings were so successful that it was decided to continue them in subsequent years. The following table gives a statistical record:

	1935	1936	1937	1938
Number of Conference Lectures	8	32	40	38
Number of Lecturers	7	20	27	27
Average attendance per lecture	15	27	36	47
Participants from out-of-town	5	55	67	93
" local	20	24	41	99
" total	25	79	108	192

AVAILABILITY OF COMPLETE TEXTS

In this report abstracts are given of all lectures presented at the Conference. In addition, as indicated by footnotes on pages 54 and 71, complete versions of two lectures have been issued by the Cowles Commission through the plan of Auxiliary Publication, and are available as American Documentation Institute Documents in the form of microfilm (images 1 inch high on standard 35 mm. safety photographic film comfortably usable in reading machines now widely available) or photoprints (readable without optical aid). For detailed information write American Documentation Institute, care Offices of Science Service, 2101 Constitution Avenue, Washington, D.C.

It is hoped that in future years it will be possible to make the complete texts of a larger proportion of the Conference lectures available in this way to those interested.

PLANS FOR 1939 CONFERENCE

Arrangements are now being made for the Fifth Annual Research Conference to be held at Colorado College from Monday, July 3 to Friday, July 28, 1939. Rooms and board for those attending the Conference will be available in dormitories of Colorado College at about $40 for the four weeks or $11 per week. Approximate summer round-trip railroad rates to Colorado Springs are: from New York, $86; from Chicago, $45; from Los Angeles, $61; from Dallas, $32.

There is no charge for attendance at the Conference. All serious students are welcome. Those who are interested should notify the Cowles Commission in order that they may receive in the Spring a preliminary announcement of the program.

ABSTRACTS OF CONFERENCE LECTURES

Tuesday, July 5—Cournot—Pioneer in Scientific Economics, HAROLD T. DAVIS, Professor of Mathematics, Northwestern University, and Research Associate, Cowles Commission.

The reasons for the neglect for more than a third of a century of the *Researches into the Mathematical Principles of the Theory of Riches,* published by Antoine-Augustin Cournot (1801-1877) in 1838, may perhaps be discovered in a comparison between the development of astronomy and the development of economics. Modern astronomy began with the data of Tycho Brahe. From these data Kepler discovered empirically his three famous laws, the keystones of modern astronomy. The foundations of mechanics were laid a few years later by Galileo and the grand synthesis, mathematically formulated in Newton's *Principia,* appeared approximately a half century after the work of Galileo. Thus in astronomy we had the data first, statistical analysis second, the foundations of mechanics third, and the mathematical formulation last of all. In economics the situation was reversed. The mathematical foundations were laid first by Cournot in 1838; these were investigated and extended a third of century later by Jevons, Walras, Edgeworth, Pareto, Marshall, and others. The real assembling of adequate data was delayed until the early years of the twentieth century and the statistical determination of the functions and parameters of the equations of Cournot is just now being carried out.

The events of Cournot's life are most completely found in his own *Souvenirs*, which he wrote at the age of 59. An extensive account of his activities, based largely upon these memoirs, is found in an article by H. L. Moore published in the *Revue de métaphysique et de morale,* Vol. 13, 1905. This enlightened journal devoted a complete number to the life and thought of Cournot and from the twelve articles which it con-

tained one may obtain a full view of the breadth of Cournot's intellectual activity.

Cournot realized early that the best foundation for a scientific career was mathematics and his first scientific publications were mathematical articles in the fields of probability and the dynamics of rigid bodies.

Cournot possessed an unusual capacity for administrative work. His ability to occupy important administrative positions and at the same time to carry on an extensive research program may, perhaps, be explained by the following observations which he made: "I quickly perceived that what one calls good administration consists of two things, machinery and good sense: the machinery for form, and the good sense for the basis of things."

Cournot's writings were in a variety of fields. He published treatises on the theory of functions and the infinitesimal calculus, the theory of probability, and the origin and limits of the correspondence between algebra and geometry. Eight volumes on the Napoleonic campaigns were prepared largely by him under the direction of the Marshal Gouvion Saint Cyr. During this period Cournot also made translations of the *Astronomy* of John Herschel, which was published in 1833, and the *Mechanics* of Gardner, which appeared the following year. In addition to his celebrated *Theory of Riches* he also wrote two other works in economics, largely to explain and interpret his earlier mathematical treatise; his scientific career culminated in two volumes of philosophy entitled: *Traité de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire.* The scope of this work is seen from the five books with titles as follows: (1) Order and Form; (2) Force and Matter; (3) Life and Organism; (4) Human Society; (5) History and Civilization.

The influence of Cournot's work, in spite of its inauspicious initial reception, has steadily increased. George Sarton included his name among the first 118 greatest mathematicians of the nineteenth century. Econometricians universally pay tribute to his genius; and, in spite of other econometric works of merit published earlier, it is probable that mathematical economics in the modern sense may be considered to have originated with the publication of the *Theory of Riches* in 1838.

Tuesday, July 5—Our National Economic Statistics: Some Blind Spots, VERGIL D. REED, Assistant Director, United States Bureau of the Census.

Twelve blind spots in our national economic statistics are in the fields of: prices; production; stocks and inventories; physical distribution of goods; flow of funds; credits; orders; labor; construction; consumers' purchases and standards of living, including rents and individual and family savings; business savings and investment and costs; and income and wealth distribution.

Conclusions as to the prices of various commodities in different industries and their trends have been and are being based on quite inadequate data. Innumerable kinds of pricing give rise to price variations affecting a series. There is need to fuse these and other price variations from period to period into unit figures on commodities of like quality and thus to reflect the actual conditions for particular commodities in particular industries. There is a lack of sufficient data on retail and wholesale prices, as well as prices of manufactured goods.

In the field of production we know astonishingly little about what happens in the production of consumer goods. Most production figures refer to goods which are for further fabrication or industrial use. Lack of information concerning inventories is responsible for one of the most pitifully blind spots in our economic statistics; quarterly reports on inventories are needed.

There is no statistical information available which charts the flow of goods from the producer to the consumer, analyzing the time the goods remain in the hands of each middleman and pointing out differences in the time that it takes for the various types of commodities to go from one stage to another. Equally as important as the flow of goods is the flow of funds. We need a complete study and some kind of measure of the flow of goods and flow of funds through all of our economic activities, showing just how much went through retail trade, wholesale trade, etc., and where the flow stagnated. Closely associated with the flow of funds is the volume, flow, and use of credit. There is a lack of statistics on this subject and a need for a series of statistical pictures—kept up to date.

Current information on orders, both placed and unfilled (with adjustments for orders cancelled), is urgently needed, as are additional statistics on shipments of goods. The construction industry is an outstanding example of an industry for which good statistics are not available; the Census Bureau has been able to obtain national coverage in censuses taken in 1929 and 1935.

We need two types of statistics on unemployment. One is a periodical census which covers the field adequately and from which considerable detail concerning the family can be obtained. The other is a current reporting system of unemployment counting, with adequate facilities for keeping the information accurate and up-to-date.

There is a need for additional data on consumers' purchases and standards of living, although data on these subjects have been improved greatly in the last ten years. More data concerning business savings, investments, and costs are especially desirable. Additional information should be procured on distribution of income and wealth. What is needed, first of all, is agreement on the definitions of what to include in wealth and income.

Important also is the collection of information concerning size of business organizations, the relation of current industrial production to current capacity, and the relationship existing between prices, capacity, and employment.

The whole problem of the relation of competition to our economic system should be studied in the strong light of unbiased statistics, rather than in the dim light of philosophy and theory. In doing so, we should keep in mind at all times that the present society in which we live is primarily a complex industrial society engaged in large-scale production, while the economic society in which the classical economists lived and from which they drew their conclusions was a society made up of agriculture and trade conducted on a very small scale with little manufacturing. We are already altering many of the economic beliefs which a few decades back were thought of as economic law. As facts replace wishful thinking, we undoubtedly shall have to change many other theories.

Wednesday, July 6—The Significance of the Curve of Income, PROFESSOR DAVIS.

The purpose of this paper is to supply a rationale for the celebrated law of the distribution of income originally announced by V. Pareto in the first chapter of the second book of his *Cours d'économie politique,* published in 1897. This law may be stated as follows:

In all places and at all times the distribution of income in a stable economic system, when the origin of measurement is at a sufficiently high income level, will be given approximately by the empirical formula $y = a x^{-\nu}$ *where* y *is the number of people having the income* x *or greater, and* ν *is approximately 1.5.*

A résumé of available data exhibits the validity of the law for a number of different states. For the United States the average value of ν is equal to 1.56 with a standard error of 0.12. It may be conjectured that any exceptional deviation from the mean value of ν would result in civil disturbance. In other words, too high a concentration, as in absolute aristocracies, or too low a concentration, as in socialistic states, are equally destructive to productive enterprise and a stable social order. Unfortunately no data are at present available to determine these disruptive limits.

The problem of determining a general distribution function for the representation of incomes, from the lowest level to the highest, is of interest. Formulas have been suggested by Pareto, L. Amoroso, D. G. Champernowne, and R. Gibrat. The following general distribution function is suggested:

If $\varphi(x)$ represents the number of individuals who possess an income between x and $x + dx$, then $\varphi(x)$ may be written

$$\varphi(x) = \frac{a}{z^n} \frac{1}{e^{b/z} - 1}, \quad z = x - c,$$

where c is the income necessary to sustain life and n is approximately 3.5.

It is easily verified that $\varphi(c) = 0$, and that, for sufficiently large values of x, $\varphi(x) \sim (a/b)\ x^{-(n-1)}$, that is to say, the distribution is of Pareto form.

The parameters for United States data in 1918, as reported by the National Bureau of Economic Research, are found to be $n = 3.67$ and $c = \$623$. Since the estimated population in 1918 was 103,588,000 and since the data reported the income of 37,569,060 persons, there was an average of 2.75 people depending upon the subsistence income, or \$226 per person. This value does not appear to be unrealistic.

The form of the frequency function was derived as follows: Consider a typical income class x, to which N_x individuals aspire to belong. If the total income for that class is assumed to be I_x, then there will be $P_x = I_x/x$ places in the class which can be filled. The number of ways in which P places can be assigned to N individuals is given by $Q = (N+P)\,!/(N!\,P!)$. The fundamental assumption is then introduced that the rate of change of Q with respect to P varies directly as Q, but inversely as the size of the income, that is $dQ/dP = kQ/x$. By means of Stirling's approximation for the value of factorials, and employing the assumption just stated, we may readily deduce

$$P_x = \frac{N_x}{e^{b/x} - 1}.$$

When N_x is determined so that P_x is asymptotic to the Pareto distribution for large values of x, the frequency function $\varphi(x)$ is obtained.

Following a suggestion made by Mr. Carl Snyder,[1] the Pareto distribution, as contrasted with the normal distribution, was regarded as valid in the discussion of the frequency distribution of special abilities. The distribution of incomes would thus be considered as a special case of this more general law which applies to the measurement of psychic abilities as contrasted with biometric measures or other measures essentially Gaussian in their form. Data were introduced from billiard scores to show that skill in this game was of Pareto form. A study by A. J. Lotka, on "The Frequency Distribution of Scientific Productivity,"[2] showed that scientific ability also fol-

[1] Cowles Commission for Research in Economics, *Report of Third Annual Research Conference on Economics and Statistics, June 28 to July 23, 1937*, pp. 60-62.

[2] *Journal of the Washington Academy of Sciences*, Vol. 16, 1926, pp. 317-323.

lowed the Pareto pattern. It was further pointed out that the executive salaries in large corporations are distributed in the same manner, indicating that executive ability follows the same law of distribution as incomes in general.

Dr. Corrado Gini has given a selected bibliography relating to the problem of the distribution of incomes.[3]. The following bibliography is supplementary to his:

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TUCKER, RUFUS S. "The Distribution of Income among Income Taxpayers in the United States, 1863-1935," *Quarterly Journal of Economics*, Vol. 52, 1938, pp. 547-587.

YOUNG, A. A. "Do the Statistics of the Concentration of Wealth in the United States Mean What They Are Assumed to Mean?" *Journal of the American Statistical Association*, Vol. 15, (new series), 1917, pp. 476-484.

Wednesday, July 6—Our National Economic Statistics: Suggestions for Improvement, DR. REED.

Economists, sociologists, and statisticians can make a contribution to humanity comparable in terms of human happiness to those of medicine and science by bringing the manifold forces of our economic life into the light of factual knowledge and statistical direction.

Improvement in economic statistics and their uses would secure for the people benefits commensurate with the country's enviable resources and free them from the demoralizing effects of violent depressions and booms. Six practical ways to improve economic statistics are: improvement in methods of collection, including duplication and its elimination; classification and comparability; improvement in analysis and interpretation; more timeliness of statistics; better publicity of the value and need for the data; and refinement of statistical data.

The country has little co-ordination and no well-rounded plan for the collection of national economic statistics. Each statistical agency gathers what it needs and duplication often results. This puts an additional burden on the business man who must furnish the original data. Unless something is done to co-ordinate properly the collection of these data and avoid duplication, the statistical fountain is going to dry up through increased business resistance and resultant high cost of collection.

Two ways of alleviating this burden on the business men are desirable. The first is to avoid having more than one agency collecting similar information from a respondent. The

second is to furnish the business man with more data in a form which will be of value to him in running his own business.

Constant improvements in questionnaires, more uniform accounting methods by business men, and establishment of standard classifications are needed forward steps. A big advance has been made within the last year in working out a standard classification of industries to be used by all governmental and private agencies which collect similar data.

More interpretation, analysis, and application of data we already possess are needed. William Randolph Burgess of the Federal Reserve Bank of New York said recently: "The problem involved is not one of simply collecting data, but one of interpretation. In fact, it would be my own judgment that we are more defective in our interpretation of the available data than in the matter of the supply of facts."

Increased timeliness of economic statistics is an urgent need. Economists and business men are clamoring for more up-to-the-minute data and often voice the criticism that government reports are "cold" when the user gets them. This condition is often caused by late reporting on the part of respondents. It behooves all agencies, concerns, and individuals interested in more up-to-the-minute statistical information to co-operate with the collecting agencies and point out the value of the data to those very concerns which are dilatory.

There are two general types of uses to which economic statistics are usually put. One is to chart currently the trend of an industry or trade, or some other phase of economic behavior. The second is for use in historical or long-term analysis. Business men are primarily concerned with the first, while the economist takes an interest in the second.

A central directory or list of available data of an economic nature should be set up. Such a directory would have to be arranged alphabetically by subjects with cross references and be capable of use by those lacking extensive statistical training. Weekly or monthly supplements would probably be necessary with a yearly revision of the directory. The Census Bureau is attempting to bridge this recognized gap in its information service by issuing indexed division catalogues covering all the reports issued by the individual producing divisions, such as population, business, agriculture, etc. These

catalogues will supplement the bulletins published by the Government Printing Office which present Census material available in printed booklets and volumes.

There is a great need of wider publicity of practical uses of available economic data. The better-equipped trade associations, chambers of commerce, and libraries are doing a splendid job of publicizing economic data by making them available to, and interpreting them for, the ultimate user. Through booklets, speeches, and articles, the staff of the Department of Commerce are attempting to assist these organizations in their activities by suggesting uses of the data.

In connection with the need for greater refinement of statistical data, a committee has been organized to advise the Bureau of the Census concerning the construction of metropolitan districts for use in the 1940 Census. These districts, along with census tracts in most of our large cities, should afford a degree of territorial refinement for our social and economic statistics which will make possible the detailed analysis of real socio-economic communities in sharp contrast to the highly unsatisfactory and purely artificial cities as defined by corporate limits.

Three conclusions are: First, there is no national statistical program based on clearly defined objectives and needs; second, steps must be taken to eliminate definitely recognized and specific blind spots in our economic statistics; and, third, we must cease to be known as wizards and sorcerers in the public mind and must encourage wider acquaintanceship with, and practical application of, the output of our efforts.

Thursday, July 7—"Clearing Variates" in Confluence Analysis, HORST MENDERSHAUSEN, University of Geneva (now at Cowles Commission).

Confluence analysis[1] aims at detecting coefficients for hypothetically adopted relations between two or more obser-

[1] Ragnar Frisch, *Statistical Confluence Analysis by Means of Complete Regression Systems*, Oslo, 1934.

vational variates. The question considered in this paper relates to the explanation of a variate supposed to be causally dependent on other variates. The assumption of a one-way causation can frequently be brought into harmony with the general interdependence of observable facts: we consider only a certain narrow section of the universe of nature and human society. In the universe, general interdependence rules. But within a given section certain ways of causation can be neglected because they lie beyond the range of the section and, therefore, are relatively unimportant. In an analysis of the confluence of more than two variates there may be two different ways by which a determining variate (x_n) displays its useful action. This x_n may be a *causally connected* variate or a *clearing variate*. Let us understand by a causally connected determining variate a variate which has a causal influence on the dependent one. A clearing variate is a useful determining variate without causal connection with the dependent one; its role in the set consists of clearing another determining (observational) variate from the effect of a disturbing basis variate.

The variations of the observational dependent variate (x_1) are due to variations of phenomena which we may call the *basis variates* ($y_1, y_2, \cdots, y_n$). Assuming further a linear combination of the influences of the basis variates, we have

$$x_1 = a_{11}y_1 + a_{12}y_2 + \cdots + a_{1n}y_n,$$

where the a's represent structural basis coefficients.[2] The values of the observational determining variates depend equally on the values of certain basis variates. These basis variates may or may not be the same as those active in the determination of x_1. Various situations in which a clearing variate appears can be conceived. Let us take a simple example. We wish to explain the volume of production of the manufacturing industries of different districts of a country by the number of wage earners in these industries (y_1). We have values for the dependent variate (x_1) and assume that $x_1 = a_{11}y_1$. We have,

[2] All variates are taken as deviations from their — static or dynamic — means and normalized. We assume that all variates x and y are effective — i.e., $\neq 0$ in at least two terms — and that all basis variates ($y_1, y_2, \cdots, y_n$) are uncorrelated. Let us further suppose that none of the structural basis coefficients equals either 0 or ∞: $0 \leqq \alpha < \infty$.

however, no observational variate expressing y_1, but only total numbers of wage earners in manufacturing industries and mines ($x_2 = y_1 + y_2$). If y_2 is not a function of y_1, which is here assumed, x_1 and x_2 will not form a complete set; x_2 is disturbed by the basis variate y_2. Suppose now we had data for the volume of production of mines (x_3) and were allowed to take this volume as a linear function of the number of occupied miners, that is $x_3 = \alpha_{32}y_2$. The introduction of x_3 will then render the set complete:

$$x_1 = \alpha_{11}y_1 ,$$

$$x_2 = \alpha_{21}y_1 + \alpha_{22}y_2 , \quad (\text{where } \alpha_{21} \text{ and } \alpha_{22} = 1),$$

$$x_3 = \alpha_{32}y_2 ,$$

$$x_1 = \alpha_{11}x_2 - \frac{\alpha_{11}}{\alpha_{32}}x_3 .$$

The variate x_3 must be interpreted as a clearing variate for x_2; it would be erroneous to conclude that we had verified a causal connection between x_1 and x_3. The result should be stated in the following way: the work of $x_2 = w$ wage earners in manufacturing industries and mines gives rise to a volume of production of $x_1 = \alpha_{11}w$ in the manufacturing industries if the volume of production of mines (x_3) is statistically kept constant. It has no meaning to say that a volume of production of the mines $x_3 = p$ gives rise to a volume of production of $x_1 = -\frac{\alpha_{11}}{\alpha_{32}}p$ in the manufacturing industries if x_2 is statistically kept constant.

Certain criteria for clearing variates can be developed. A comprehensive formulation of the hypothetical relations between the variates in terms of observational and basis variates appears useful for statistical analysis and a meaningful interpretation of its results.

Thursday, July 7—The Theoretical Derivation of Dynamic Demand Curves, GERHARD TINTNER, Assistant Professor of Economics and Mathematics, Iowa State College.*

This is a continuation of the paper given at last year's Conference[1] and later published.[2] It is an extension of the work of Hicks and Allen[3] for the dynamic case.

The individual is at the point in time 0 and plans in the discontinuous case for the points $1 \cdots n$. Denote by x_{vs} the amount of the commodity X_v which he plans to consume at the point in time s. Let p_{vs} be the expected price of this commodity, i_s the expected interest rate, $r_s = 1 + i_s$ the expected accumulation factor for the period s, and $R_s = r_1 r_2 \cdots r_s$ the expected accumulation factor for the period $0 \cdots s$; define $q_{vs} = p_{vs}/R_s$ the discounted price. Let I_s be the expected income for s and $J_s = I_s/R_s$ the discounted income. An integrable utility function F depends upon the $x_{vs} (v = 1, \cdots, m; s = 1, \cdots, n)$ and is to be maximized under the following condition (budget equation):

$$(1) \qquad \sum_{v=1}^{m} \sum_{s=1}^{n} x_{vs} q_{vs} = \sum_{s=1}^{n} J_s = J,$$

where J is the total discounted income.

The solution is

$$(2) \qquad -q_{vs}\lambda + F_{vs} = 0, \quad (v = 1, \cdots, m; s = 1, \cdots, n),$$

where λ is a Lagrange multiplier (marginal utility of money). Differentiating (1) and (2) we get the following linear system:

(3)

$$\sum_{v=1}^{m} \sum_{s=1}^{n} q_{vs}\, dx_{vs} = -\sum_{v=1}^{m} \sum_{s=1}^{n} x_{vs} dq_{vs} + \sum_{s=1}^{n} dJ_s = -\sum_{v=1}^{m} \sum_{s=1}^{n} x_{vs} dq_{vs} + dJ$$

$$-q_{ut} d\lambda + \sum_{v=1}^{m} \sum_{s=1}^{n} F_{vs,ut} dx_{vs} = \lambda dq_{ut}, \quad (u = 1, \cdots, m; s = 1, \cdots, n),$$

* This paper has been accepted for publication in *Econometrica*.

[1] Cowles Commission for Research in Economics, *Report of Third Annual Research Conference on Economics and Statistics, June 28 to July 23, 1937*, pp. 91-93.

[2] "The Maximization of Utility over Time," *Econometrica*, Vol. 6, April, 1938, pp. 154-158.

[3] "A Reconsideration of the Theory of Value," *Economica*, New Series, Vol. 1, 1934, pp. 52-76, 196-219.

where the subscripts of F denote partial derivatives.

We introduce the following determinant:

$$(4) \qquad D = \begin{vmatrix} 0 & F_{11} & F_{12} & \cdots & F_{mn} \\ F_{11} & F_{11,12} & F_{11,12} & \cdots & F_{11,mn} \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ F_{mn} & F_{mn,11} & F_{mn,12} & \cdots & F_{mn,mn} \end{vmatrix}.$$

Denoting minors in the usual manner we get the following general result for the demand differential:

$$(5) \qquad dx_{ut} = \frac{\lambda}{D}\Big[\Big(-\sum_{v=1}^{m}\sum_{s=1}^{n} x_{vs}dq_{vs} + \sum_{s=1}^{n} dJ_s\Big) D_{ut} + \sum_{v=1}^{m}\sum_{s=1}^{n} dq_{vs}D_{vs,ut} \Big].$$

We can derive from this equation income, price, and interest derivatives and also elasticities. Introducing $k_w = J_w/J$ we get the following relationships between interest elasticities:

$$(6) \qquad \frac{E\, x_{ut}}{E\, J_w} = \frac{E\, x_{ut}}{E\, I_w} = k_w \frac{E\, x_{ut}}{E\, J}.$$

If we define $k_{zw} = (x_{zw}q_{zw})/J$ and the elasticity of substitution

$$\sigma_{ut,zw} = (D_{ut,zw}\sum_{v=1}^{m}\sum_{s=1}^{n} x_{vs}F_{vs})/(x_{ut}x_{vs}D),$$

the price elasticity appears as:

$$(7) \qquad \frac{E\, x_{ut}}{E\, q_{zw}} = \frac{E\, x_{ut}}{E\, p_{zw}} = -k_{zw}\frac{E\, x_{ut}}{E\, J} + k_{zw}\sigma_{ut,zw}.$$

The interest elasticity is:

(8)

$$\frac{E\, x_{ut}}{E\, r_w} = -\frac{1}{J}\frac{E\, x_{ut}}{E\, J}\Big[\sum_{s=w}^{n}(J_s - \sum_{v=1}^{m} x_{vs}q_{vs})\Big] - \sum_{v=1}^{m}\sum_{s=w}^{n} k_{vs}\,\sigma_{ut,vs},$$

where the term in square brackets is the expected saving in the period $w \cdots n$.

These results can be generalized for the continuous case where utility is a functional rather than a function. The income, price, and interest elasticities appear then as the solutions of systems of integral equations.

Friday, July 8—Factors Determining Domestic and Foreign Copper Consumption, FRANCIS MCINTYRE, Research Associate, Cowles Commission, and Associate Professor of Econometrics, Colorado College.

We must first trace briefly the postwar economic history of the copper industry. Various efforts at control of prices and production had evolved from associations of producers during this period. One era of control reached its peak in 1929-30, when the New York price of refined copper was established at 18 cents a pound in April of 1929, and held at that level until April of 1930.

It is remarkable that this price of 18 cents was maintained for one full year, in the face of the greatest market crash in history, but there is no evidence that it was wise. Control finally broke in April of 1930, and prices fell 30 per cent in a month, 50 per cent in a year. The 18-cent price created much resentment, and almost no sales, so that huge inventories developed, in the face of self-imposed restrictions in copper production by the big producers.

The consistent restriction policies of the most efficient (technologically speaking) American producers transferred the bulk of low-cost production to sources outside of the United States. This country has never consumed, on net balance, a pound of imported copper, but a tremendous hue and cry arose for a tariff on the importation of the red metal, and in 1932 a duty of 4 cents per pound was enacted, immediately dividing the world copper industry into two self-contained parts. Great differences have developed, since the enactment of the tariff, in the type of control appearing in these two distinct parts of the copper world.

The foreign cartel represents at least 95 per cent of non-United States production. Close control over volume of production is exercised, and each participant agrees to limit his offerings upon the market to a commonly accepted percentage of production in a certain base period which is not disclosed. This percentage was set at 105 per cent last fall, and has just this month (July 1) been reduced to 95 per cent. Thus production and offerings are rather precisely determined, but the price remains relatively free to seek its economic level.

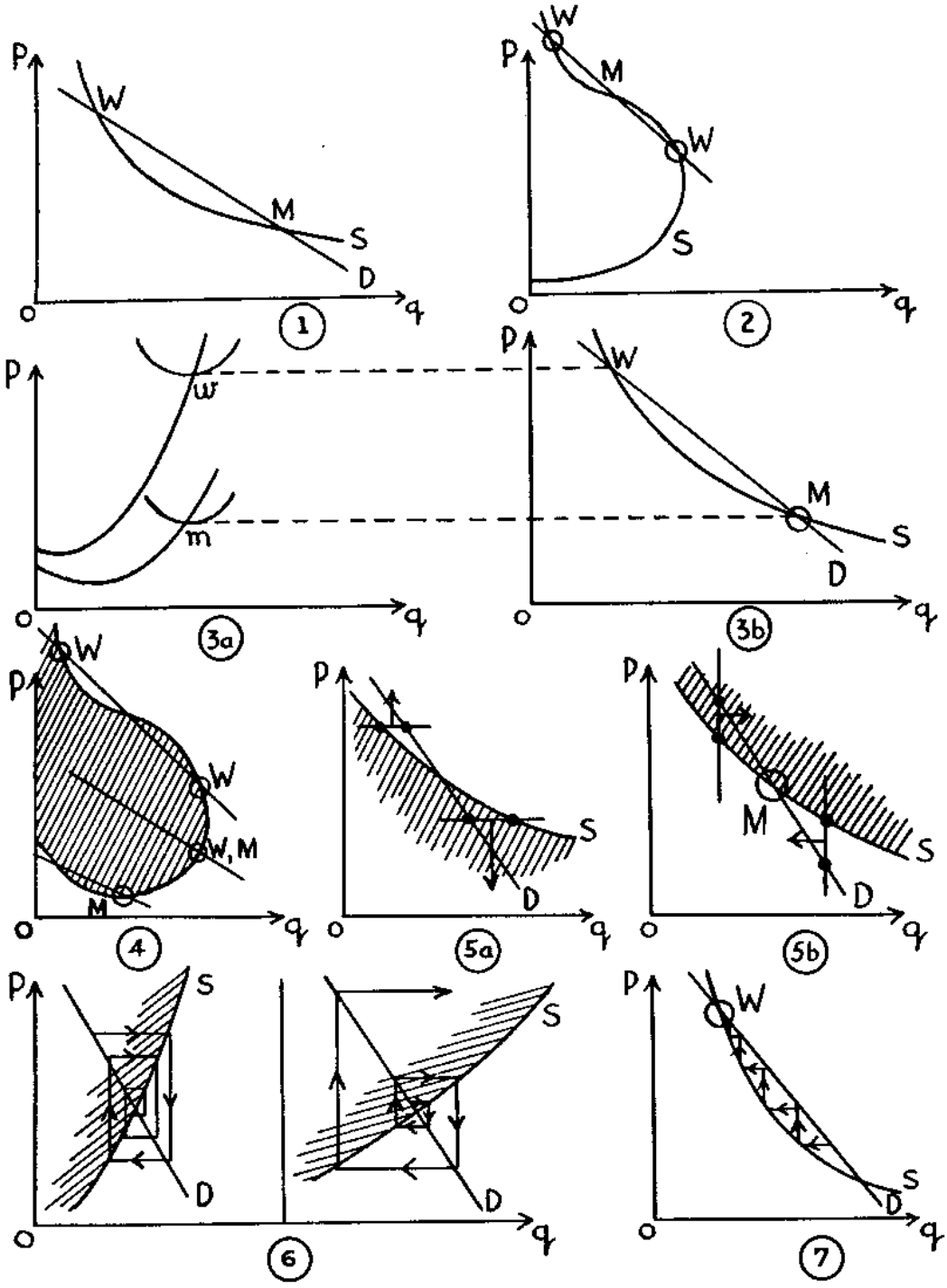
The pricing of copper in the United States appears to be quite a different affair. Adoption of the procedure of the European cartel, if overt, would almost certainly be made the basis of court action by the Federal Trade Commission. It seems clear that, instead, the large copper producers in this country quote a price which is raised whenever consumption shows the least signs of sustaining it, and lowered only when smaller producers, desperate for sales, "shade" the price. This means that, of the two variables, price and consumption, consumption or domestic utilization of copper is much more nearly free to seek its economic level in the American market.

The lantern slides reveal the success of our statistical efforts to estimate price and utilization of copper in the domestic and in the world market, using as additional variables in the analysis only the level of industrial production in general, and of wholesale prices. They show that much closer estimation of the "free" variable than of the controlled variable is possible.

Thus in London, market for the rest of the world except the United States, we can estimate consumption with an accuracy of approximately 94 per cent, but we can estimate price, the relatively free variable, with an accuracy of about 97 per cent. In this country, where the relatively free variable is consumption of copper, we can estimate it with an accuracy of 95 per cent, while our accuracy in estimation of price is only 80 per cent.

Friday, July 8—The Nature of Supply Curves, ABBA P. LERNER, Assistant Lecturer in Statistics, London School of Economics.

According to Walras, who argues in terms of the adjustment of price when demand is unequal to supply, *W* in Figure 1 represents a position of stable equilibrium and *M* a position



of unstable equilibrium; while according to Marshall, who argues in terms of the adjustment of output when price is unequal to average cost, the reverse is the case.

Figure 2 shows a supply curve with a "backward rising" section where Walras' arguments apply but not Marshall's. Figure 3b shows a curve that is " forward falling" because of external economies of scale which affect the average and marginal costs of the firm (shown in Figure 3a). Here Marshall's argument applies but not Walras'.

Figure 4 shows a generalized supply curve combining both of these elements and illustrates a more general rule that would satisfy both Walras and Marshall. *Equilibrium is stable where the demand curve comes down to the right out of the shaded area of effective supply.*

Figures 5a and 5b show that both Walras' and Marshall's conclusions about the equilibria are based upon the assumption of supply and demand reacting simultaneously. The points on the two curves joined by a horizontal (Walras) or vertical (Marshall) straight line moving together towards a a stable or away from an unstable equilibrium.

Figure 6 shows the "cobweb" complication that arises even when the curves cut the right way (satisfying both Walras' and Marshall's and our own more generalized formula) if there is a time lag in supply and the supply curve is flatter than the demand curve.

Figure 7 shows that for the "backward rising" supply curve there is no additional "cobweb" complication because if the supply curve is flatter than the demand curve it also cuts it the wrong way, and vice versa. This argument cannot be applied to the "forward falling" supply curve because it is never a "true" short-period supply curve (in the peculiar sense required here) but rather a long-period curve of supply price.

Monday, July 11—The Price-Level Concept, O. STRANGE PETERSEN, Lecturer in Statistics, University of Aarhus, Denmark.

Few if any concepts are used more frequently in economic literature than that of the price level, and few have been discussed more extensively. Nevertheless there is a deplorable lack of precision as to the meaning of the concept. In textbooks it is generally introduced without any explicit or precise definition; and in the discussion of index numbers certain formal "tests" are often formulated instead of a clear statement of what exactly is to be measured.

There are several distinct notions lying behind the term price level, and in many discussions it is necessary to analyze the application of the concept in order to find out which one is meant.

The three most important price-level concepts seem to be:

(a) The price level in the strict sense, which assumes that all prices considered move at the same time, in the same direction, and in the same degree, apart from random fluctuations in individual prices. A general price level in this sense does not exist in the real world, and, if it did, would be of no economic significance. The concept, however may be useful when dealing with a small homogeneous group of prices.

(b) The price level as introduced in the equation of exchange,

$$PT = mv\,,$$

where PT stands for $\sum p_i t_i$. Thus interpreted, P and T should be conceived of as vectors (the "price situation" and the "quantity situation"), but when the term price level is introduced it is assumed that P can be replaced by an index number. This would only be possible if all the t's remained constant, or if the changes in T were changes in level in the strict sense. In this case an index for T could be determined independently of the other expressions in the equation, which could then be solved for the "price level." In general, however, there is no logical solution.

Nevertheless this indeterminate concept, or an equally vague notion of some price average, weighted according to the

"importance" of the commodities, is that most frequently thought of when the term price level is used in economic theory. In this sense the price level is conceived of as a kind of index of the total monetary value of transactions. It is a general price level, and cannot be applied to parts of the price system separately.

(c) The price level in the sense of the cost of living. This concept can in theory be precisely defined for a group of persons with the same scale of preferences and the same income.

While these concepts are the most important, still other notions are sometimes introduced. In the discussion of a standard of deferred payments, the price level is used as a means of determining the amount which a creditor justly and fairly should be repaid. Here a moral element is taken into consideration. More important are the attempts to use the price-level concept in the analysis of business cycles. As all modern monetary theorists agree, however, an explanation of the business cycle cannot be given in terms of the general price level but only by considering changes in relative prices. Such statements as that the business cycle will be avoided if the price level is stabilized have in themselves no meaning. Haberler has pointed out that the problem is to define the price level in such a way that it becomes significant in this sense. It is extremely unlikely, however, that any single price index could be found which would serve this purpose.

From the statistical point of view, the purpose of computing an index number to express the price level may have one of two meanings:

(1) To study the causes of price movements by isolating the effect of some common cause which is presumed to have affected all prices considered in the same manner. This idea underlies any attempt at determining the price level from a sample, but the necessary assumptions are never fulfilled in the price system as a whole, and not even in any comprehensive group of prices such as for example wholesale prices.

(2) To measure the effects of price changes. It is sometimes possible to measure the significance of price movements with respect to a definite question (for instance to describe the effects of given price changes on the satisfaction derived

by a given person from a given income). No single measurement is sufficient to measure the significance in all respects; and with regard to some important problems the answer cannot be given at all in a single number.

Index numbers may be useful in many cases as a first rough guide, but for more thorough purposes a detailed study of individual price movement is necessary.

It would appear that a purification of terminology would clarify discussions on price questions.

Where only vague notions of price movements are involved, equally vague expressions such as "a tendency of prices to rise" should be preferred to "a rise in the price level," and where the cost of living is meant this more precise expression should always be used rather than "the price level."

The most appropriate meaning of the term price level seems to be that described above as "the strict sense." Its field of application is narrow, but the loss would hardly be grave.

To be a useful tool in scientific analysis a concept should be precisely, explicitly, and unequivocally defined. The price-level concept does not meet these requirements and perhaps therefore would be best avoided altogether.

Monday, July 11, Tuesday, July 12—The Dynamics of the Current Depression, CHARLES F. ROOS, Director of Research, Institute of Applied Econometrics.

The purpose of this paper is to subject various theories of business cycles to statistical verification or rejection in so far as new data obtained from confidential business sources permit, and to offer extensions or modifications.[1] The author's own theory of price and production fluctuations,[2] which con-

[1] Acknowledgment is made to Victor S. von Szeliski, who constructed the series used in this paper.

[2] C. F. Roos, "A Mathematical Theory of Price and Production Fluctuations and Economic Crises," *Journal of Political Economy*, Vol. 38, Oct., 1930, pp. 501-522.

tains the elements of the later theories of Frisch, Keynes, J. M. Clark, Tinbergen, and Haberler, has been used as the quantitative formulation most suitable for testing.

The total of all money transactions is obviously related to the volume of trade, but it is necessary to correct for financial transactions. In its simplest usable form the equation of exchange becomes:

$$(1)\quad MV + y_c M_c V_c + (1 - y_c) M_c V_c + M_b V_b = \frac{1}{T} \sum_{i=1}^{F} P_{mi} q_{mi} + \sum_{i=1}^{G} P_{ci}\, q_{ci} + \sum_{i=1}^{R} p_{ri} q_{ri} + \sum_{i=1}^{S} p_{si} q_{si},$$

where M is the quantity of money in circulation in the interval of time t to $t + T$, and V is its transaction velocity; M_c = quantity of bank money, and V_c its velocity; M_b = quantity of other money, brokers' loans, corporation credits, etc., and V_b its velocity; and the parameter y is the proportion of bank money being used for the purchase of goods and services. The first sum on the right-hand side is the total of money transactions involving raw materials; the second, consumers' goods and new capital goods; the third, capital-goods ownership in the form of stocks and bonds; the fourth, dividends, interest, taxes, etc.; and the last, services.

After deducting transactions in stocks and bonds, interest and dividend payments, taxes, and currency transactions, which are mostly wage and salary payments, this reduces to

$$(2)\quad M_c V_c = \frac{\lambda}{T} p_{gi} q_{gi} - \nu W,$$

where λ and ν represent correlation coefficients and W an index of pay rolls. The comparison of productive expenditures $M_c V_c$, thus computed by correcting bank debits, with stock prices and commodity prices, shows that changes in the latter follow those in the former, with a lag of several months.

The relationship of M_c and V_c, if any, is important. An investigation shows that, except during the period 1926-1931, when it is doubtful whether demand deposits fully represented money supply, the transaction velocity, except in recessions or booms, has been relatively stable. Velocity remained on one

plane when banks gave immediate credit for checks deposited and on a lower one after the principle of deferred credit was introduced. During the transition period 1930-1933 velocity declined. It is also true that velocity has always decreased sharply about eight or nine months after money supply has begun a decreasing trend, and has begun to recover to the stable area about the time money supply first began to increase or some governmental policy was announced that would reasonably insure increasing money supply. In the boom rising commercial loans to finance abnormal orders and inventory building usually induce lower bond prices. This is competition purely for bank reserves.

It still remains to show whether or not increases or decreases in productive expenditures regularly begin with purchases of consumers' or capital goods and whether nonmonetary factors can induce important changes. A series of the physical volume of consumer-goods orders (other than food) based on confidential figures from major companies, and a series of the physical volume of retail sales, obtained by dividing an index of retail prices into a weighted average of department-store, mail-order-store, and variety-store sales, corrected for trend, have been compared. In the short run, the difference between the two series must be represented by changes in inventory, with a time lag to allow for fabrication and shipment. This expected relation holds satisfactorily for an inventory series constructed from percentage of warehouse space occupied and department-store stocks corrected for price changes. The lag is two or three months, except in the 1934 period when inventory building seems to have occurred without orders rising above consumption. This may have been caused by shifts in trade from small to large units under the N.R.A. Thus total consumption, as distinct from that measured by the index, may have been below orders. Other divergencies from the normal relation seem to be associated with governmental action or expectation of such action.

In respect to the flow of capital goods, the data bear out the theory that changes in the demand for, and production of, consumers' goods and services, tend to give rise later to much greater changes in the demand for, and production of, those producer goods which are used for their production.

In the winter of 1937 the purchase of capital goods took an abnormal leap. Retail consumption was rising slowly and consumer-goods purchases by retailers, wholesalers, and jobbers far outstripped it. Capital-goods orders rose because of the high level of consumer-goods orders and because these were rising sharply. Overbuying of consumer goods was caused partly by miscalculation of consumption by retailers, wholesalers, and jobbers and partly by fear that the Wagner Labor Act would result in retarding production and increasing prices. These fears with respect to labor were realized; then consumption fell off, and orders for capital goods dropped to the low point of 1932, from which they are now rising. This recovery is due in part to the reversal of policy by the Federal Reserve Board, but in the main it is attributable to the normal reaction of American business following the working down of excessive inventories.

Evidence is available to support the author's earlier theory of business cycles, which can be stated briefly as follows:

In the earlier work the demand per unit of time for a commodity was taken to be a functional[3] of the quantities of other commodities produced, of their prices, and also of the rate of change of these quantities. By using functionals instead of functions, theories are implicitly incorporated which rely on either cumulative effects of past factors, particularly so far as durable goods are concerned, or on the length of the production process. A cost function which depended upon the rate of production, prices of materials, and the rate of change in these quantities was used. If under these conditions entrepreneurs attempt to maximize profits over a period of time, the theoretical price and production curves as functions of time are of exponential or sine type, the particular function depending upon the interest rates and the magnitudes of various statistical coefficients.

The coefficients of the cost function can remain nearly constant while the various prices determining them are changing. Similarly the supply of money or credit in an economic system can remain adequate to meet the needs of the system

[3] A functional, or function of a line as it is sometimes called, is determined by all values of a curve, whereas a function is determined by values at a point. A definite integral is an example of a simple functional.

because of cancellation of price movements. As long as this cancellation is nearly complete, interest rates may be expected to be stationary; but, when increases of price or production, as a whole, seriously overbalance decreases of price or production, monetary troubles may be expected.

Tuesday, July 12—The Approximative Determination of Indifference Surfaces by Means of Engel Curves, ABRAHAM WALD, Research Fellow, Cowles Commission.

Let us denote consumers' goods by $G_1, G_2, \cdots, G_n$. A set of goods q consisting of q_1 units of the goods, G_1, q_2 units of the good $G_2, \cdots$, and q_n units of the good G_n can be represented by a point q in the n-dimensional Euclidean space with the coordinates $q_1, \cdots, q_n$. Each individual has a scale of preferences in this space of goods, that is, each individual can say of any two points in the space of goods whether he will prefer one to the other or whether they will be indifferent to him. The scale of preferences can be described by a real function $I(q)$ which gives for every set of goods q a real value $I(q)$ such that $I(q') = I(q'')$ if q' and q'' are two indifferent sets of goods, and $I(q') > I(q'')$ if q' is preferred to q''. Such a function is called an indicator.

Many indicators exist for a given scale of preferences. If $I(q)$ is an indicator then every monotonic function $F(I)$ of I is also an indicator. The sets of points q for which the indicator $I(q)$ has a constant value is called an indifference surface. It is obvious that the indifference surfaces do not depend on the choice of the indicator; they are uniquely determined by the scale of preferences.

It is of great importance to know the shape of the indifference surfaces. If we know the indifference surfaces we are able to solve many problems; we can determine the demand curves for consumers' goods at any price and income; we can calculate the index of cost of living, and so on.

The determination of the indifference surfaces should be made by means of observable data, which we have at our dis-

posal. Such observable data are: income, market prices, quantities purchased or consumed. The observable data do not suffice for an exact determination of the indifference surfaces, but their approximate determination is often possible. The indicator can in many cases be well approximated by a polynomial of the second degree, at least in the small part of the space of goods in which we are interested. We shall therefore make the assumption that the indicator can be approximated by a polynomial of the second degree.

By observing the prices, income, and quantities purchased or consumed, we are able to determine in each time period the Engel curve, that is to say, the distribution of income among consumers' goods. Since the indicator is a polynomial of the second degree, the Engel curves must be linear. It has been shown in this lecture that, if the Engel curves $C_1, \cdots, C_k$ in k different time periods are known, then the indicator $I(q)$ can be uniquely determined in the linear space S' determined by the straight lines $C_1, \cdots, C_k$. The formula for $I(q)$ has been given. If k is large enough we get the indicator in the whole space of goods.

It has been further shown that the Engel curves $C_1, \cdots, C_k$, if $k > 2$, must fulfill some integrability conditions. If the Engel curves do not fulfill these conditions, the hypothesis that the shape of the indifference surfaces remained unchanged during the whole time period of observation cannot be maintained. Hence in the case of $k > 2$ we have statistical criteria whether the hypothesis of the invariance of the indifference surfaces during the whole period is justified. H. Staehle made some applications of this theory and found that in some cases the above hypothesis was justified, but in some other cases it was to be rejected.

If we consider only two time periods 1 and 2 and the corresponding Engel curves C_1 and C_2, we are already able to solve the problem of the index of cost of living. The problem of the index is the following: We have to calculate for each given income E_1 in the first period the equivalent income E_2 in the second period. By means of the Engel curve C_1 we can determine the set of goods q_1 purchased by E_1. Since the indicator $I(q)$ is known in the linear space S' determined by the Engel curves C_1 and C_2, we can calculate the point q_2 of the

Engel curve C_2 for which $I(q_2) = I(q_1)$. The value E_2 of q_2 calculated with the prices of the second period gives us the income equivalent to E_1 and the index is given by E_2/E_1. If we make these calculations we get the same formula for the index, already demonstrated in another way.[1]

Wednesday, July 13—Price Dislocations Versus Investment, EMIL LEDERER, Professor of Economics, New School for Social Research.

The recent development in economic theory stressed investment as the pivotal point of the dynamic system. Though it is certainly one of the great merits of the Keynesian theory to have directed attention to the strategic position of investment, it led to an underestimate of the problem of relative prices. There are still economists, however, who stress the point that price and wage rigidities are responsible for the severity and length of depressions, and that therefore flexibility of prices and wages would soon create a new equilibrium from which a new prosperity could easily be attained.

This view, however, is too general; first of all we have to know what the concepts of rigidity and flexibility mean. Prices are rigid, if they do not respond to changes in costs and in demand. Three degrees of flexibility might be distinguished:

1. In the case where producers overestimate demand and have to market their whole output (perishable goods or an economic necessity to sell), prices will decline even below marginal prime costs. These are "momentary prices," which will either change rapidly in the next period of production (cobweb problem) or remain steady over a longer period, if the demand does not increase, while the producers have to maintain their output.

2. If prices decline to the level of marginal prime costs,

[1] See the writer's paper on index numbers in *Zeitschrift für Nationalökonomie*, Wien, 1937.

because demand shrinks, and if they decline further, in case marginal prime costs decrease, they are flexible—though it might well be that under these conditions the level of production could not be maintained. That will be the case if demand shrinks more than costs decline. Prices that behave in this way might be called short-run-equilibrium prices.

3. Prices are considered to be rigid if they do not decline as far as marginal prime costs, in case demand shrinks; if, however, they cover only those overhead costs which are part of current expenditures, such as interest on bonds and costs of replacement that cannot be postponed, they can still be called flexible; they might be called long-run-equilibrium prices, though such prices should cover overhead.

Monopoly or oligopoly prices are not less flexible than competitive prices, though they are higher than competitive prices would be in case it were possible to use the same production technique under conditions of perfect competition.

Flexibility of wages is difficult to define; they are certainly flexible in case they vary with the cost of living. Frequently, however, wages are called rigid in cases where they do not decline to a level at which all workers could be employed, in other words, in case they do not conform to the momentary situation.

Prices of agricultural commodities and of raw materials are "momentary prices"; those of manufactured commodities, produced under competition, are short-run-equilibrium prices; while prices of the most basic materials (bricks, steel, and so on) are long-run-equilibrium prices—especially where prices of basic materials are not perfectly competitive and where the demand during depression is inelastic.

A perfect flexibility of all prices and wages (decline to "momentary prices") would rather disrupt the economic system than help toward recovery. It is only a combination of flexibilities and of rigidities that can prepare the way for an early recovery.

Dangerous rigidities of prices and wages, if there are any, center in the producers'-goods industries. Are they of great importance? Some economists stress the point that they existed also during the nineteenth century, and that they did not prevent revival after short depressions. But this argu-

ment presupposes that the conditions for revival (apart from the price system) are identical with those prevailing during the nineteenth century. The difference compared with the nineteenth century is that the structural trend (determined by the strength of the dynamizing forces) weakened considerably; with a weak structural trend, that is, with few profitable investment opportunities, the rigidity of investment-goods prices will be detrimental, while it would only slightly retard an upswing, in the case where many new promising ventures were available.

To compensate for this weakness of dynamizing forces, public investments have been suggested. But they are not likely to lead to a real upswing unless they induce profitable investments in private business. In such a situation, prices of producers' goods and wages in basic industries must be relatively low in order to strengthen the weak incentives for investment. And therefore a public-works policy should be combined with a price policy, tending to create incentives for investments (possibly by subsidies); during the last prosperity period, however, public investments seem rather to have precipitated an upward movement of prices of producers' goods that probably contributed to the collapse in 1937.

Wednesday, July 13, Thursday, July 14—Full Employment and Easy Money—An Analysis of Mr. Keynes and His Critics, JOSEPH MAYER, Honorary Consultant in Sociology, Library of Congress.

The purpose of the present analysis is not to add just one more commentary on Keynes' *General Theory of Employment, Interest, and Money* to the many that have already appeared. The purpose is rather to appraise the positions of Keynes and his critics on the question of Full Employment and Easy Money in the light of considerations advanced by the speaker in this Conference two years ago.[1]

[1] "Pseudo-Scientific Economic Doctrine," *Abstracts of Papers Pre-*

One thing seems clear at the outset. Keynes and his critics appear to be at complete loggerheads as to what traditionalism or orthodoxy is and as to how Keynes' book is related thereto.

Rowse, Elliston, Ashton, Mussey, and Hardy regard the book as marking a "revolution" or a "break with accepted theory."

Henderson, Coe, Hansen, Viner, Franklin, and Taylor are not at all sure that such a break is implied.

Keynes himself thinks of the writing of his book as "a long struggle of escape . . . from habitual modes of thought and expression," which escape some critics feel has by no means been accomplished.

But even his most severe critics, like Schumpeter, agree with Ashton that Keynes has here courageously undertaken to bombard the citadel of classical doctrine, whether or not he has actually penetrated the stronghold.

In these lectures the problem of full employment and easy money will be examined under the following headings:

FIRST LECTURE

1. Keynes' Central Argument and Its Reception.
2. The Psychological Factors Involved.
3. Equilibrium Economics.
4. Income, Saving, and Investment.
5. Capital and Its Marginal Efficiency.

SECOND LECTURE

6. Money, Liquidity, and the Rate of Interest.
7. The Circulating Medium, Loan and Investment Funds, and Easy Money.
8. Conclusions.

The following excerpts are taken from the conclusions:

"The investment process in Keynes' theoretical world, as Schumpeter puts it, 'has hardly anything to do with the investment process in the actual world.' Let us list some of the divergencies:

sented at the Research Conference on Economics and Statistics Held by the Cowles Commission . . . July 6 to August 8, 1936, Colorado College Publication, General Series, No. 208, pp. 38-42.

"(1) Keynes' detailed analysis of 'investment' is apparently confined to 'the *purchase* of an asset,' old or new, an old asset such as a house, a machine, or a 'stock of finished or unfinished goods' (evidently exclusive of changes in price), or a new asset such as new capital equipment *purchased* 'out of income.'

"(2) Now, besides the *purchase* of capital assets, some are *created* directly out of income, possibly 25 per cent of aggregate savings being thus converted in the United States in 1929 (that is, without the savings flowing through investment channels). Such direct conversion of income into capital assets, while not intrinsically inconsistent with Keynes' definitions, seems to have been overlooked in his theoretical account.

"(3) In real life, moreover, purchases of assets (or their direct creation) are made from money income, whether productively or unproductively acquired, and not merely from productive income alone, as Keynes' definitions imply. The contingencies thus far considered might be comprehended in more realistic definitions of investment than Keynes provides, without there still being any fundamental divergence from 'saving.'

"(4) Beside these contingencies, however, productive investment may and often does occur as the result of loans extended but not liquidated in the current period; and, to the extent that this happens, *current* saving and *current* investment diverge.

"(5) In addition, part of current saving (as occurred in the United States in the latter 1920's) may be absorbed in bidding up the prices of old assets, and this part thus finds no counterpart in productive investment at all. Here is another, and a very important divergence, the use of savings unproductively. Half of aggregate savings in the United States were apparently thus unproductively absorbed in 1929.

"(6) Another part of current savings, besides those that may be unproductively absorbed, is held by income recipients in liquid form (because of 'liquidity preferences'). Keynes speaks of this part of saving as being 'invested' in liquidity, which may be a theoretically defensible extension of the concept, but it certainly stretches the meaning beyond that generally understood in analyzing the creation of new capital goods; and, in terms of additional current employment (the

main objective of Keynes' discussion), such extension of the concept 'investment' is of course meaningless.

"There are thus at least three important contingencies, not provided for in the traditional definitions, which cause divergencies between current investment in new capital goods and current savings: productive investment through loans extended but not liquidated in the current period; savings held in liquid money form; and savings unproductively absorbed in the speculative bidding up of the prices of old assets. The last-mentioned of these contingencies seems to be the most important factor overlooked by Keynes in his analysis, and apparently also by most of his critics, although the far-reaching effects of the first-mentioned contingency appear likewise to have been largely neglected. . . ."

"We may now return to Keynes' use of the equilibrium apparatus and present certain critical comments thereon. . . .

"In so far as Keynes' exposition reflects merely the mathematical approach developed by Harrod and Hicks, his equilibrium views must be regarded in terms of determinateness only. And it is from this angle that Harrod's 'statics' criticism takes on meaning, for thus construed Keynes' apparatus presents a cross-sectional view of the economic system, an instantaneous photograph, from which temporal, cyclical, and secular factors are omitted.

"But Keynes also treats of the economic system in flux. He has in mind shifting as well as stationary equilibria and speaks of any number of positions of underemployment balance. At one point he even talks about 'a cyclical movement' around an equilibrium position.

"Whether any of the equilibrium positions described by Keynes are stable is probably not so important, in the light of our preceding discussion, as whether they are obstructive or constructive. Hansen pictures certain conditions leading to an obstructive dead-center balance, that is, 'restrictive institutional factors and rigidities,' such as cost inflexibilities and monopolistic control of supplies, and he seems to suggest that Keynes' argument proceeds quite apart from such restrictions. But how about Keynes' psychological factors and the two per cent that John Bull 'cannot stand' — 'the ultimate causal forces resting in the mores, customs, habits, and be-

havior pattern of the people'? Are not these merely other forms of 'restrictive institutional rigidities'? To the extent that longstanding prejudicial customs and habits, often exceedingly irrational in character, obstruct the movement toward full employment, they would seem to be about as potent in bringing on underemployment positions of obstructive dead center as are cost rigidities and monopolistic controls.

"Whether Keynes holds an organismic view, as well as mechanistic and mathematical views of economic equilibrium, is not entirely clear, but this is also hardly important. It is quite clear that, in addition to other views, he holds and emphasizes the normative conception and that his central argument implies both stable and unstable positions of equilibrium at the same time. His argument suggests two conditions of balance, one constructive and normative and the other obstructive and mechanical, and a condition of freedom or unbalance. The constructive balance is represented in the implied necessity, through social control, of maintaining a certain moving equilibrium between consumption and productive investment. The obstructive balance is represented in various situations of dead-center underemployment equilibrium. The freedom or unbalance applies to any condition of employment short of full employment. From the point of view of stability or instability, the three conceptions seem to be combined by Keynes somewhat as follows: The deliberate maintenance of a moving balance between consumption and productive investment will prevent haphazard dead-center positions of underemployment equilibrium and will leave the economic system free to move forward constructively towards a final position of full employment. Unless the first condition of a deliberate moving balance is maintained, underemployment equilibrium sets in and the advance towards full employment stops, the industrial machine being in the position of an automobile mired in a mud hole, the machine and its spinning wheels being held in obstructive balance because of the lack of traction in the mud."

Thursday, July 14—Some Statistical Measurements of Labour Mobility in England, R. G. D. ALLEN, Lecturer in Statistics, University of London, at London School of Economics.

Taking a long-period view of labour mobility, we can distinguish between occupational (or industrial) transference of workers and geographical movement. Two main sources of information have been used to indicate the extent of such movements — the Census of Population and the yearly data on "insured" workers published by the Ministry of Labour. An interesting method of estimating both the direction and the extent of movement of workers into a specific area (e.g., London) has been worked out by Brinley Thomas (cf. articles in *Economica,* 1934, 1937, and 1938). The method takes the number of "unemployment books" exchanged each July in the area and determines the offices, all over the country, at which the books were originally issued.

In a short-period approach, we concentrate on the ever-changing, day-to-day demands of employers for workers of various kinds and determine how the supply of labour is adjusted to meet the demands. The recruitment of workers may or may not involve occupational transference or geographical movement. Many questions arise — what are the methods of recruitment adopted by public and private employers? — is the labour supply insufficient and, if so, how do employers modify their demands? — if labour is available, how much delay is there in placing workers and what are the causes of delay? Lack of statistical material has held up investigation of these problems. But the records of the placing service of the Ministry of Labour now provide abundant information on that part of the recruitment of labour taking place through the offices of the Ministry and an investigation of these records has recently been undertaken for the London area. Some results are available for two trades (Building and Catering) where there is a large turnover of labour.

For Building Trade workers recruited through offices of the Ministry of Labour in certain areas in London, in June 1936 nearly 20 per cent of all workers finding jobs were placed after an interval of at least 2 days from the time the

employer notified his needs. Long delays, however, were infrequent, intervals of a week or more occurring in only 1.5 per cent (in inner London) and 3.4 per cent (in outer London) of all placings. For skilled craftsmen, delays in placing (of at least 2 days) occurred in about 30 per cent of the cases and many of the workers (over 50 per cent in inner London) were drawn from some distance to work in the areas concerned. For example, 17 per cent of all skilled workers placed in the important inner London districts came over 5 miles to work.

Much greater delays were involved in placing workers in the Hotel and Catering trades. In June and July, 1936, of all house-workers (e.g., chambermaids, porters) recruited through the Ministry of Labour for establishments in central London, over 70 per cent were placed after 2 or more days, 35 per cent after at least a week, and 13 per cent after 2 or more weeks. Many of these delays are "nominal," e.g., involving the giving of notices or the taking up of references. But, when all allowances are made, we find that serious delays occurred in the placing of about half of all resident, and of about one-third of all nonresident, women house-workers. Amongst skilled workers (e.g., cooks), many delays were apparently the result of low wages offered by the employer, or of long hours and poor conditions of work.

Friday, July 15—Conditions for the Success of a Pump-Priming Policy, MARGARET F. W. JOSEPH, Research Associate, National Economic Institute, London.

The capitalist system appears to have reached a structural crisis, as can be seen particularly in its failure to recover from cyclical depressions. This has led to a concentration of attention on business-cycle policy, and to the treatment of various branches of economics, hitherto studied in separate watertight compartments, from this general point of view. Such a co-ordination of the different fields of economics offers

increased possibilities of mitigating or controlling the cycle. The point of view of business-cycle policy is essentially a monetary one, and has involved a broadening of both the objectives and scope of monetary policy. With regard to the former there has been a progressive reorientation from the traditional objective of merely regulating the external value of the currency to that of attempting to control the internal business situation. These two goals are to some extent mutually incompatible. Until the 1931 financial crisis, most monetary authorities regarded the gold value of the currency as sacred—internal credit conditions had to be subordinated to its maintenance. Since the great depression, the emphasis has been shifted, particularly by certain English and Scandinavian economists, away from external towards internal monetary policy. The importance of foreign trade and long-term investment has declined, largely owing to international political tensions, and the necessity of creating domestic employment has become the central problem.

In attacking this problem the scope of monetary policy has had to be extended; the traditional instruments, the discount rate and open-market operations, have proved insufficient. These instruments, by improving the terms on which funds could be borrowed, were directed towards stimulating a revival of private investment. But nothing short of a negative rate of interest could induce new investment during the cumulative deflation and shrinkage of incomes characteristic of the depression phase of the cycle. It therefore becomes necessary to act directly on the income stream. For this purpose an effective instrument is found in the national budget. The government, unlike private investors, is in a position to borrow independently of expected returns.

To succeed in its purpose of increasing income, it is essential that government borrowing should not take place at the expense of private investment. The two main obstacles to the fulfillment of this condition are: (1) a rise in the rate of interest, and (2) psychological factors affecting confidence. A rise in interest rates may be prevented by monetary policy designed to increase commercial-bank reserves. The maintenance of confidence must depend on the level of education of business men and their understanding of business-cycle policy.

Government borrowing may arise in two ways: either from a reduction in receipts or from an increase in expenditure. The deficit will have both primary and secondary effects on the flow of income, and their extent will depend on the nature of the taxes remitted and of the objectives of government expenditure. With regard to primary influences on consumers' demand, not only the effect on total income but also that on its distribution will affect the flow of spending. Saving increases in higher income classes, so that tax remission will be more stimulating the less progressive the taxes remitted, and increased expenditure will be more effective the poorer the recipients. The secondary effects of a government spending policy depend on the degree to which it stimulates private investment.

Pump priming has to serve two types of purposes: to check the secondary effects of cyclical depressions, as well as to attack their causes. For the first purpose comparatively small expenditures should be sufficient. They can best be provided in two ways: by anti-cyclical timing of normal public investment, and, where this is not enough, by *ad hoc* expenditures of a highly flexible kind such as unemployment relief. In addition to this, however, a progressive increase in government investment may be necessary to compensate for the tendency of the marginal inducement to invest to lag behind the propensity to save in a progressive economy.

Corresponding to a deficit policy during depressions, a policy of debt repayment may be applied to check unhealthily rapid expansion during the boom. If the situation is such that the necessity for increasing debt during depressions is greater than that for repayment during booms, the problem of an increasing trend in the national debt may arise. This problem turns out to be a somewhat artificial one. (1) An internal debt involves at most a transfer of income from one class to another. (2) The distribution of existing capital between private and public investments has no particular economic significance. (3) Only the increase in government debt, not its size, might involve competition for funds with private industry; but, since the policy is advocated only for periods where the private demand for funds is low, this problem does not arise.

Certain changes in the form of presentation of the bud-

get, such as its separation into a capital and income budget, may serve to mitigate the disturbing effects on confidence of a high national-debt figure.

It is proposed that the method by which government expenditures are financed, whether by taxation or by loans, should be determined mainly from the standpoint of business-cycle policy, with a view to promoting the fullest possible utilization of existing productive resources; and debt should be repaid, not on grounds of moral fetish, but only at times when the business situation calls for restraining action.

Friday, July 15—An Autopsy and Diagnosis of Failing and Non-Failing National Banks, HORACE SECRIST, Professor of Economics and Statistics, Northwestern University.

This lecture is concerned with the methodology used in and the results secured from a study of the life histories from 1921 to the date of failure of 741 individual national banks failing in 1925, 1929, 1930, 1931, and 1932, and of 111 national banks from 1921 to 1932 which did not fail.[1]

The basic assumption underlying the procedure in the analysis of the balance-sheet data of the banks in question was that symptoms of relative strength could be discovered and their significance determined by a comparative analysis of failures against failures and of failures against non-failures. Symptoms were first assumed and their validity then determined by observing what occurred to the institutions under examination. The test of the accuracy of the diagnosis and autopsy were found in the repetitive nature of the differences which the comparisons disclosed, standards of reference for the behavior of banks failing in given years being that of those deferring failure and of those which survived. A series of controls was thus established, comparative operating conditions being the criteria which were used to determine relative

[1] See Horace Secrist, *National Bank Failures and Non-Failures—An Autopsy and Diagnosis*, Principia Press, Bloomington, Ind., 1938.

strength or weakness, and to indicate the time when failure appeared immiment.

The banks were selected in keeping with the following conditions: (1) they must have been chartered before 1921; (2) annual balance-sheet data for them individually must be available from 1921 to the year immediately before failure, or to 1932 in the case of the non-failing institutions; and (3) each bank must retain its identity throughout the periods to which the data relate. These conditions were applied both to those which "died" and to those which "survived." The first group covered the entire country; the second was drawn at random from the Seventh and Tenth Federal Reserve Districts.

The basic balance-sheet amounts and ratios derived from them were comparatively analyzed, the objective being to discover through time systematic and repetitive differences which were independent of the relative size and location of the institutions. The comparisons were of the following order for the years after 1920:

Failures Occurring in	Banks Serving As Controls								
1932	Non-Failures								
1931	"	"	Failures 1932						
1930	"	"	"	"	Failures 1931				
1929	"	"	"	"	"	"	Failures 1930		
1925	"	"	"	"	"	"	"	"	Failures 1929

Only when consistency of behavior in terms of the several reference criteria was secured was significance assigned to the results. The method of analysis was one of trial and error, a priori standards being rejected for those which were affirmatively determined. Those found were not conceived as absolutes. Neither the assumptions nor the conclusions took this form. Differences of a quantitative type were found, but the assignment of them with the fact and time of failure, or with survival, was possible only because of knowledge after the fact. The prognosis was conditioned by the diagnosis. Some banks failed while others remained solvent; moreover, failures occurred at different times. Knowing this, one seemed justified in assigning significance to the differences which were found, ex post facto, to be important. Such a method of establishing standards likely to lead to relative stability appeared to be superior to one which assumed them a priori.

The conclusions related to the comparative behavior through time of the following balance-sheet items, first, as aggregates for the several classes of banks, and second, as amounts for each member of each class: (1) Total Resources, (2) Total Deposits, (3) Total Loans and Discounts, (4) Total Loans and Discounts and Other Bonds, and (5) Total Capital Funds. Following the discussion of the short- and long-time trends of these amounts treated singly and together, for failures and non-failures variously classified as to location and size, the conclusions pertained to the comparative levels and trends of the following ratios: (1) Loans and Discounts to Total Deposits, (2) Total Deposits to Total Resources, (3) Total Capital Funds to Total Deposits, and (4) Total Capital Funds to Total Liabilities.

After specifying some of the significant specific conclusions, the speaker commented upon the more general aspects of his findings. Continuing studies of the type here reported on but extending also to income and profit-and-loss data found in the office of the Comptroller of the Currency are badly needed. Why cannot they be made? If they were, the misleading and often false standards which are too generally held to distinguish sound from unsound banking would soon be replaced by something more scientific and dependable.

Bank failures are peculiarly an American phenomenon—a disgrace. Cannot the conditions which make them possible be diagnosed and our banking system be placed on a sound basis, as it should be? This study and the methods utilized may in some measure indicate what is possible in this respect.

Monday, July 18—The Goal of Lasting Prosperity, RALPH J. WATKINS, Director, Bureau of Business Research, University of Pittsburgh.*

For the past two decades we have heard much of such

* For copy of complete lecture with charts, order Document No. 1141 from American Documentation Institute, care Offices of Science Service, 2101 Constitution Avenue, Washington, D.C., remitting 43 cents for copy in microfilm, or $2.50 for photoprints readable without optical aid.

phrases as "lasting prosperity," "sustained recovery," and "continued employment." Critics of the business-enterprise system have condemned that system because it has not yielded lasting prosperity, and its defenders have charged these failures against interferences with its normal operations. Explicitly stated or implicit in the argument is the assumption that, given certain conditions, our business-enterprise system can be expected to achieve permanent prosperity. Let us turn to the statistical annals.

The record I employ is the monthly index of industrial production in the Pittsburgh district, January, 1884 to June, 1938, computed and published by the Bureau of Business Research, University of Pittsburgh. This index, prepared under the supervision of my colleague, Mr. Wilbert G. Fritz, is a weighted composite of manufacturing and mining production, and is adjusted for seasonal variation only.

The charted index records an endless process of change. The volume of production almost always is moving up or moving down. Change is the norm, not evenness of level. Slopes and sharp peaks and V-shaped valleys are the figures of speech, not plains, plateaus, and level valleys.

I find with this index 23 rather clearly defined periods of declining or depressed business and 22 periods of rising or prosperous business. My purpose was to divide this 654-month span of time into easily recognizable periods of more or less continuous recovery and prosperity on the one hand and similar periods of recession and depression on the other hand. Personal judgment necessarily entered in the process. The 23 periods of declining business aggregate 233 months, or 35.7 per cent of the total, and the 22 periods of rising or prosperous business aggregate 420 months, or 64.3 per cent.

Of the 23 declines, 11 lasted from 2 to 6 months, the extent of the drops ranging from 14 per cent to 42 per cent. Seven of the declines lasted from 8 to 12 months, in extent ranging from 20 per cent to 53 per cent. Five of the declines lasted more than a year each, ranging in duration from 14 months to 44 months and in severity from 25 per cent to 71 per cent. The 1929-33 decline is the longest on record as well as the most severe. Only seven of the 22 rises lasted 6 months or less, the magnitude of the rise ranging from 30 per cent to

117 per cent. All the other 15 rises lasted more than a year, 8 lasting from 14 months to 24 months and ranging in extent from 27 per cent to 129 per cent; and 7 lasting from 27 months to 44 months, ranging in extent from 67 per cent to 165 per cent. In magnitude the greatest rise is that from September, 1934 to March, 1937. The longest expansion period on record is that from September, 1896 to May, 1900.

A business-enterprise system implies a considerable measure of economic freedom, involving countless "free acts" or unco-ordinated acts. Even on an a priori basis we could hardly expect a composite straight line of development. From what we know concerning past fluctuations in business activity and from what we know about the operation of a business-enterprise system, I believe we may say that there is presumptive evidence at least that business fluctuations are inherent in a business-enterprise system and that these fluctuations, bringing their succession of recovery, prosperity, recession, and depression, will continue so long as we have this system.

My policy suggestion is that we place chief emphasis on mitigating the consequences of economic fluctuations. Naturally, I would not suggest that we cease our search for specific causes of instability or that we cease our efforts at removal of those causes after their discovery—if they are of such a nature as to be within our reach. But let our near-term goals, at least, be more modest. If we are equipped with social and economic organization for ameliorating the effects of depression, then depression will cease to be a major social tragedy.

We can predict, with some confidence, that each cycle of prosperity and depression is likely to see a chain of interventions by government calculated to mitigate some of the consequences of these fluctuations as well as to eliminate some of their more glaring causes. If those interventions can be made to conform to the logic of an enterprise system and directed chiefly toward a mitigation of the consequences of economic fluctuations, then in the long run they may produce a more stable society in which both private enterprise and public enterprise will find adjustment within a moving equilibrium. Let our aspirations remain high, but let us base our policies and our promises on what may reasonably be expected. Perhaps in that way lies surer progress.

Monday, July 18, Wednesday, July 20—Patterns of Family Expenditure: The Effect of Social Level and Family Composition, MR. ALLEN.

For a family with given preferences, the pattern of expenditure is described by a set of expenditure functions showing the effect on various items of expenditure of changes in the income of the family, in the prices of consumers' goods, and in the size and composition of the family. A social group consists of families with varying individual preferences but with certain habits of consumption in common. The problem is to define a pattern of expenditure characteristic of the social group, representing the common habits of consumption, and to describe the individual variations about the characteristic pattern. A method is suggested for deriving characteristic patterns of expenditure for social groups represented in a collection of family budgets. Since prices are assumed constant, the patterns relate to the factors of income and family composition only. A complete social group is divided into subgroups of families of given size and composition. Scatter diagrams of expenditure and income are obtained for each subgroup, and expenditure relations, connecting expenditures on various items with income, are fitted. The expenditure relations for different subgroups of families (of varying composition) can be compared and suffice to describe the pattern of expenditure characteristic of the whole social group. The variation of individual family preferences is shown by the distributions of residual expenditures taken from the expenditure relations. Particularly convenient results are obtained when the expenditure relations can be taken in linear form (cf. Allen and Bowley, *Family Expenditure*, 1935).

The method suggested is tested with the aid of a collection of family budgets made by the Urban Study of Consumers' Purchases in a number of U.S. cities in 1935-36. Expenditure patterns, of linear form, are derived to show the effect of income and family composition for groups of wage-earner, clerical, and professional families in Chicago. It is found that "personal" items of expenditure such as clothing, recreation, and personal care vary almost in proportion to income, social levels and family size having only minor effects on these expen-

ditures. For other items of expenditure, the social grouping and the composition of the family have equally important influences on the modes of expenditure. This is true both of the "urgent" items of expenditure (food, housing, and fuel) and of net savings and the "luxury" items (such as transportation and furnishings). For example, housing expense is a most urgent item for clerical families of husband and wife only; expenditure is high at low incomes and increases slowly as income increases. The expenditure is much less urgent for similar wage-earner families and for clerical families with one or more children; the expenditure on housing here is smaller at low incomes and more responsive to increases in income. In conclusion, the Chicago material is used to throw light on the validity of the method of "equivalent adult" scales, commonly used to eliminate the effect of varying family composition on expenditure. The available evidence suggests that the method is of very doubtful general applicability.

Tuesday, July 19—A Cost Theory of the Trade Cycle, UGO PAPI, Professor of Economics, University of Rome.

In a country without banks, an upward movement in trade after a period of depression can be initiated only as the result of an event which arouses the hope of increased profit, there being at the same time sufficient saving to provide for a first application of productive factors.

In the case of a manufacturer's production being below the demand this error may be corrected by successive expansions. Each expansion of production will lead to an increase in certain incomes and the expenditure of this increase will tend to raise the price of many commodities.

But the adjustment of the supply to that particular quantity of goods which can be profitably produced, together with a continuous equilibrium between production and consumption, is never actually attained, since, in the expansion of activities, the cost finally exceeds the price in the endeavor to attain a

satisfactory ratio between the two. The increase in costs, more rapid than that in prices, is determined by factors which are unpredictable, some being due to the action or negligence of the entrepreneur and others to the action or negligence of those connected with him in regard to instrumental, complementary, or substitutional goods. Numerous errors may occur when a manufacturer attempts to estimate, even approximately, the cost of an increase in production in the near future.

These errors arise in the following manner: In the light of his past experience the manufacturer's calculation of the cost of his output is on a percentage basis. No distinction is made between expenses which — within certain limits of production—do not vary and are therefore in inverse ratio to the output, and those which vary in direct ratio. For this reason the estimation of the various items of expenditure on the basis of unvarying percentages is liable to fall short of the reality.

The quantity of goods manufactured and sold is usually below the estimate for a plant as producing up to its full capacity, while the expenditure is the same as if the output coincided with the estimate.

If such defects, which are hardly perceptible even to experienced and prudent manufacturers, should continue through several productive processes, the factory will probably incur heavy expenses not included in the estimate, even though no additional outlay be necessary in respect of premises, improvements, and publicity.

Once the maximum output has been attained an enlargement of plant is necessary for any further increase, no matter how insignificant, and this cannot be effected without incurring heavy expenses.

In addition to delay in the consignment of the various parts requisite for the finished product, there will now be increased obstacles, since the new factors of production will not harmonize with the original elements to such an extent as to be entirely equal to the demands made upon them. It follows, then, that the actual output of the new plant will be considerably less than double the percentage of utilization attained by the original plant. A factory, for instance, with a capacity of 100 and a waste of 25 may be enlarged until its capacity is

200; the waste will not only increase from 25 to 50 or, in other words, will not remain in the proportion of 25 per cent, but will increase to 30 or 32 per cent.

Inductive enquiry shows that producers' goods increasingly remote from the finished product are required in modern manufacturing processes. Many stages must be passed before a motor, an aeroplane, or a cannon can be produced. An increased demand for these finished goods naturally leads in the first place to a shortage of producers' goods. But in the course of time the supply of these goods tends to become excessive as each firm is prompted to avoid a possible shortage.

It is impossible to anticipate the moment when the temporary surplus of the more remote instrumental goods will be absorbed by the market so that the demand of the firms responsible for the finished product will revive. The cost of production, therefore, will finally increase also for those who produce consumers' goods.

The estimate of costs is not only complicated but arbitrary. The entrepreneur of the vast concerns which are so numerous in these days continually reallocates the burden of the total costs, removing it from one special product to another according as the selling conditions are more or less favorable to him, and also according to the continual modification in the supply of the various goods or services produced by him. The proceeds of the sales decide the proportion of the expenditure to be assigned to the various products. It is obvious that these proceeds bear no relation to the price of the factors of production. The difficulty of estimating them in advance is therefore all the greater.

Should the manufacturer desire to go a step further and estimate the *selling price* of the finished goods he will meet fresh difficulties, since, when his output finally reaches the market, the price may fall to an extent which cannot be foreseen.

Finally, if the demand for consumers' goods should vary during the process of production, the manufacturers would incur a loss owing to the reduction in price which would result. *None of these elements which curtail profits can be accurately determined in advance.* It follows, then, that the manufacturer who gradually increases his output is, con-

sciously or unconsciously, falsifying his own estimates of expenses and profits.

The errors tend not only to increase in magnitude as the expectation of profit becomes more certain, but also to appear *in the same direction*:

(A) In consequence of the favorable event the entrepreneurs of many productive branches of industry, seeing the possibility of increased profits, devote themselves *all at the same time* to increasing their production.

(B) The expansion itself, nearly always chaotic in the general eagerness to take advantage of the favorable moment, gives rise to increased costs which neutralize the economies, both internal and external, to an extent which cannot be foreseen by the entrepreneurs.

(C) These increases, presenting themselves as errors of underestimation of costs on the part of the entrepreneurs, cannot be regarded as *accidental* errors and therefore as positive no less than negative. On the contrary, they are linked to that *unity* of direction which, in all the undertakings on the market, leads to the expansion of production with a view to profit, subsequent to the favorable event. They are, therefore, undoubtedly errors, but in one sole direction. Hence it will be seen that the mass of errors which are almost inevitable, and begin at a point not easily determined, result in a more rapid increase in the *cost of production* than in the selling price.

The industrial fluctuation may be defined, therefore, as the development of a series of errors in production, first on the defective and later on the excessive side, which have their origin in an event favorable to production. The more extended fluctuation may also be regarded as due to the errors of firms which fail to grasp immediately the magnitude, duration, and overlapping of such events as are favorable or unfavorable to a country's production.

Tuesday, July 19—The Measurement of Incremental Revenue and Cost, THEODORE O. YNTEMA, Professor of Statistics, University of Chicago.

One unit of economic theory which merits more study than it has received is that of the principles which guide the pricing and quantity of output of the individual firm. Advertising, for example, is not satisfactorily treated in received theory.

We must study the reaction of business men to the data on revenue and cost which they possess. Usually they have data on total revenue and cost, and average revenue and cost, for one output point, or at best comparable figures for a very few widely scattered outputs. Incremental revenue and cost figures are not easy to obtain. They are especially difficult of estimation when advertising is involved.

Average costs are useful, and are used by business men, with varying degrees of intelligence. Some, if not most, feel almost immoral if they sell at a price below average costs. Others make rather good estimates of marginal cost from previous movements of average cost. Still others will go so far as to recognize that the maximum-profit price covers marginal cost, and then ignore many truly direct costs in computing this margin.

If p and q are price and quantity, respectively, S is total outlay on "selling" (as distinct from "producing") costs, and η is the elasticity of demand for the individual producer's output, a necessary condition for maximization of his profit is:

$$\frac{p}{-\eta} = \frac{dS}{dq}.$$

Three possible attacks on the problem of measuring incremental "production" costs suggest themselves.

(1) We might study the components of cost, try to estimate for each component its marginal cost, and add.

(2) We might make a time-series analysis of costs, correlating historical costs with other variables.

(3) Where several plants or offices are available for analysis, we might correlate costs at any one time with size of plant, or other variables.

Of these three possibilities the first, although "rough and ready," is frequently avilable when (2) and (3) are not, due

to inadequacy of data. The best example of the second approach is probably a study made at the University of Chicago by Joel Dean, who analyzed the effect of size of order, labor turnover, quality of material, and other variables upon historical costs, for a furniture concern for which management and equipment were substantially constant.

It has been possible to try out the third method of analysis in the case of a personal-finance company which makes small loans from offices operated along almost identical lines in more than 100 cities. Here we have an opportunity to observe the changes in marginal cost associated with changes in the size of the operating unit, yet little variation is found. Such differences as have been observed show that the long-run marginal costs are somewhat higher, because of limitations on specialization, for the smaller units.

The evidence for these same concerns is that the short-run marginal costs are still lower, and do not turn up within the observed range of output. How then can a stable equilibrium be reached? The answer is that while these marginal costs of "producing" remain substantially constant, the marginal cost of "selling" (i.e., advertising expense) rises rapidly, and is much above the average selling expense, thus making possible a stable equilibrium for the individual firm.

As more data become available, we shall perhaps find many enterprises where the marginal cost of producing is constantly falling, but where stable equilibrium results, as a consequence of a more than offsetting increase in the marginal cost of advertising.

In estimating the marginal cost of advertising it is of course necessary to know what increase in sales was due to a given advertising appropriation. Advertising displayed on a single day may increase output for several months.

The time interval is also important to the small-loan company when it is trying to determine the optimum price or interest rate which it should charge. A reduction in this rate will not be reflected immediately in any increase in loans requested, but there will be a gradual increase in the volume of these sales, and profit possibilities are further strengthened by reduced fear of legislative action restricting or prohibiting the concern's activities.

Finally, one must be impressed by the multiplicity of margins the entrepreneur needs to measure. Their study is an important economic exploration.

Wednesday, July 20—The Structure and Nature of Business Cycles, F. D. NEWBURY, Economist, Westinghouse Electric & Manufacturing Company.

Two major ideas are put forward: first, that business and industry cycles, as usually presented, are actually complex combinations of several cyclical components of quite different frequencies (in addition to the commonly recognized 12-month or seasonal component cycle); and second, the suggestion is made that the two major groups of component cycles can be usefully associated with the two major divisions of business activity for forecasting business conditions.

The two groups of component cycles are, first, those relatively short-wave components of from 20 months to 28 months in cycles of short-lived consumption goods and of 40 months average length in durable-goods industries; and, second, those relatively long-wave components of from 6 to 10 years in length in general industry, and up to 20 years in length in residential and commercial building construction.

The two kinds of business activity are, first *current operations* (both production and consumption) which are financed from current income and which affect current profit and loss situations; and second, *capital operations* which are financed from capital funds.

The suggestion is made that current operations are associated with and fluctuate with the short component cycles. These fluctuations are fairly regular in timing and amplitude and are predictable to a useful extent (since 1850).

Capital operations are associated with and fluctuate with the long component cycles. These fluctuations are irregular both in timing and amplitude and are governed by the conditions that govern long-term expenditures.

Under present conditions any analysis of prospective business conditions must take into account effects of Federal Government cash expenditures in excess of cash receipts. The rise and fall in these net expenditures since 1931 to March, 1938, and the prospective increase over the next 12 months, constitute the largest single factor in business fluctuations during these years. These Federal net expenditures partly affect current operations and the short cycles (W.P.A., for example); they also affect capital operations and the long cycles (public works, housing, roads, flood control, etc.).

Considerable statistical evidence is presented of the existence and magnitude of these two groups of component cycles in industry and of the comparative regularity of the dominant 40-month component cycle in general industry.

The idea that these component cycles are related to current and capital operations is speculative and requires additional statistical study which is under way. The idea is presented at this Conference to invite constructive criticism.

Thursday, July 21—The Effect of Wage Rates on Business Activity—Some Statistical Indications, ELMER J. WORKING, Associate Professor of Agricultural Economics, University of Illinois.

Over a period of years there has been developed a considerable body of thought indicating that the primary cause of depressions is underconsumption or oversaving. Underconsumption and oversaving have been especially stressed as the causes of the great depression which began in the latter part of 1929 and of the current one which started about a year ago. There are a number of variants of the theory, but the argument usually indicates that the productive capacity of the United States has exceeded consumption because of failure of the purchasing power of consumers to keep pace with the growth of production and capital equipment. From this it is said to follow that people with large incomes have had too

much money to invest and people with small incomes have had too little to spend.

On the other hand, the events of the latter part of 1936 and the early part of 1937 are such as to suggest that the current depression may have been brought on by the rise of wage rates. In view of the conflict of these two lines of thought, the need to turn to whatever factual data are available and are pertinent to the question is evident.

From available indexes of wage rates and commodity prices it is possible to construct wage-price ratios for a period of nearly 140 years. These ratios have a marked upward trend which is due to the increasing effectiveness of labor. This in turn is presumably due on the one hand to the improvement in the knowledge of the arts of production and on the other hand to the increasing amount of capital which has been combined with labor in the production process.

The deviations of the wage-price ratio from its trend presumably give a rough indication of the periods during which and the extent to which wage rates are abnormally high or abnormally low relative to commodity prices. The cause of their being high or low has usually been the comparative inflexibility of wage rates relative to commodity prices, but at some times it appears to result from a change which is initiated in wage rates.

When the wage-price ratio is compared with the course of business activity it appears that at least since the Civil War there has been a tendency for depressions to be prolonged and severe in the periods when the wage-price ratio has been rising or high relative to its trend. On the other hand, in years when the wage-price ratio has been declining or low relative to its trend, such depressions as have occurred have usually been short-lived.

Although this suggests a significant relationship of real wages or the wage-price ratio to business activity it is necessary to explain how it can be that we can have periods of prosperity (such as that of the 1920's) at times when the wage-price ratio is high and increasing relative to its trend.

The reason—or at least a fundamental reason—is to be found in the fact that when wage rates are increased relative to the value of the product it pays to use methods of production

which are "labor saving." The necessity of increasing the amount of capital per worker and of having it in new forms creates a sort of "speculative" demand for the production of new capital goods and hence an increase in wage rates may for a time increase the demand for labor. Employment may even increase for a time in spite of the higher wage rates. But after a considerable addition to the supply of new capital goods has been made, production and employment may be expected to fall off, resulting in a period of severe depression unless downward readjustments are made in wages relative to commodity prices.

Many questions concerning and ramifications of the foregoing analysis are extremely interesting and significant, but the time available does not permit a discussion of them. I wish, however, to stress the fact that the foregoing is not presented as a theory of business fluctuations. Disparities between wage rates and commodity prices constitute only one type—though a very important one—of price disparity which is causally significant to business fluctuations. Furthermore there are many other factors, some of which are fairly independent of price relationships and some of which are causally important to the price relationships. All these need to be considered in any complete understanding of the dynamics of our economic system.

If the foregoing reasoning is correct, however, it follows that the theories which have been developed by economists and other people indicating that the fundamental cause of depressions is the failure of real wage rates to keep pace with productivity are a tragic error. They are a tragic error because they have contributed to the action of labor leaders and governments to maintain wage rates at artificially high levels or even to increase them when the opposite was needed in order to lay the foundation for a lasting prosperity. Note, however, that I speak in terms of wage *rates*, not wage *payments*. Lower wage rates would contribute to greater employment and larger wage payments.

Thursday, July 21—On the Occasion of the Cournot Centenary: The Economic Work of Augustin Cournot, RENÉ ROY, Professor of Mathematical Economics, University of Paris.

The work of Cournot in Economics consists of three books: *Recherches sur les principes mathématiques de la théories des richesses* (1838); *Principes de la théorie des richesses* (1863); *Revue sommaire des doctrines économiques* (1877). He set himself the goal of applying to economic questions the methods which had already proved themselves useful in other sciences. He can hence be called the founder of scientific economics. Although these books were published at considerable intervals, he kept himself all the time well-informed about economic matters and his knowledge in this field was immense.

In the preface to the *Recherches*, he pointed out that to obtain precision in economic thought it was essential to use not only elementary algebra but also the branch of analysis which deals with arbitrary functions subjected only to certain conditions. The *Recherches* contains the essential part of the economic investigations of Cournot, and has been the basis of the later work of the mathematical school in economics. Hence it is sufficient to point out the really characteristic passages of this work in order to show the importance of the contribution of its author to the progress of scientific economics.

1. The Law of Demand. In approaching the study of demand, which is the basis of all the further developments, Cournot takes great care to state the principles upon which his investigations are based: "We shall use only a single axiom, . . . that is to say, that everyone tries to get from his goods or from his labor the greatest possible value." This is substantially the hedonistic principle which has since been employed by the classical masters of mathematical economics, Walras and Pareto. Cournot continues: "Let us admit that the sale or annual demand D is, for every commodity, a particular function $F(p)$ of the price p of this commodity. If we knew the form of this function we should know the law of demand or of sale." He showed that this function must have continuity and negative slope, and made use of it in general mathematical reasoning

without specifying for it any exact algebraic form. He anticipated the work of his successors, however, by pointing out that the law of demand in special cases should be determined statistically by observations, from which an empirical formula or curve could be constructed. Cournot was not able to determine the quantitative elements from observations, but he gave very substantial indications of the form of the law of demand for different types of commodities, and presented, without giving it a name, what was essentially Marshall's concept of the coefficient of elasticity of demand.

He also introduced the idea of the total value of a commodity, the product of the price p by the corresponding demand, $F(p)$. From this, with the aid of purely mathematical reasoning, based upon the idea of continuity, and upon the existence of a finite limiting price and finite limiting demand, Cournot demonstrated the existence of an optimum price corresponding to the maximum of total value. He suggested statistical determination of this optimum price for different commodities. He pointed out that economic problems affecting a commodity differed greatly according as its current price was below or above this optimum price.

Cournot considered only the price of the commodity in question; he looked on this procedure as only a simple first approximation. The later introduction of the interdependence of prices goes further, but Walras and Pareto who used them nevertheless recognized the advantages of the concept to which Cournot had restricted himself.

2. Monopoly. Cournot studied price formation under monopoly because it was the simplest hypothesis, and served as an introduction to the more complicated case of competition. The monopolist tries to maximize the monopoly profit, gross receipts less cost of production. Cournot considered two cases: one where the cost of production could be considered zero or independent of the quantity produced, the other where the cost of production was taken as a function of the quantity produced. In the second case he introduced the marginal cost of production, though not by that name,

It is necessary even today in applying taxes to monopolized services to come back, more or less consciously, to the principles first established by Cournot and later developed by

Dupuit. Hence the work of Cournot and his immediate followers still has a very up-to-date importance.

Cournot's modifications of monopoly theory to fit the case of several producers was not so successful from the modern point of view, which tends to develop distinct theories for the two cases. In our opinion, the position of Cournot can only be justified by the extremely abstract character of his mind and by the entirely geometric nature of his reasoning.

3. Foreign Exchange. The chapter on this subject in the 1838 volume is very exceptional. This short exposition, even more than the law of demand, after a century still retains all its importance; and we may even say that no notable change has taken place in our ideas on these matters.

He made only the assumption of a universal metallic standard, in which the ratio of every monetary unit to a gram of the monetary metal is invariably fixed. He then considered all the quantities which appear in the balance of payments of two countries, and showed how the settlement of this balance determines the small variations in exchange which induce transfers of metal only when they exceed a certain limit: "Always when the ratio $m_{2,1}/m_{1,2}$ differs from unity by not more than the cost of shipping one monetary unit from one place to the other, then the balance between the two places will be adjusted without any actual shipment of money, and only by the exchange value." Does not this formulate in very clear and concise words the theory which has since been called "gold points" theory?

He next discussed arbitrage in order to deduce from it a system of relations which connect the exchange values of a number of countries, considered two by two. This discussion is the first example of a synthesis which permits clear analysis of the nature and number of variables considered in the economic system, as well as of the relations which exist between them. In other words, Cournot here pointed the way to the founders of the theory of economic equilibrium, who try to show exactly the degrees of freedom of the economic system by comparing the number of variables with the number of relations which connect them.

The chapter on foreign exchange is of great geometric rigor, but at the same time is very closely connected with con-

crete phenomena. He treated it like a man familiar with the technique of arbitrage and with all the policies which different countries have used in building up their monetary systems.

4. Cournot's Influence in Economics. Cournot's economic work had little success during the nineteenth century, especially in France. Dupuit, who published his first essay in 1848, had similar views but made no reference to Cournot. But Cournot's influence has grown abroad and with the passage of time. It seems that in spite of the development of the theory of economic equilibrium by the Lausanne School there is still room for the more modest ideas of Cournot and Dupuit. The simplifying hypotheses of these two authors still have great interest, furthermore, for certain applications of statistics and mathematics to economic phenomena.

Friday, July 22—Problems of the Long Cycle in Economic Development Since the Eighteenth Century, S. V. CIRIACY-WANTRUP, Assistant Professor of Agricultural Economics, University of California.*[1]

With only three and one-half "long cycles" under statistical analysis as to phase and amplitude, the term "cycle" should be used with great care or not at all. On the other hand, there are historic phenomena of what may be called "periods of expansion and stagnation" in economic activity of longer duration than those associated with the "business cycle" and sufficiently marked and of a sufficiently common character and common causal origin to allow common theoretical treatment. Only two problems offered by those phenomena can be dealt

* For copy of complete lecture, order Document No. 1140 from American Documentation Institute, care Offices of Science Service, 2101 Constitution Avenue, Washington, D.C., remitting 30 cents for copy in microfilm, or $1.20 for photoprints readable without optical aid.

[1] An abstract of a paper which represents itself the summary of extensive research is extremely misleading. For a more appropriate expression of the writer's views the reader should consult *Agrarkrisen und Stockungsspannen,* Berlin, 1936, 445 pp. An English translation and adaptation of this study is being prepared in co-operation with Dr. C. O. Hardy of the Brookings Institution.

with here: that of evidence and that of theoretical explanation.

The evidence is contained first in the well-known general movement of all prices[2] upward from the 80's of the eighteenth century until about 1815, downward until about 1840, upward until the beginning of the 70's, downward until the middle of the 90's, upward until 1920, and downward since. The evidence is equally clear in the movement of prices, caused by difference in sensitiveness between different price series, relative to the effects of those forces which bring about the general movement of all prices. These shifts in the relationship of sensitive and rigid prices have led to changes in the relative position of whole industries in opposite directions during the periods of price upswing and price downswing. The three long depressions of agriculture and of those industries which share with agriculture the obligation to sell at sensitive prices and to buy at rigid prices is explained by this fact.

A further important set of evidence can be drawn from the study of the "business cycle" or the "short cycle."[3] The periods of expansion and stagnation find their expression through the differences in absolute and relative duration and intensity of upswings and downswings between successive short cycles. These differences are in the same direction during a period of expansion or stagnation but their magnitude changes in a regular fashion between the three or sometimes four short cycles of the same long period. The magnitude of change in a period of stagnation is much greater between the first and the second than between the second and third short cycle, whereas in a period of expansion the magnitude of change is much greater between the second and third than between the first and the second short cycle. It is, therefore, easier to distinguish successive expansion and stagnation periods than successive stagnation and expansion periods.

The evidence of periods of expansion and stagnation in production, trade, wages, income, and employment appears as

[2] The term "prices" includes—besides the prices of commodities—the prices of services as well as wage and interest rates.

[3] Under "short cycle" we understand a duration from seven to eleven years. This cycle which finds its expression mainly in the key industries is—at least internationally—more important than the many shorter cycles which have been discovered in certain consumption-goods industries or in the production of perennial crops and of livestock.

deviations from a usually very pronounced long-time trend which must be eliminated through logistic curves, through straight or parabolic logarithmic curves, and through a combination of the latter before the longer movements become clearly visible. These long-period deviations from trend are the least in agriculture and the greatest in the quasi-permanent-goods industries. The consumption-goods industries occupy an intermediate position.

The changes in production, trade, employment, wages, and income relative to trend but in the same direction as the price movements are important for the theoretical explanation. The price movements and incidentally the so-called agricultural depressions can be traced to changes in industrial employment and money wages. The same theoretical tools which are useful for explaining changes of employment, production, and income during the short cycle can be employed for the longer periods of expansion and stagnation. These tools which were first used by Wicksell combine the overproduction, underconsumption, and monetary theories of industrial fluctuations in a general theory of savings, investment, and income formation. Changes in the expenditures for investment, which is the shortest formulation of the essential phenomena, are caused by favorable or unfavorable changes in *internal* conditions for investment—for instance by price changes of loanable funds, of human labor, and of raw materials—and by *external* stimuli which change the marginal net productivity of entrepreneurial investments or of public investments irrespective of whether the internal conditions for investment have become more or less favorable. It can be shown that the internal conditions for investment created during the expansion or stagnation period have not been sufficient to account for the transition from one period to the other.

Of the five powerful external stimuli, namely technological progress, natural phenomena (harvest and livestock cycles, catastrophes), opening up of markets, production of monetary metals, and finally war and preparation for war, the last two and particularly the last one have been the most important during the last one hundred and fifty years. Public investments in war goods have in the short run qualitatively the same economic effects as entreprenurial investments in quasi-

permanent goods, however different they may be with regard to the quantity of the effects even in the short run and to the after effects in the long run. Space does not allow either to treat the economic effects of wars in more detail and in their historical setting or to trace the many interreactions between wars and the technological development, the opening up of markets, and the production of monetary metals. We must be content here with the rather general statement that wars have been, at least during the capitalistic period under review, the decisive external stimuli for the outbursts of investments, incomes, and prices, which were characteristic for the last part of the expansion periods.

The reaction from these outbursts on the other hand dominated characteristically the first part of the following stagnation period until their aftereffects tapered off. During the last part of the stagnation period and the first part of the following expansion period stimuli other than wars were of equal if not of greater importance. If, therefore, that part of both periods which was dominated by wars is cut off, very little statistical evidence and little theoretical foundation remains for the longer periods of expansion and stagnation in economic activity.

The term "long cycle" is justified only if it can be established that there is and why there is a periodicity of major wars. It seems not impossible to make at least certain economic, social, and political tendencies towards such a periodicity conceivable, but such tendencies have only conditional not causal importance for historical events.

Friday, July 22—Study of a Law of Demand: Postal Traffic in France from 1873 to 1936, PROFESSOR ROY.

In order to illustrate by a concrete example the concepts of Cournot in the matter of demand, this paper summarizes a study undertaken in co-operation with the speaker by M. E. Morice, professor of mathematics. This study concerns the postal traffic of France from 1873 to 1936. Monopolized serv-

ices have always seemed particularly suitable for econometric researches; the results of the work of M. Morice confirm this opinion, for they have permitted a satisfactory determination of the elasticity of demand for postal traffic and of its development in the course of time.

In the period before the World War the rates were changed only twice. This stability makes it possible to consider these changes of rates as veritable experiments in the sense of Cournot, since the periods of stable rates led to a satisfactory determination of the law of development of the traffic in time. It is not the same for the postwar period; it is then necessary to have recourse to a purely statistical procedure, the validity of which rests in large part on the conclusions which arise from the study of the prewar period.

Part 1. Determination of the coefficient of elasticity of demand. In a monopolized service, the variations of rates are in general infrequent but relatively important. The result is that the equilibrium between price and consumption does not re-establish itself until the end of a certain period of adjustment: four years in the case of the rate reduction in 1878.

Under the hypothesis that the equilibrium observations—preceding and following the period of adjustment—may be represented by a law of the form $y = A\ e^{bt}\ p^{\lambda}$, the coefficient of elasticity λ is defined by the formula

$$\lambda = \frac{\log y_2 - \log y_1 - b\,(t_2 - t_1)}{\log p_2 - \log p_1},$$

t_1 and t_2 being two points of equilibrium situated, one immediately before the change of rate, the other at the end of the period of adjustment.

Various modifications of the above formula may be used depending upon the distribution of accidental variations and of the cyclical variations. For the period before and after the modification of postal rates for ordinary letters in 1906 these give values of λ ranging from —0.52 to —0.75.

Part 2. Prewar period, 1873-1913. In this period the rate for ordinary letters was reduced from Fr. 0.25 to Fr. 0.15 in 1878, and to Fr. 0.10 in 1906. Each time the public took three or four years to accustom itself to the change. Except for the transition periods, the logarithms of the numbers of letters lie

approximately on three parallel straight lines. The annual rate of increase is 2.5 or 2.6 per cent; the elasticity at the first rate of change is $\lambda = -0.59$ and at the second $\lambda = -0.44$. In general the distribution of the deviations of the two series shows a very distinct cyclical character, and develops parallel to the general business cycle.

In 1906 the reduced rate for ordinary letters was the same as that for post cards, and thus resulted in the substitution of letters for post cards in many cases. Making allowance for this, coefficients of elasticity for ordinary letters become $\lambda = -0.50$ in 1878 and $\lambda = -0.35$ in 1906. These calculations show that the coefficient of elasticity, characteristic of the demand studied, has diminished in absolute value between 1870 and 1914.

Part 3. Postwar period, 1922-1936. The rate for ordinary letters was raised from Fr. 0.10 to Fr. 0.25 in March, 1920, to Fr. 0.30 in July, 1925, to Fr. 0.40 in April, 1926, and to Fr. 0.50 in August, 1926.

In spite of the stability of the rate for ten years after 1926, there were important variations in the number of letters; these variations showed a distinct negative correlation with the real price calculated by the aid of an index of retail prices.

A curve fitted by the time-regression method of Professor Henry Schultz gave an annual rate of growth of 2 per cent, and an elasticity of $\lambda = -0.41$, but the fit was not as good as for the prewar period. The computation of the elasticity by different formulas gave approximately the same value for the average of the period, but showed considerable variation from year to year.

Part 4. Conclusions. One may derive from this study the following essential points:

1. The elasticity of demand for postal service has decreased distinctly in absolute value between 1878 and 1906. Its present value is approximately the same as immediately before the war.

2. The distribution of the deviations between the observed and the calculated values is influenced by economic cycles.

3. As Professor Henry Schultz has noted (in a study of the demand for sugar in the United States), different meth-

ods give approximately the same value of the coefficient of elasticity. Nevertheless, the great instability of the values given by the formulas for the variation of elasticity from year to year make preferable, in the present case, a law of constant elasticity, or of elasticity varying only slightly with the time, its mean value at an epoch t being directly determined by a study of a period including this epoch.

4. The sensitivity of the public to "real prices" is distinctly shown by the study of the period 1922-1936.

5. For this same period the "real receipts" have varied between 100 and 200 million, while the number of letters has remained between 1440 and 1780 million (deviation less than 25 per cent). The minimum receipts correspond to the period of maximum traffic because of the corresponding low value of the real price.

These figures show all the interest of an exact study—in particular for a monopolized service—of the adaptation of price to economic conditions, taking account of the elasticity of demand for the merchandise or service considered.

Monday, July 25—Mathematics in Economics, HENRY SCHULTZ, Professor of Economics, University of Chicago.

The idea of applying mathematics to economics is a very old one, having been urged by Giovanni Ceva in 1711. It was not, however, until 1838 that the first successful attempt to apply mathematics, *as a tool of discovery,* in economics was made. In that year Augustin Cournot published his *Researches into the Mathematical Theory of Wealth* which, though it was completely forgotten for a third of a century, is now recognized as one of the greatest of economic classics, and still constitutes a most excellent introduction to mathematical economics.

The function of mathematics in economics is not primarily to facilitate numerical computations, but to establish relations between *functions* of whose algebraic expressions we may be ignorant. It is these abstract functions which are

used over and over again to state and to clarify the laws of demand and supply, to determine the incidence of taxes, and to derive the shape of economic series.

As an illustration of the deductions which Cournot reached by the aid of mathematics we may cite the following:

A tax on an article under unlimited competition will always raise its price but by an amount less than the tax itself.

A tax proportional to the net income of a producer will not affect the price of his product.

Fixed charges among costs of production do not affect price nor do taxes on fixed charges.

A tax on one of two component articles will raise the price of that article and of the composite article, but will lower the price of the other component. (It is assumed that the ratio of the two components in the composite is fixed.)

A related paradox discovered by Edgeworth is that if a monopolist sells two articles, say first- and third-class railway tickets for which the demand is correlated, it may be possible to tax the third-class tickets, at a fixed amount each, with the result that the monopolist not only pays the tax but lowers the prices of both kinds of tickets. Professor Hotelling has shown that the necessary conditions for this phenomenon to occur are not so improbable as they were supposed to be by Edgeworth.

In the field of general equilibrium, which was developed after Cournot's time, the fundamental problem which the mathematical method enabled us to solve is the following: Given an economy with such and such individuals, producing, consuming, and saving under such and such technical and social conditions, to determine:

(1) The quantity of each commodity that will be bought and sold by each individual;

(2) The quantity of each productive service that will be used in the making of each commodity; and

(3) The prices of the commodities and of the services.

The solution of this problem constitutes the greatest achievement of Léon Walras and Vilfredo Pareto: They showed that there exists a number of independent equations which is equal to the number of unknowns and that the problem is, therefore, theoretically determinate. It is impossible

to solve this problem without the aid of mathematics for the human mind can not solve mentally more than two simultaneous equations.

In the field of dynamic economics the problem which economists are attempting to solve with the aid of mathematics is how prices and quantities fluctuate in time as a result of disturbances in the system.

From a survey of the contributions of mathematics to economics it is clear that whatever may have been true in the infancy of the science, the mathematical method is now the *sine qua non* for the future development of pure theory. The differential and integral calculus, "to which man owes directly or indirectly the greater part of the control that he has obtained in recent times over physical nature," have become indispensable in our field. This is also true of algebra — the greatest labor-saving device the human mind has produced. More recently economists have also been attracted to the "wily charms" of the calculus of variations.

The fact that the mathematical method has been found useful, and even indispensable, to the study of economics does not mean, however, that mathematics constitutes an open sesame to economics. No amount of mathematical skill can take the place of a thorough familiarity with the workings of our economic institutions in their social, legal, and historical setting. Moreover, there is a decided difference between studying economics *with* mathematics, and studying it *as* mathematics. What has enabled us economists to apply the mathematical method successfully to part of our field — and it is a small part — is the fact that in the market place men do choose among alternative ways of satisfying their wants, and so approximate the ideal calculator of the best way of economizing resources to satisfy given ends—the economic man. In its proper sphere, the use of the universal language of mathematics leads to a removal of artificial and conventional barriers of thought and to an enlargement of our view. It also has a more practical advantage. It enables us, without the inspiration of genius, which no one can command, to solve certain problems—yes, even to solve them mechanically in complicated cases where genius itself becomes impotent.

Monday, July 25—Is Industrial Planning Compatible with Continued Individual Ownership of Property? MORDECAI EZEKIEL, Economic Adviser, Office of the Secretary, United States Department of Agriculture.

Planning may be conducted on many levels. Each individual plans his future life, dimly at least, when he decides how much and what kind of schooling he will try to get. Each business man makes plans, based on forecasts of the future, when he buys a new machine or builds a new factory. Each efficiency engineer plans, consciously and intensively, when he rearranges a factory layout or reorganizes an assembly line. All industrial activity involves advance planning of some sort or other, by those in positions of responsibility.

I shall use the term Industrial Planning to apply to economic planning on two higher levels. These are the intra-industry level, as between concerns within each industry, and the inter-industry level, as between major industries. Except illegally through "Gary dinners" and the like, or legally for a brief time under the NRA codes, planning even on the intra-industry level has been little developed in this country. Effective industrial planning on the inter-industry level is almost unknown.

I have shown elsewhere[1] that concerted inter-industry planning for increased production might achieve much higher levels of production and consumption than we have been able to attain, even momentarily, without such planning. These higher levels of production and consumption would preserve the consumers' freedom of choice and would balance production against such demands. Would it be possible to draw up such plans, and to carry them into operation, while the ownership of factories and farms remained in private hands?

Programs of expanded production cannot be carried out successfully unless monetary and fiscal policies, as well as the actions of individual concerns and industries, are all properly correlated. Public agencies already have broad powers and responsibilities with respect to policies and actions in the financial field. Effective plans for industrial expansion must cover both the financial and the industrial phases of the prob-

[1] *$2500 a Year*, Harcourt, Brace & Co., 1936.

lem. My discussion here is limited to the industrial phase.

Some economists believe that public planning for privately owned concerns is impossible, because of the political pressure groups that would become active. But is it not possible that industrial planning can be so conducted as to operate with the co-operation, instead of the antagonism, of the owning groups?

We are now in the sixth year of agricultural adjustment operations. With their national goals and commodity programs, these constitute the greatest single experiment in industrial planning in America. Four elements employed in the agricultural planning might prove useful elsewhere:

1. Economic incentives offered individual entrepeneurs to co-operate in the program.

2. Democratic procedures in developing the programs and in administering them, with much of the planning coming up from the bottom.

3. Provision for the production of adequate supplies for normal consumption, plus an increased reserve to protect against weather and other hazards.

4. Protection to the co-operating individual against surpluses if the plans do not work perfectly by commodity loans and ever-normal granary operations.

With these elements it has been possible to secure concerted action by the bulk of our commercial farmers, whereby their production is directed according to national programs. At the same time they individually remain responsible for the efficient and effective operation of their farms, so that the drive of individual initiative has not been lost.

Suitably modified to fit the conditions of urban industry, these same techniques might be employed to bring about a concerted and balanced increase in the volume of industrial activity and employment, without disturbing the individual ownership of the properties involved.

The individual entrepeneur finds it difficult to plan today. Much of that difficulty arises from his lack of any dependable knowledge of what the future holds as to industrial activity, demands for his product, prices of that and competing products, and prices of production factors. To the extent that inter-industry planning reduces the uncertainty as to the future,

and, at least for the year ahead, limits the area over which changes may occur, it will give each individual proprietor a better and firmer basis for his planning his own activities. By basing the activities of different concerns on one rational and consistent plan instead of on many divergent or conflicting guesses, the chances of events coming near to the plan will be greatly increased.

Tuesday, July 26—Statistics in Economics, PROFESSOR SCHULTZ.

Although there is still some dispute as to the exact role of statistics in economics, we no longer speak of quantitative *versus* qualitative economics. We know that the first cannot be dispensed with, if for no other reason than because distinctions of quality underlie distinctions of quantity. We also know that it is chiefly the want of realistic statistics and a method of analyzing them that is responsible for much of the aridity of the social sciences.

Rather than expatiate on this obvious proposition it is more instructive to illustrate, by means of a problem from economics, how an abstract theory which is expressed in non-measurable magnitudes can be restated so as to admit of a statistical test, and how the very process of testing suggests a reformulation of the original theory, which in turn must stand the test of statistical verification.

The problem which is considered in this paper is, "How to test the rationality of human behavior in the market place?" This problem is of importance not only to economists, but also to psychophysicists, advertisers, business men, and others.

In pure theory we say that he is a rational or consistent individual who allocates his (fixed) income among the various goods and services available in the market in such a manner, that the utility which he derives from the last "penny's worth purchased" is the same for all commodities.

In mathematical terms this condition is:

$$(1) \qquad m = \frac{\varphi_1}{y_1} = \frac{\varphi_2}{y_2} = \cdots = \frac{\varphi_n}{y_n},$$

where $\varphi_i = \varphi_i(x_1, x_2, \cdots, x_n)$ is the marginal utility of the commodity X_i, y_i is the price of the commodity (X_i), and m is the marginal degree of utility of the individual's income expenditure. To divide the marginal degree of utility of any commodity by its price is to make the unit of the commodity the "penny's worth."

If the individual's expenditure on any one commodity constitutes only a small proportion of his total income, then the marginal degree of utility of money (m) may be taken as approximately constant, and the following proposition can be proved mathematically:

> The demand behavior of a rational or consistent individual with respect to any two commodities (X_1) and (X_2) is such that the TOTAL effect of a 1-cent increase in the price of (X_1) brings about the same change in his demand for (X_2) that the TOTAL effect of a 1-cent increase in the price of (X_2) brings about in his demand for (X_1).[1]

Mathematically this condition states that:

$$(2) \qquad \frac{\partial x_i}{\partial y_j} = \frac{\partial x_j}{\partial y_i},$$

where

$$x_i = x_i(y_1, y_2, \cdots, y_n, r), \qquad (i = 1, 2, \cdots, n),$$

is the demand function for the ith commodity and r is the individual's money income.

This proposition belongs to a category of laws which is comparatively rare in the social sciences: It specifies quantitatively definite relations which must exist between the individual's demand functions — if his behavior is rational. It enables us, therefore, to test the extent of the agreement between theory and fact.

An analysis of the demand for beef and pork shows that the assumption of rationality or consistency is realized approximately.

[1] Henry Schultz, *The Theory and Measurement of Demand*, Chicago, 1938, p. 599.

Condition (2) can be generalized so as to take into consideration the case when the expenditure on the commodities constitutes a large proportion of the income.

The process of recasting principles is not new to economics. In fact, it is the natural outcome of any attempt to define concepts so that they will have meaning in terms of operations, and is not peculiar to economics.

Economic laws, however, have not the definiteness and the universal demonstrability of physical laws. This is particularly true of the so-called dynamic laws of economics which are simply empirical extrapolations of the present situation.

Although a theory of dynamic economics is still a thing of the future, we must not be satisfied with the *status quo* in statistical research. Many of the statistical researches that are carried on in the social sciences lack the inspiration of any theory — static or dynamic. Relatively few of us like to perform the necessary *mental experiments* which should precede the assembling and the manipulating of the data, if the results are to be significant. Relatively few of us pause to ask ourselves, *before embarking on the investigation:* What are we trying to prove or disprove? What propositions will have to be established for proving or disproving our thesis? Can they be derived from the statistics which we propose to collect? If so, by what method of analysis, and to what degree of probability? It is not surprising, therefore, that much of what passes as research in the social sciences is a more or less aimless distribution of questionnaires and a more or less mechanical elaboration of erroneous data through the use of correlation coefficients and other statistical devices. Research is not good simply because it is mathematical or statistical, or because it makes use of ingenious machines. Research is good if it is significant, if it is fruitful, if it is consistent with established principles, or if it helps to overthrow erroneous principles.

Tuesday, July 26—Does Industrial Planning Involve Predetermination of Price Policy? DR. EZEKIEL.

As I indicated in my preceding talk, I am using "industrial planning" to apply to plans for industry on the intra-industry and inter-industry level. By price policy I mean the policies which govern such planning with reference to the level and the movements of prices, both as a whole and for individual commodities.

Economists have argued long as to whether a socialist economy could operate effectively without the use of prices.[1] That question does not concern us here, for, as I indicated in my previous paper, I am dealing with planning activities within our present capitalist economy, where prices will continue to be the means of distributing income, determining profit and loss, and influencing the choices of consumers and of producers.

Price-policy problems of many sorts would arise in drawing up expansion programs, even for selected basic industries. Here are some that would have to be faced:

1. Would the planning concern itself solely with production quotas, or would it also cover the prices at which the programmed production was to be offered for sale?

2. If prices are covered, would the plans apply only to the general price level, or to prices and price relations as between individual commodities?

3. Where costs of production are reduced by larger volume or by technological change, how should this gain be divided as between higher profits, lower prices, and higher wages? Should higher wages be restricted to workers in industries with such declining costs, or should they be applied even to industries where they will produce higher prices?

4. How will errors in planning production and prices be corrected? Should errors in estimating the production-price-consumption balance be reflected in changed prices or in changed commodity stocks and inventories? Would the flexibility of prices under a planned program differ as between agricultural and industrial products?

[1] See articles by Lerner, Dodd, and Lange in the *Review of Economic Studies*.

5. Will the price for labor (wages) and the price for capital (interest and profits) be set on the basis necessary to attract the needed supply of each into each industry? If not, what basis should be used? A fair share in national productivity, for labor? A fair return on reasonable investment, for capital? What is "fair" in either case?

6. Will prices be fixed for all concerns in each industry, or will a limited range of price competition between concerns be possible?

So long as consumers are free to purchase what they will, at the prices charged, and so long as producers are free to combine their elements of production at the price prevailing for each material, machine, or labor skill, prices will obviously affect quantities used. Accordingly, I believe that no program of planning production can work unless the relation of individual prices to production and consumption is also given due consideration. I would therefore answer to the first question "yes," and to the second question "both." How that price policy is to be developed will depend upon how the various other questions stated above are answered. Differences in the answers would make great differences in the type of planning which would result.

Wednesday, July 27—Periodogram Analysis with the Phase a Chance Variable, EDWARD L. DODD, Professor of Actuarial Mathematics, University of Texas.

In place of a set of harmonic constituents, each rigidly periodic, Eugen Slutzky[1] considers random curves exhibiting only a sort of quasi-periodicity. Indeed, for over a decade, papers have been appearing which in some way abandon strict periodicity. We may, in fact, think of a period of 8 years as substantially existing when 4 cycles may take 33 years, even when there is no more tendency to make up the lost year than

[1] "The Summation of Random Causes as the Source of Cyclic Processes," *Econometrica*, Vol. 5, 1937, pp. 105-146.

to lose another year. As a rather slight departure from rigid periodicity, I am here postulating a simple chance element in the phase, to note what effect this has upon some functions used in period testing.

Let $z_1, z_2, \cdots z_r, \cdots$ be independent chance variables with the same symmetric distribution, so that for each z_r the expected value $E(z_r) = 0$. Let a be an initial phase. Suppose that k is a quasi-period, where some multiple of k is equal to K, a whole number. Let $\vartheta = 360°/k = 2\pi/k$. Then, as a quasi-periodic function y_r write:

$$(1) \qquad y_r = a\cos(\theta r + a + Z_r), \quad Z_r = z_1 + z_2 + \cdots + z_r.$$

Suppose now that y_r is tested for the period k by using K consecutive terms y_r. With $m = 0, 1, 2, \cdots$ set $t = mK$; and let

$$(2) \qquad c_r = \frac{\cos r\vartheta}{\sqrt{K/2}}, \; s_r = \frac{\sin r\vartheta}{\sqrt{K/2}},$$

$$(3) \qquad C_m = \sum_{r=t+1}^{t+K} c_r y_r, \quad S_m = \sum_{r=t+1}^{t+K} s_r y_r, \quad I_m = C_m^2 + S_m^2.$$

Now, if in (1) the chance variable Z_r had not been present —that is, if each $z_r = 0$—then the functions in (3) would take their well-known simple values:

(4)

$$C_m = a\sqrt{K/2}\cos a, \quad S_m = -a\sqrt{K/2}\sin a, \quad I_m = Ka^2/2.$$

But with these chance elements z_r present, the expected values $E(C_m)$, $E(S_m)$, and $E(I_m)$ each consists of a principal part and a sort of remainder term, the principal parts approaching C_m, S_m, and I_m of (4) when each z_r approaches zero. If we set

$$(5) \qquad \eta = E(\cos z_r), \quad H = 1 + \eta + \eta^2 + \cdots + \eta^{K-1},$$

$$a' = a + Z_{t+1},$$

and ignore remainder terms, we may write:

$$(6) \qquad E(C_m) = \frac{aH\cos a'}{\sqrt{2K}}, \quad E(S_m) = \frac{-aH\sin a'}{\sqrt{2K}};$$

$$(7) \qquad E(I_m) = a^2 + \frac{2a^2}{K}\sum_{s=1}^{K-1}(K-s)\eta^s\cos^2 s\vartheta.$$

To illustrate (7) suppose that z_r is normal with variance σ^2, then $\eta = E(\cos z_r) = e^{-\sigma^2/2}$. Suppose that $\sigma = 0.2$ radian $= 11.°46$, making the probable error about 8°; then $\eta = 0.9802$. If now $k = 8$, and $K = 2k = 16$; then from (7) $E(I_m) = 7.23a^2$, which is about 90 per cent of the value of I_m in (4). If however, we take 4 cycles, and set $K = 4k = 32$, then $E(I_m) = 13.09a^2$, only about 82 per cent of the value of I_m in (4). If, as here taken, the chance element z_r has a probable error of about 8°, it would not indeed be unusual for 4 cycles of this 8-year "period" to take 33 years.

From (6), (7), and similar formulas, we may estimate to what extent various test functions will be damaged by a chance phase element. It is obvious that the larger the variance σ^2, the smaller must be the set of K consecutive terms, that we can profitably group together.

Wednesday, July 27—Problems Involved in Planning of Production, Prices, and Wages in Industrial Planning, DR. EZEKIEL.

One set of problems in industrial planning will revolve around the exact nature of the production function, for each typical size of plant in each industry for which a program is being prepared. In addition to the elements usually included in the production function, this will lay especial emphasis upon the changes in real production costs (a) as output rises toward the installed capacity of each concern, and (b) as new technological methods are employed. The determination of these relations will be especially important in the heavy industries where overhead costs bulk large and where, according to the scanty information now available, direct labor costs per unit fall rapidly as output rises.

A second set will revolve around the provision for capital-goods expansion. Obviously any expansion program must provide for the capital goods necessary in producing the immediate programmed expansion of consumption goods. But be-

yond that, how rapid an expansion in underlying heavy industry should be budgeted for? Population increases and other physical factors aside, the answer to this problem will ultimately involve the appraisal of the relative significance of immediate increases in consumption goods versus provision for future increases in such goods. How far individual savings preferences can aid in reaching this decision is a related problem. To the extent that these problems can be resolved, the present disorderly and erratic character of the flow of capital goods and the creation of new capital goods may be made far more rational.

A third set of problems will relate to the operating question of how errors in plans will be corrected. Will prices be changed within the pre-planned period? Will production plans be revised currently? Or will errors be allowed to show up in carry-over changes, with the necessary adjustment to be made in subsequent periods?

A fourth set of problems relates to the extent to which planning will be substituted for competition. Should planning be restricted to those industries where competition is ineffective, while other more competitive industries are left to be regulated by market interactions as heretofore? That might mean leaving textiles, clothing, and merchandising unchanged, while planning is used for heavy industries. In such case, would competition also be relied on to improve labor conditions in the unplanned industries?

Fifth, what about the distribution of income? Would planning definitely be directed to raising lower-bracket incomes, and holding down on upper-bracket ones? Or should this phase of the problem continue to be handled by progressive income and estate taxes and by relief payments?

How far statistics can aid in advance planning is another fruitful field of consideration. Instead of attempting to solve an infinite set of simultaneous equations, as Robbins has suggested would be necessary, industrial planners can use statistics and statistical analyses of many types as guide posts. These include past performances of production and sales under various changes in industrial activity and national income, past records of family expenditures at different income levels, and analyses of the effect of price and income on consumers'

demand. In interpreting those results of the past, it will be necessary to allow for the fact that past periods of high production have been periods of unbalanced production, with a higher proportion of capital-goods production, relative to consumer goods, than could be long maintained. Balanced programs for the future will have to be so constructed as to be dynamically stable, as between capital goods and consumer goods. It will also be necessary to allow for the effect that altered prices may have in changing consumers' choices under previously prevailing prices, as shown by studies of consumers' expenditures.

Obviously, there are many balances that could be struck, between production of each planned product and requirements for consumption or capital expansion; between incomes to be distributed and goods or services to be sold to consumers; between capital expansion, and savings and credit formation; between labor and raw materials available and labor and raw materials required; between regional production programs, regional population prospects, and regional housing programs. In the early stages of planning perhaps only the increments introduced by the plan on each side of each balance can be considered. The minimum number of balances to be studied, and the minimum number to be required to be fulfilled before a program can be carried into action, is itself a problem to be examined.

Thursday, July 28—Recursive Methods and the Analysis of Time Series, MERRILL M. FLOOD, Research Associate, Princeton Local Government Survey, Princeton University.

One problem often met in the analysis of times series is that of "predicting" the value of a time variable $y(t)$, for $t = 1, 2, \cdots, N$, in terms of its value $y(t—1)$, $y(t—2)$, $\cdots$, $y(t—v)$ known for the preceding v time intervals. More generally, a prediction may be desired for several such series $y_h(t)$ for $h = 1, 2, \cdots, n$.

In this paper *it is assumed that the expected value of each series at time* t *depends linearly on the observed values of all of the series at preceding times.* This is written more precisely in the form,

$$(1) \qquad E[y_h(t)] = \sum_{j=1}^{n} \sum_{k=1}^{v_j} r^h_{jk} y_j(t—k) \text{ for } h = 1, 2, \cdots, n.$$

Since most of the essential difficulties are met in the case $s = 1$ of a single series, it is convenient to illustrate the method and results by discussing this simpler case. Thus, the relation (1) becomes

$$(2) \qquad E[y(t)] = \sum_{k=1}^{v} r_k y(t—k).$$

The method of maximum likelihood provides the necessary estimates $\bar{r}_k$ of the parameters r_k. In particular, if

$$d(t) = y(t) —E[y(t)]$$

and if it is assumed that $d(t)$ is distributed normally with mean zero and variance s^2, the required estimates are:

$$\bar{s}^2 = \frac{1}{N—v} \sum_{t=v+1}^{N} [y(t) — \sum_{k=1} \bar{r}_k y(t—k)]^2, \quad \bar{r} = S^{-1} S_0,$$

where

$$\bar{r} = \sum_{k=1}^{v} \bar{r}_k e_k, \quad S = \sum_{j,k=1}^{v} S_{jk} e_{jk}, \quad S_0 = \sum_{k=1}^{v} S_{k0} e_k,$$

$$S_{jk} = \sum_{t=v+1}^{N} y(t—j) y(t—k) \text{ for } j,k = 0, 1, \cdots, v,$$

and where e_k and e_{jk} denote the fundamental vectors and matrices in a space of v dimensions; thus e_{jk} denotes the square matrix of order v with unity in the (j,k) position and zeros elsewhere.

The relation (2), with r_k replaced by $\bar{r}_k$, now provides estimates $\overline{y(t)} = \sum_{k=1}^{v} r_k y(t—k)$ of the variable $y(t)$ in terms of the preceding v observations $y(t—1), y(t—2), \cdots, y(t—v)$. The question arises naturally concerning the possibility of extending the predictions more than one time interval by re-

peated applications of the relation (2). This further extrapolation requires the determination of the expected values $E[y(t)]$ and their variances $E[y(t) - \overline{y(t)}]^2$ at each step as the relation (2) is iterated—and these quantities are next determined.

To display this result it is convenient to define a matrix M and vectors y_t by the equations,

$$M = \sum_{k=1}^{v} \bar{r}_k e_{1k} + \sum_{k=2}^{v} e_{k\,k-1}, \quad y_t = \sum_{j=1}^{v} y(t+1-j)\, e_j,$$

and quantities d_{kt} by the relations,

$$d_{kt} = e'_1(y_t - M^k y_{t-k}).$$

The variance at the kth step, under the assumption that $E[d_{1\,t-k} d_{kt}] = 0$, is

$$s^2_k \equiv E[d_{kt}^2] = s_1^2 \sum_{h=1}^{k} (e'_1 M^{h-1} e_1)^2.$$

If it is now assumed that the given time series is the result of successive applications of the recursive relation (2), with initial values $y(0), y(-1), y(-2), \cdots, y(1-v)$, then estimates are required for these initial parameters—and an explicit expression is obtained for these estimates. If this initial vector is denoted by y_0, then $\bar{y}_0 = L^{-1}L_0$, where:

$$L = \sum_{h=1}^{N} \sum_{j,k=1}^{v} (e'_1 M^h e_j)(e'_1 M^h e_k)\, e_{jk},$$

$$L_0 = \sum_{h=1}^{N} \sum_{k=1}^{v} y(h)(e'_1 M^h e_k)\, e_k.$$

Finally, these results are extended to the general case of $n > 1$ time series and the methods are applied in the analysis of actual time series. The time series used in the application are daily high, low, opening, and closing prices and volume of sales on the New York Stock Exchange of General Motors common stock.

Thursday, July 28—Statistical Hypotheses and Estimation in Historical and Critical Light: A Scherzo on the Semantics of the Modern Mathematics of Statistical Theory, I; ARNE FISHER, New York City.

In the 1920's there arose in the United States a new cult of business statistics, and closely connected with it there came to the fore a flourishing and influential sect, the "Normalites," who have been and still are exercising a dominant influence on the development of statistical theory in this country. They consider fundamental the concept of a normal for any economic phenomenon, and express actual values by percentages above or below it. Yet few, if any, of them seem to know what their occult "normal" really means. F. C. Mills, to be sure admits that "such a normal value is essentially an empirical construction" and "should not be assumed to possess any normative significance."[1]

It would thus seem not at all unlikely that the "normal" may even be abnormal. If so, the issue merely simmers down to a question of semantics through the simple device of changing the meaning of words.

In contrast to this method, which fits trends and computes seasonal variations, and ends with residuals representing cyclical and random fluctuations, the writer presents a method which he has used for over twenty-five years.

This method, which is founded on the so-called quasi-systematic error theory, rejects at the very outset the postulate that long-term trends of social and economic phenomena over a long period of years move in straight lines. Any force that moves far and wide and with a moderate velocity does not follow the proverbial flight of a crow, but progresses in a wave-like formation. The underlying ideas of the quasi-systematic-error theory, which is quite different from the ordinary error or frequency-curve theory are: Observations when arranged chronologically show inevitably large fluctuations from week to week or month to month (as known for more than a century by writers on vital statistics). The sources of such errors, which are usually correlated or bound together, may be classified under the following categories:

[1] *Statistical Methods*, Revised, New York, 1938, p. 229.

(1) Periodically recurring forces which occur with decidedly marked regularity and intensity over a *constant* period of time.

(2) Purely fortuitous variations, or chance errors, subject to the laws of the calculus of probabilities. Such variations are considered *independent of time.*

(3) Evolutionary variations (or, as Lexis calls them, symptomatic variations) of far less regular form that the periodic fluctuations in the sense that their periods or phases are not known a priori.

It will be noted that (1) corresponds to seasonal variations. Category (3) represents a blend of what the "Normalites" somewhat vaguely call "long-term trends" and "cyclical variations" (business cycles). The writer contends that it is rationally impossible to separate the two, because evolutionary forces, moving over a long period of time, progress invariably in wavelike formation and not as simple straight lines or curves of low parabolic order.

The distinction of the quasi-systematic-error theory lies in the fact that it is applied directly to the original data, instead of requiring prior corrections for seasonal variations and long-term trends. Such corrections, together with the elimination of chance variations, enter automatically in the application of the theory, with the result that the final trend is generated as a smooth, undulating curve. A detailed description of the method has been given elsewhere.[2]

The method has been applied to monthly averages of weekly freight-car loadings from January, 1918 to December, 1927, and the results can be compared with the "normal" method as applied to the same data by Mills.[3] Mills' analysis rests or falls with the determination of the "normal" trend from which all his subsequent results are derived. Yet this trend is based on the relatively short period of 10 years; a straight-line trend for the period 1918-1937 gives very different results. Moreover, the straight-line trend method implicitly assumes that the quasi-cyclical variations are to be regarded as chance errors. An examination of the residuals resulting from Mills'

[2] *Journal of Accountancy*, September, 1937.
[3] *Op. cit.*, pp. 284-304.

method, however, shows that they are systematic rather than accidental.

On the other hand, when the quasi-systematic error theory is applied to the data, a sinuous-shaped curve is produced with relatively regular recurring seasonal variations around it. By the application of what Thiele calls an "error critique" it is possible to calculate the expected carloadings for each month. The 120 percentage residuals given by the differences between the observed and expected figures, resulting from the quasi-systematic method, are found to show a neat frequency distribution around a mean value of zero in much closer conformity to the Gaussian error curve than Mills' residuals, which give a compound distribution with two pronounced maximums in the neighborhood of —12% and +3% respectively. The dispersion (standard deviation) of Mills' residuals is 7.5 against 3.8 in the residuals from the proposed method, which also shows nearly ten times as great a correspondence with accidental errors when judged by Helmert's criterions of the alternation of positive and negative signs. Again, an application of the chi-square test gives a value of P greater than 0.86 for the quasi-systematic error method and of less than 0.01 for Mills' method in relation to the observed frequency distributions of the respective values of his residuals.

These results, however, apparently are not convincing to some of the "Small Sampleites," among whom a few have gone so far as to claim that the above-mentioned high value of P might be an indication, as they put it, "that the results were rigged up to confound serious research workers in the theory of sampling." If this somewhat dubious criticism is to be taken seriously, what is one to say about the numerous sets of carefully collected observations in plant physiology, notably by the celebrated Danish biologist, Johannsen, with equally high values of P, unless, of course, Le Bon Dieu in his inscrutable wisdom created variation within the species with the sly purpose of confounding future generations of statisticians. In the writer's opinion (and speaking as an amateur as it were) much credence should not be given to hypotheses or theories when the calculated value of P falls below 0.5. Yet many "Small Sampleites," Harold Hotelling among others, are apparently willing to accept a theory or hypothesis when P may

be as low as 0.05. Dr. T. Fry is even more optimistic. He seems willing to accept a certain theory though P is less than 5 in 1000.[4]

To be sure, most people possess a germ of the gambling instinct. But gambling is one thing and the testing of an alleged scientific method or theory is quite another, and even the most enlightened of investigators may well tremble to engraft the fruits of small-sample theory upon the conduct of trade and industry, lest the cross should prove too vigorous for the aged vegetables. One also should recognize that a cloak of scientific piety is not necessarily inconsistent with a certain amount of nimbleness in mundane affairs. Yet, as compared with the psychological attitude of Dr. Fry, whose faith in his pretty theory remains unskaken, though empirically the odds are against him in the proportion of 995 to 5, the tale of the Scotchman, as reported by the late F. C. S. Schiller, "who was willing to take his oath, his dying oath, to the truth of his assertion, but shrank from betting saxpence," is hardly an exaggeration.

An examination of the at times puzzling attitudes of some of our modern statisticians leads a disinterested observer to three different conclusions:

1. That high values of P are often viewed with suspicion by the "Normalites," while extremely low ones are often accepted, contrary to the instincts of the ordinary man.
2. That, as Anatole France has pointed out in connection with the administration of baptism, it is only necessary to consider who applies the chi-square test and not the subject matter to which it is applied.
3. That the chi-square test probably is of little value in testing hypotheses, since numerous numerical illustrations show that diametrically opposite hypotheses may yield equally high values of P.

[4]T. Fry. *Probability and its Engineering Uses*, New York, 1928, p. 295.

Friday, July 29—Some Pitfalls in Applying Mathematics in Economics, F. L. GRIFFIN, Professor of Mathematics, Reed College.

There are several common types of errors or inadequate discriminations in applying mathematics, instances of which occur in economic literature or discussion. They include: (1) confusing necessary with sufficient conditions; (2) failing to discriminate between distinct values, because of inadequate definition or notation; (3) tacitly using additional postulates besides those announced, or modifying the latter in the argument; (4) using symbols where they cease to be defined, and regarding equations as equivalent which do not hold for exactly coextensive situations; (5) neglecting to note consequences of the nonintegrability of a differential equation; (6) assuming invalid relations among partial derivatives, or erring as to independence of variables; (7) making questionable uses of various statistical procedures.

Several instances of (1)–(3) above occur in the two papers of Dr. O. Lange on "The Determinateness of the Utility Function."[1] Some of the instances in the first paper were noted by Dr. Lange in the second. Two others may be considered either as coming under (3) or as gaps in the argument. Important instances in the second paper vitiate both major steps of the proof: (*a*) For a nonlinear function F the intermediate point or value of ϑ would usually be different in equations (1) and (3), and the argument does not exclude the possibility that $F'(\varphi)$ might be negative—as an example makes clear. (*b*) A similar criticism applies to equations (7a) and (7b), with the result that there is no common factor $d\varphi$ in the left-hand member of (9), which blocks the conclusion that $F'(\varphi)$ has equal values at the points in question. An instance of (1) above also occurs, as shown by an example.

Instances of (4)-(5) above occur in the controversy[2] of Professors Amoroso and Roos over Professor Evans' use of the differential equation $y = a + bp + h\frac{dp}{dt}$ to study changes

[1] *Review of Economic Studies*, Vol. 1, pp. 218-225; Vol. 2, pp. 75-77.
[2] *Giornale degli Economisti*, Vol. 44, pp. 69-70; Vol. 46, pp. 3-4; *Journal of Political Economy*, Vol. 38, p. 502.

of price p and demand y with the time t. (a) There are logical objections to regarding the above equation in its given derivative form as equivalent to the differential form $(y - a - by)\ dt = h\ dp$ for a point-set in which t is held constant: indeed the derivative, and hence the given equation, are meaningless there and no conclusion can be deduced from that equation relating to such a point-set. (b) The second or differential form is a nonintegrable equation. Thus the surfaces alluded to by Amoroso as represented by this equation do not exist. Moreover, special solutions such as were mentioned by Roos, of the character $p = p(t)$ and $y = y(t)$, have no connection with the solution for a constant t, and do not imply anything as to the latter. (The same is true of the substitute differential equation proposed by Amoroso in his lectures, unless special conditions be imposed upon the coefficients.)

Another instance of (3) or (7) above occurred in an address before a mathematical association, in which it was stated that the teachers in a certain city had been proved to be overpaid, by showing that their salary scale had risen since 1913 more than had incomes on the average.

A classic instance of (6) is to be found in the Cournot-Bertrand-Pareto discussion of the problem of duopoly. Humbler instances are available. Various instances of (7) are at hand.

Friday, July 29—Statistical Hypotheses and Estimation in Historical and Critical Light: A Scherzo on the Semantics of the Modern Mathematics of Statistical Theory, II; MR. FISHER.

One cannot deny that in so far as the literature on "statistical method" is concerned we are richer for the period that begins with the twentieth century; richer but not better off. To the historically inclined the old saying that there is nothing

new under the sun seems indeed to be particularly appropriate in some of the modern researches on mathematical statistics. Merely in the way of illustration we may refer to Professor Whittaker's[1] pious mention of Lord Rayleigh's formula in connection with the scattering of Beta rays. As a matter of fact, the formula was given nearly half a century before Rayleigh by the late Danish minister of finance, Andrae, in investigating the vector-scatter of rifle bullets in target shooting.

It is no easy task to disentangle the high doctrine of modern statistical method from its heterogeneous accretions within a learned literature which within two decades has reached formidable proportions. The job is most disconcerting, nay even distressing, so that the most diligent of disinterested critics may well shrink from the appalling task in trying to read the whole canon of novel dogmas in the gifted headmasters' peculiar prose. Weird terms like *variance, optimum statistic, efficient statistic, unbiased statistics, cumulants,* and mildly obscene *Schimpfwörter* like *lepturkosis, homoskedastic,* or *posterior fiducial inference* stand out like quills on the porcupine ready to impale even the most amiable of skeptics.

Much of this high-sounding nomenclature is essentially the pasting of rakish labels over old names, as for example, the use of "variance" for the older term of "dispersion" (i.e., the square of the standard deviation), or "kurtosis" for "peakedness" (or "flatness").

The claim has been made by some "Small Sampleites" that this new demi-vierge nomenclature is necessary because of the rise of an allegedly new theory of mathematical statistics and statistical inference developed in England and America under the leadership of investigators like R. A. Fisher, Neyman, Wishart, Hotelling, Shewhart, and Wilks, and that, while the older nomenclature took its root in the classical probability theory, the modern dogmas with their posterior fiducial inferences require a new liturgy by the division of statistical method into two separate divisions of *descriptive* and *stochastic* statistics, respectively, the latter based on the concept of probability as a limit of relative frequencies subject to the vagaries of chance.

[1] *Calculus of Observations,* London, 1924, p. 208.

Traces of such a division are already found in the works of the older Bernoulli. The idea was, however, first clearly formulated by Thiele, long before the moderns began to play therewith, in his distinction between "actual" (or observed) and "presumptive" laws of errors (or frequency distributions) which are identical with the "descriptive" and "stochastic" statistics.

Thiele seems to have been the first to recognize clearly that an adequate study of statistical samples should proceed on the basis of the numerous types of algebraic symmetrical functions which can be calculated from the sample itself and among which the "moments" of Pearson represent only a particular type. Besides the "moments," which Thiele called "sums of the powers of the observations" and denoted by the symbol s_i around an arbitrary origin, he introduced also another and more powerful type of symmetric functions the semi-invariants, denoted by μ_i and related to the moments through the following identity, first given by Thiele in 1889,

$$s_{t+1} = \sum B_t(i)\, s_{t-1}\, \mu_{i+1},$$

where $B_t(i)$ are the usual bionomial coefficients.

Moments can therefore be expressed canonically in terms of semi-invariants and vice versa, a circumstance which Thiele himself made use of in his 1889 *Almindelig Iagttagelseslaere* in expressing the presumptive (or stochastic) semi-invariants in terms of moment coefficients. Of late, and apparently for no good reason, Professor Hotelling has plastered the philologically atrocious label of "cumulants" over the old semi-invariants. It is not for us to judge of such novelties, though to follow an argument similar to that of Anatole France's, if semi-invariants are to be called "cumulants" what are we in the future to call the cumulants themselves.

A question which occupies a central position in modern statistical method is that of sampling. Two distinct problems seem here to face the investigator: one, the problem of specification, is largely of an a priori nature; the second, which is more a problem of estimation, is closely related, if actually not identical, to the a posteriori probabilities encountered in the old Rule of Bayes over which the moderns quarrel so much. The a priori problem is essentially that of the testing of hypo-

theses in the sense that one examines whether a random sample meets certain a priori specified conditions. Thiele was the pioneer in these investigations, and even Dr. Wishart admits in the July, 1930 number of *Biometrika* that a mathematical procedure "which has been described as the method of 'Student' (1908) . . . is essentially the same as that employed by Thiele." One may even go further and add that the numerical tables by Thiele from 1889 of the combinatory powers of the product moments are simpler of application than the formidable lozenge diagrams employed by R. A. Fisher nearly forty years later, so that Shewhart's claim that "from 1733 to 1908 there was nothing else that we could turn to but the old theory of errors in observations"[2] does not, like certain fine vintages, improve with the lapse of years.

The a posteriori problem, the estimation of the presumptive (stochastic) limits of the semi-invariants in an unknown universe on the basis of the semi-invariants (or other parameters) as calculated from a sample of size n, is vastly more difficult, though it is the one which is most frequent in practical work. If $'\lambda_i$ denotes the estimate of the unknown semi-invariant λ_i in the universe and μ_i is the sampled semi-invariant corresponding thereto from a sample of n, one finds as early as 1889 that Thiele supplied the following estimates:

$$'\lambda_1 = \mu_1 ;$$
$$'\lambda_2 = n(n-1)^{-1}\mu_2 ;$$
$$'\lambda_3 = n^2\mu_3[n(n-1)]^{-1} ;$$
$$'\lambda_4 = n^3[\mu_4 + 6(n-1)^{-1}\mu_2^2] : [(n-1)(n^2 - 6n + 6].$$

The fact that Thiele thus anticipated the work of the "Small Sampleites" by nearly 40 years is explicitly admitted by Dr. Irwin who says that R. A. Fisher's cumulants k_1, k_2, and k_3 "are identical with Thiele's estimates." It needed, therefore, courage of a rather droll kind for another disciple of Fisher's, J. Wishart, to maintain that "the first three of these (Fisher's) expressions bear a superficial resemblance to Thiele's formulae, a fact which has led recent writers into erroneous criticism of the authenticity of Fisher's recent and highly important contributions to statistical theory." To be

[2] *Bell System Technical Journal*, April, 1926.

sure, remarks like Wishart's are apt to happen in foggy weather!

As to the fourth-order semi-invariant Thiele's estimate differs from the one by Fisher. Some "Small Sampleites" say that Thiele's estimate is "biased" while Fisher's is "unbiased." Again it seems to be a question of semantics since in any method of estimation bias in some form or another must necessarily enter in the way of preference for some particular hypothesis or method.

A closer investigation discloses, however, that an "unbiased estimate" is but another rakish label pasted over what Thiele and Tschuprow called "presumptive values" (or estimates). According to Dr. Irwin "an estimate is 'unbiased' whenever its mathematical expectation equals the quantity estimated." This is exactly the same as Tschuprow's old definition of a "presumptive value." Moreover, since the moderns here express a rather prejudiced or biased preference for the mathematical expectation (or first-order semi-invariants), the followers of R. A. Fisher have raised their prejudice to a metaphysic, as it were, in considering their personal bias as unbiased.

It is also well to bear in mind that some years prior to Fisher the Danish actuary, Bertelsen, gave the identically same formula as Fisher for $'\lambda_4$ (or k_4). But Bertelsen showed also that this formula was incompatible and in many instances would not correspond to real values of the individual observations in the universe. He supplied therefor another estimate from which this defect was removed. A numerical example verifies that the substitution of Fisher's k_4 "cumulant" in the rigorous a priori formula for the dispersion (or variance) yields a negative result, or an imaginary value for the standard deviation, while Thiele's and Bertelsen's estimates yield positive and hence compatible results.

ABSTRACT OF PUBLIC LECTURE

Wednesday, July 20—The Life of Augustin Cournot: His Thought, His Philosophical Tendencies, and His Work, RENÉ ROY, Professor of Mathematical Economics, University of Paris.

Although economic thought has assumed in our times an importance which it never had before, it had its pioneers among the Greek philosophers, the scholars of the middle Ages, and the Mercantilists of the seventeenth and eighteenth centuries. It was only in the latter half of the eighteenth century, however, that a true school of economists, was formed, the Physiocrats, who conceived the idea of "economic laws." Like the Mercantilists, however, they were more occupied with solving concrete problems than with isolating abstract concepts.

Augustin Cournot, on the other hand, proposed to apply to economic questions the methods which had been tested in other domains, and it is for this reason that he may be considered the founder of scientific economics. His ideas were fully set forth in 1838 in his *Recherches sur les principes mathématiques de la théories des richesses,* of which we celebrate the centenary today.

The Intellectual Influences. Antoine-Augustin Cournot was born August 28, 1801 at Gray, in Franche-Comté. He studied at the École Normale. He was secretary for ten years to the Marshal Gouvion Saint Cyr, became a bachelor of law in 1827 and a doctor of sciences in 1829, and published scientific papers on astronomy, mechanics, and mathematics. He attracted the approval of the mathematician Poisson who launched him on a brilliant educational career. After serving at the universities of Lyons and Grenoble, he became in 1838 inspector general of studies and took up his residence at Paris.

In addition to his scientific studies, Cournot was widely read in the philosophers, from Plato to Kant. The greater part of his published works were treatises on philisophy, prepared

with the same precision and the same dryness as a scientific work.

Of all the subjects to which he applied himself, the most striking was without doubt the theory of chance, on which he published a treatise in 1848. This predilection for the phenomena of chance had a great influence, not only on his philosophical concepts, but also on his ideas as to the social sciences. He distinguished "quantitative sciences" like physics, which required few experimental verifications, and "sciences of combinations," like the social sciences, which required permanent records of facts. Cournot thus seems to have been one of the first thinkers who foresaw both the statistical orientation to be given to scientific research since the middle of the nineteenth century and the possibility of extending this concept to sociology, which, in his time, was hardly considered a science. It is nevertheless essential to note here that for Cournot the notion of probability resides in the very nature of things apart from any philosophic concept, while for Laplace, who, in his works, had especially considered errors of observation, this recourse to probability meant only the imperfection of our knowledge; it seems that in this regard the evolution of our ideas since that time has confirmed the view of Cournot, as witnesses the success of probability in all fields.

The Work of Cournot. After his early scientific papers, Cournot devoted himself essentially, on the one hand to philosophy, on the other hand to economic problems. As a link between these two essential currents of his thought, we may profitably choose the work in which he developed in 1848 his concepts of the theory of chance, *Exposé de la théorie des chances et des probabilités.* Moreover, in his philosophical works he frequently reverts to economic problems and shows their place among other social phenomena.

His intellectual culture was of a high degree of universality, and all his reading was enriched by long and profound meditation, in the course of which he had the occasion to exercise his critical spirit with rare sagacity. Everyone agrees that his most important contributions have been the Law of Demand, the Theory of Monopoly, and Foreign Exchange.

The Influence of Cournot. In general the works of Cournot were far from meeting the success which was their due,

and as we have already noted this disrepute may be attributed in part to the dryness of his style and the barrenness of his exposition. The originality of his concepts, even their daring, sometimes also repelled his contemporaries, and it is not surprising indeed that Cournot suffered the disfavor of all innovators.

In the economic field, in particular, the reception by his contemporaries was singularly deceptive; the *Recherches* of 1838 appeared in the midst of general indifference, and twenty-five years later Cournot revealed his bitterness in the preface of his *Principes de la théorie des richesses,* which, although it used no mathematics, had hardly more success.

Nevertheless his work gradually made its way. Sooner abroad than in France it aroused admiration and formed the starting point of a new orientation of economic studies. Although no French critic took account of the *Recherches* before 1873, it had already been quoted by Bocardo, Jevons, and Walras. The influence of Cournot was therefore established, and it showed itself very profound, since it was the sole generator of the series of publications using the new concepts which gravitated around the Lausanne School. As to what particularly concerns the tendencies shown by the development of econometric researches, the influence of Cournot has proved truly fertile, because the originality of his thought was especially marked, in the first place by systematic recourse to mathematical analysis for the presentation of economic phenomena, and in the second place by the statistical concepts, that is to say, precisely by the two kinds of problems with which econometrists are concerned. In a more general way, we may say that his name remains closely associated with the modern scientific spirit, which constantly appeals to the idea of probability, and which is opposed in some wise to the rigidity of the ancient logic founded by Aristotle. In the mathematical expression of scientific laws this distinction appears in a particularly clear way, when we note the growing use of "stochastic" (probability) relations instead of functional relations.

If Cournot was not a builder of cathedrals, he personifies on the other hand the accomplished type of modest and disinterested scholar who brings his stone to the edifice, and it is

not surprising that with time his influence in the scholarly world has only grown.

Measuring thus the import of a work which, for a century, serves as a guide to experts in scientific economics, we may congratulate ourselves in concluding that at its birth scientific economics had for its champion a thinker who, by the universality of his culture and the depth of his insight, knew how to bring to the study of economic phenomena the sense of fine distinctions and geometric rigor, both indispensable in such a field.