Stanford University, Calif.
October 1, 1927

Dear Dr. Frisch –

I was delighted to receive your letter and to learn that you are so near. I shall be happy to see you when you visit Stanford. If you will let me know the time of your arrival I will meet the train. My office is in Room 74, in the mathematics department; my telephone is Palo Alto 900, Local 74. Dr. Holbrook Working and Dr. J. S. Davis will also want to see you.

Very sincerely,

Harold Hotelling

Telephone / Telegrams } HARPENDEN 21.

Railway Station: HARPENDEN (L.M.S.)

ROTHAMSTED EXPERIMENTAL STATION,
HARPENDEN, HERTS.

Director:
SIR JOHN RUSSELL, D.Sc., F.R.S.

STATISTICAL DEPT.
R. A. FISHER, Sc.D., F.R.S.
J. WISHART, D.Sc.
J. O. IRWIN, M.A., M.Sc.

August 2, 1929

Dear Dr. Frisch —

Thank you for the copy of your paper, "Sur une formule générale de moyenne," with its highly interesting and general result. I have enjoyed following out some of the special cases, such as $a_K(x) = x^K$.

With sincere regards,

Harold Hotelling

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

Feb. 6, 1930

My dear Dr. Frisch:

I have just finished reading with the greatest interest the two reprints I received from you yesterday. The paper "On Approximation to a Certain Type of Integrals" I am loaning to a student who is working with such approximations, Mr. J. A. Reid. I have been greatly struck by the generality and value of your generalization of Rolle's and Hadamard's theorems, and by this use of them.

The treatment of statistics by means of matrices is an interesting subject, at which I have made one or two inconsequential attempts. I usually begin with a matrix x of observations, the element x_{ij} being the jth individual's value of the ith variable. Then xx' is a symmetrical square matrix of product moments and sums of squares. The study of correlation between two variables can be extended in this way to a "correlation" between two sets of variables; for example to the relation between the crops x of a group of agricultural products and the prices y. The matrices xx', yy', xy' and yx' would all come into consideration. I like your discussion of the "scatter coefficient," and believe that it will eventually prove highly important in practice.

6/2 - 1930

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

As you indicate on pages 96 and 97, your work raises a new set of problems in the theory of sampling distributions. I wonder whether light will not be thrown on them by the paper of R. A. Fisher, more recent than yours, "The General Sampling Distribution of the Multiple Correlation Coefficient," in the Proceedings of the Royal Society. A, vol. 121 (1928), p. 654.

With sincere regards,

Harold Hotelling

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

February 12, 1931.

Professor Ragnar Frisch,
Yale University,
New Haven, Connecticut.

My dear Frisch:

Your opinion on a question of terminology I should value very highly. The functions which Thiele, in his English "Theory of Observations" called "half-invariants," have been called "semi-invariants" by most recent writers. However in the important book of Whittaker and Robinson the less difficult word "seminvariants" is used instead.

All these words are open to several objections. They are long and uneuphonious; the most frequently used of them, "semi-invariants," is extraordinarily hard to pronounce; moreover its pronunciation is ambiguous. Further, its use has been pre-empted in other portions of mathematics. I have known of cases of students in search of information about these statistical functions working diligently through White's book on cubic curves because the word semi-invariant is used to describe properties of these curves. In fact, its use in pure algebra and geometry is likely to give rise to awkward interference. None of the three names has yet found its way into the English dictionaries with the definition in probability and statistics.

In spite of the strong and natural conservatism in matters of language, a shorter and simpler new word seems in this case desirable. The number of people who are writing about these quantities is still not too large to preclude the possibility of general agreement on a better term. I venture to suggest "cumulants". This word indicates an important property of the functions; it is smooth and unambiguous im pronunciation, and it represents a gain in brevity. To be sure it was formerly used in a very few places to indicate the determinants now always called continuants; the obsolete and insignificant character of this usage is indicated by the treatment of the word in the dictionaries. To be specific, I propose to call the r^{th} cumulant the r^{th} derivative, when $t = 0$, of

$$\log \int e^{tx}\, dF(x).$$

The word "cumulant" in this sense is approved by R. A. Fisher and by C. C. Craig. When I used it at a joint meeting of the American Statistical Association and the American Mathematical Society at Cleve-

12/2-1931

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

land recently, the comments indicated favorable interest in the word "cumulants." If it appears that there is a possibility of its use becoming general I shall introduce it in my treatise on statistical theory. I should much appreciate any remarks you care to make on the merits of the word and on the possibility of its general adoption.

Very sincerely yours,

Harold Hotelling

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

February 26, 1931

Dear Dr. Frisch:

Thank you for your letters of February 21, and particularly for your suggestion of "half-invariants."

In the treatise I shall discuss partial and multiple correlation, both in the usual way and with reference to the sampling distributions of the quantities which are used. Most of these distributions have been discovered by R. A. Fisher, some by J. Wishart. Fisher's discovery of the exact general distribution for the multiple correlation appears in the Proceedings of the Royal Society of London, A, vol. 121 (1928-9), pp. 654-673. A list of his works is in the back of the third (1930) edition of his "Statistical Methods for Research Workers." These sampling distributions I am trying to elucidate in the book. Whether to use the matrix method of presentation I am undecided; I was very enthusiastic about it six or seven years ago, and have used it repeatedly in my classes and in short unpublished talks at the meetings of the San Francisco Section of the American Mathematical Society. Your paper in the Nordic Statistical Journal seems to me to serve a very useful purpose. I do not believe that you need worry about most American statisticians not knowing it, since most of these gentlemen know nothing

26/2 - 1931

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

whatever of a theoretical nature.

As I shall probably be at Columbia University after this year, I shall look forward to seeing you more frequently.

With cordial good wishes,

Harold Hotelling

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

May 28, 1931

My dear Frisch -

I am glad to receive your letter from Minneapolis and to learn that you have arrived there safely. If you are still there in the last week in August we shall meet, as I shall probably be in Minneapolis at that time. The only difficulty is that we may not be able to leave here soon enough, owing to the necessity of disposing of our house on a falling and timid market. We hope to camp in Glacier and Yellowstone Park.

Fisher's paper to which I referred is "Tests of Significance in Harmonic Analysis," Proceedings of the Royal Society, A, Vol. 125 (1929), pp. 53-59. It gives a most beautiful derivation of an exact sampling distribution which takes account both of the selection of the greatest amplitude and of the sampling errors in estimation of variance.

Hoping to see you this summer I am

Most cordially yours,

Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

April 8, 1932

Dear Dr. Frisch --

It was a great pleasure to receive from you today an autographed copy of your new book. I shall read it with much interest, having already read Professor Irving Fisher's essay on a similar subject in the memorial volume for John Bates Clark.

Incidentally Professor Clark, whose eightieth birthday was celebrated by that volume, recently reached his eighty-fifth birthday, which was celebrated by a dinner in the Faculty Club given by the graduate students, at which he and his wife were guests, and spoke. For me it was of very great interest, as I had never seen him before.

I trust that you have fully recovered from the broken leg which I heard you incurred some time ago.

There seems to be a general sentiment, in which I for one concur, that you should be editor of the new journal "Econometrica" which Mr. Cowles will subsidize.

With cordial good wishes I am

Sincerely your friend

Harold Hotelling

P. S. I am sending you separately some recent reprints.

524 Riverside Drive
NYC

B. O. I c
evs. nr.
261 A

American Association for the Advancement of Science
And Associated Societies

SUMMER MEETING, SYRACUSE, N. Y.
JUNE 20 TO 25, 1932
WINTER MEETING, ATLANTIC CITY, N. J.
DECEMBER 27 TO 31, 1932

OFFICE OF THE SECRETARY OF SECTION K

Columbia University, New York City

May 26, 1932

Dr. Ragnar Frisch
c/o Professor Irving Fisher
460 Prospect Street
New Haven, Conn.

Dear Dr. Frisch:

I am glad to learn that you are to be in this country. At the request of Professor Fisher I have been arranging for discussion of the papers to be presented at the joint meeting of Section K and the Econometric Society at Syracuse. Your paper is scheduled for 9:15 a. m. Wednesday, June 22. Dr. S. S. Wilks, a very keen National Research Fellow in mathematical statistics, has agreed to discuss your paper. Will you please send him, as much as possible in advance of the meeting, a copy of your paper, or a synopsis of your remarks? His address is 401 West 118th Street, Apartment 63, New York City.

Looking forward to seeing you I am,

Truly yours,

Harold Hotelling

Harold Hotelling

Columbia University
in the City of New York
FACULTY OF POLITICAL SCIENCE

June 2, 1932

Dear Dr. Frisch:

Thank you for your very kind letter of May 19 regarding my paper on Edgeworth's taxation paradox and the nature of demand and supply functions. This paper has been accepted for publication in the Journal of Political Economy, though on account of its length there may be some further delay.

I hope to see you if when in New York you can find time to drop in at my office, 501A Fayerweather Hall, and in any event at Syracuse.

Truly yours,

Harold Hotelling

Harold Hotelling.

Uregistrert vedlegg til brevene fra H. Hotelling til R. Frisch [1932].

Mr. Alfred Cowles, 3rd,
Mining Exchange Building,
Colorado Springs, Colo.

I shall be glad to have you include my name in the list of the Advisory Editorial Board of the Econometric Society.

(signed) Harold Hotelling

[copy to Irving Fisher
and Ragnar Frisch]

P. S. The pressure of reviewing books and articles is now so great that I will not be able to review more than one or two papers a year at the most for Econometrica.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

January 27, 1933

Dear Professor Frisch -

Thank you for your letter of the 13th. I quite agree with you that standard errors in themselves are not adequate measures of accuracy. This is particularly the case when, as is still done in many cases, the ratio of a deviation to its standard error is treated as normally distributed, and interpreted in terms of probability by means of a table of the normal probability integral. For this particular inadequacy, which for small samples may lead to serious fallacies, the use of Student's distribution with the correct number of degrees of freedom is a great improvement. To be sure, even Student's distribution is derived from the assumption of a normal distribution of the population sampled; but it seems from general considerations likely that even for samples from non-normal populations Student's distribution gives very close approximations for quite small samples.

Whether or not standard errors are used for a particular questions, it seems to me essential to have some sort of interpretation of inferences drawn from statistical data in terms of probability. This of course may be done in a great variety of ways, apart from the use of standard errors, e. g. by means of the chi test, the exact distribution of the correlation coefficient (whose standard error is highly misleading), exact tests in periodogram analysis and the analysis of variance, etc. Often rough tests, using the binomial distribution, are suitable for finding whether an excessive number of deviations are of one sign, so as to disprove the hypothesis that they are equally likely. I believe that the inferences whic are drawn by eye and by judgement, from graphs and from the examination of data in detail, may quite generally be thrown into these forms. Free-hand methods and graphs are valuable for preliminary exploration, and for exhibition, but any valid conclusions to be drawn from them can also be obtained in a more objective fashion, with a proper use of the canons of statistica inference, which are also capable of yielding conclusions not t be deduced from graphs by visual examination.

But the need of ultimate attention to interpr tations in terms of probability need not at all stand in the wa of getting immediate first estimates of things like ratios of marginal utility. The critical examination by means of probabil is merely the necessary later step.

I congratulate you on your excellent editing of the first issue of Econometrica, which recently reached me. With cordial good wishes,

Harold Hotelling.

Thanks! I think – much the same –

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

March 9, 1933

Professor Ragnar Frisch
Storgaten 9
Oslo, Norway.

Dear Professor Frisch:

Thank you for your letter of February 20th, which just arrived, with enclosures.

The manuscript of Raymond Garver on "The Edgeworth Taxation Phenomenon" to which you refer in your letter is not among the enclosures received. I suspect that it may by mistake have been sent to Mr. Nelson, while material intended for Mr. Nelson was enclosed with the letter to me. Accordingly, I am sending Mr. Nelson all the enclosures with your letter, which are the following:

Letter from you addressed to Mr. Nelson.
Carbon copies of letters from you to Henry Schultz and J. Tinbergen.
Manuscripts by Edward Theiss and J. Tinbergen, stamped ECONOMETRICA ACCEPTED FOR PUBLICATION.

I am having this material registered.

Very sincerely yours,

Harold Hotelling

Harold Hotelling

Copy of this letter to
Mr. Wm. F. C. Nelson
Cowles Commission for Economic Research
Colorado Springs, Colo.

Columbia University
in the City of New York
DEPARTMENT OF ECONOMICS

8 October 1933

Dear Professor Frisch:

Thank you for your letter of 21 September; thank you also for the two interesting reprints you sent me.

The book on statistical theory on which I have been working for several years will perhaps be out in another year or two. I have had several chapters of it mimeographed for the use of students in the statistics course which I conduct each year. The supply of these is now exhausted, but I expect to get out a new edition of them soon. If you would like to have a set of these mimeographed sheets, I will be glad to send you one when the new supply is available.

With cordial good wishes I am

Sincerely your friend,

Harold Hotelling

P. S. Mr. Sune Carlson, who is here from Stockholm on a Rockefeller fellowship and is studying mathematical economics and statistics, yesterday spoke of hearing you with great interest when you spoke at a meeting at Stockholm.

Could you get one second hand, I should be glad to pay for it.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

February 11, 1934

Dear Professor Frisch:

Thank you for your kind letter of January 26. I shall as you suggest keep an eye out for good econometric papers.

The mimeographed material I am glad to present to you with my compliments. You may find material of interest to you in some of it. I should be glad to have any suggestions you may care to make regarding it. Please disregard the numerical errors in some of the examples, which I now have listed with the help of students.

The Philadelphia meeting was a very good one, which you would have enjoyed.

I am enclosing for the abstracts to be published in Econometrica one dealing with my principal contribution since January 1, 1933, the only other being a small note in Econometrica. There are also reviews, but I do not think you will want to publish abstracts of these. You may perhaps have seen the review which I published in the last Journal of the American Statistical Association of Secrist's book. I said it was rotten. This moved him to write a letter to the editor, several times as long as the review, with a demand that it be published in full. To this I am writing a short reply, to which also he is replying. The point, I suppose, is that all reviews should be favorable. But we will never get good work in our subject until there is really critical appraisal.

Most cordially yours,

Harold Hotelling

P. S. I believe it a very good idea to run the abstracts as you plan. But the system of designation by Roman numerals does not seem to fit the abstract enclosed herewith.

[illegible]

Columbia University
in the City of New York
DEPARTMENT OF ECONOMICS

August 20, 1934.

Professor Ragnar Frisch,
301 Mining Exchange Building,
Colorado Springs,
Colorado.

My dear Professor Frisch:

I am sorry that I shall not see you at all while you are in this country. I am spending the summer at Medina, Washington, and do not expect to return to New York before about September 22.

I am glad my paper on the "Demand Function Subject to a Limited Budget" is to appear in Econometrica, and hope it will come out as soon as possible, since it bears on a great deal of statistical work now being done.

Cordially yours,

Harold Hotelling

HAROLD HOTELLING.

HH:S

WORK OF HAROLD HOTELLING RELATING TO MATHEMATICAL ECONOMICS AND TO THE THEORY OF STATISTICS.

1. A general mathematical theory of depreciation, J. Amer. Stat. Assn., vol. 20 (1925), pp. 340-353.

2. The distribution of correlation ratios calculated from random data, Proc. Nat. Acad. Sci., vol. 11 (1925), pp. 657-662

3. An application of analysis situs to statistics, Bull. Amer. Math. Soc., vol. 33 (1927), pp. 467-476.

4. Differential equations subject to error, and population estimates, J. Amer. Stat. Assn., vol. 20 (1927), pp. 283-314.

5. Spaces of statistics and their metrization, Science, vol. 67 (Feb. 10, 1928), pp. 149-150.

6. The physical state of protoplasm (with L. G. M. Baas Becking Henriette v. d. Sande Bakhuyzen), Verhandelingen der Koninklijke Akademie ~~te~~ van Wetenschappen te Amsterdam, Afdeeling Natuurkunde (Tweede sectie), vol. 25 (1928). Pp. 28-31, containing a contribution to the theory of statistics, by Harold Hotelling.

7. Applications of the theory of error to the interpretation of trends. Proc. Amer. Stat. Assn., March, 1929, pp. 73-85. (With Holbrook Working.)
8.
8. Stability in competition. Economic Journal, vol. 41 (1929), pp. 41-57.

9. The consistency and ultimate distribution of optimum statistics. Trans. Amer. Math. Soc. vol. 32 (1930), pp. 847-859.

10. Recent improvements in statistical inference, Proc. Amer. Stat. Assn., March, 1931, pp. 79-89.

11. The economics of exhaustible resources, J. Pol. Econ., vol. 39 (April, 1931), pp. 137-175.

12. Causes of birth rate fluctuations. J. Amer. Stat. Assn., vol. 26 (1931), pp. 135-149. (With Floy Hotelling).

REVIEWS

1. E. E. Day's "Statistical Analysis," J. Amer. Stat. Assn., vol. 21 (1926), pp. 360-363.

2. L. I. Dublin's "Population Problems," J. Amer. Stat. Assn., vol. 21 (1926), pp. 503-505.

3. R. A. Fisher's "Statistical Methods for Research Workers," J. Amer. Stat. Assn., vol. 22 (1927), pp. 411-412.

 Harold Hotelling

4. Ditto. 2nd ed. Same journal, vol. 23 (1928), p. 346.

5. Ditto. 3rd ed. Same journal, vol. 25 (1930), p. 381.

6. W. P. Elderton's "Frequency Curves and Correlation," Bull. Amer. Math. Soc., vol. 34 (1928), pp. 515-516.

7. Cournot's "Mathematical Principles of **** Wealth," Amer. Math. Monthly, vol. 35, (1928), pp. 439-440.

8. G. C. Evans' "Mathematical Introduction to Economics," Amer. Math. Monthly (1931)

9. Ditto. J. Pol. Econ., vol. 39 (1931), pp. 107-109.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

February 21, 1935.

My dear Frisch:

Your book on "Confluence Analysis" arrived today, and I have already read a large part of it since receiving the volume. Please accept my thanks, and also my congratulations. It contains a large number of valuable things. Your "tilling technique" seems particularly ingenious.

In a footnote you express some doubt as to the rapidity of convergence of my iterative process for determining principal components in case two of the roots are nearly equal. You are quite right. If the characteristic root sought is only slightly greater than another, a large number of repetitions of the iteration will be required. However I am now working on a method of accelerating the convergence which will very largely remove this practical difficulty. This method, which has already shown its efficacy in a number of examples, consists in the first instance of squaring the matrix of correlations, and recalling that the characteristic roots of the squared matrix are the squares of those of the original matrix, while the corresponding vectors have proportional components. With the new matrix, each iteration is exactly equivalent to two with the old. But the squared matrix can itself be squared, and when this is done repeatedly, we can have an iterative process converging at 4, 8, 16, 32 or 64 times the original rate. Further improvements in the process are undoubtedly possible. For example, in getting the second and higher components, it is not necessary to square repeatedly the reduced matrix, but to get the desired power from the corresponding pwer of the original matrix, using the algebra of matrices, and the numerical arrangement of this we have made quite elegant.

I am looking forward to seeing you in Colorado Springs this summer at the anticipated meeting of the Econometric Society. I have about decided to spend the summer there.

With cordial good wishes, I remain

Sincerely yours,

Harold Hotelling

Hans von Trebert

Nordische Hotelly "Reval Stern... "

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

July 15, 1935

Professor Ragnar Frisch
University of Norway
Oslo, Norway

My dear Frisch:

I received today your letter of June 27, and the manuscript of Lerner and Singer which I am returning herewith. I agree both with your observation that the paper is excessively long, and with Dr. Zeuthen's remarks.

The paper consists chiefly of an elaboration of two points: (1) If a seller can have a monopoly in a sufficiently extensive market he will choose this in preference to a similar market in which there is competition; (2) Separate monopolists in unrelated markets do not influence each others' exact locations. These propositions would seem too obvious to require the very extended demonstrations given, especially as these demonstrations rest on a form of demand curve far more special than is necessary. To assume that demand is independent of price below a certain price, and is zero above that price, is to introduce a peculiar type of discontinuity and to specialize the demand curve in a way that, instead of throwing new light on the modification of competitive conditions resulting from elasticity of demand, actually obscures the true state of affairs.

The manuscript is devoted largely to attacking the conclusion contained in the paper on Stability in Competition (by Harold Hotelling, Economic Journal, March, 1929), that there is a tendency for competitors to take up locations so close together as to cause unnecessary transportation, and otherwise to imitate each other more than an adjustment to a social optimum would bring about. But the conclusion attacked is actually verified by the results of the authors' calculations presented in their Figure 10. For n sources of supply with linearly and uniformly distributed demand, it is plain that the criterion of minimum transportation would require that the sources be placed at the centers of n equal segments into which the whole interval is divided. This condition is not satisfied by any of the solutions presented in Figure 10, save only that corresponding to monopoly.

In a preponderance of cases, location is determined once and for all, permanently, or for so long a period at least that it can be regarded as fixed during the process of determining selling prices. An entrepreneur in deciding upon his location will, it is true, take careful account of the probable price and volume of sales he can obtain; but these, under competitive conditions, are arrived at by successive approximations (if at all), with the competitors assuming fixed locations. This was the situation assumed in the 1929 article. It is very different from the free simultaneous variation of price and location from moment to moment assumed by the present writers.

15/7-1935

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

They do not perceive the difference, and consequently misquote, on p. 3, what they state to be the eighth assumption in the 1929 article. In that article, B considers A's price fixed when setting his own price, and he considers A's location fixed, but not A's price, when determining his own location. At this early stage, B assumes that prices will be fixed competitively, but in a manner depending on the location B is about to choose. This confusion is involved in the reasoning leading to the conclusion on p. 10, that B "will in no case behave in the way suggested by Hotelling." To obtain that conclusion, they assume that A's price, as well as his location, remains unchanged in the face of B's competition -- a rather remarkable assumption -- while assuming also that B is free to change both his price and his location.

The 1929 article was concerned with a market in which competition was assumed to exist, not with a state in which a prospective entrepreneur had a choice between monopoly and competition. Under competition of the active sort considered, elasticity of demand is of secondary importance. When a seller surrounded by active competitors raises his price while they keep theirs fixed, his loss of business consists partly of a decrease in total demand, and partly of a transfer of business to his competitors. This transfer of business seems in all ordinary circumstances the important part of the loss, and this was what was considered in the 1929 paper. The introduction of elasticity of demand will of course change the quantitative results obtained for purposes of illustration; but if the demand function introduced is continuous and monotonically decreasing over a whole range of prices including as a subinterval the range considered, it does not appear that the qualitative conclusions reached in the 1929 paper need be altered in any particular.

To examine this subject, it would be appropriate to use a demand function continuous and monotonically diminishing in an infinite interval; for example, $q = 1/p$. It would also be desirable to carry out the investigation in at least two dimensions, since in the one-dimensional case a slight discontinuity, noted in a footnote of the Economic Journal paper, complicates matters, and since the most realistic assumptions must introduce a plurality of degrees of freedom.

In certain parts of the manuscript, as in the lower part of p. 7, the reasoning is very difficult to follow. The conventional use of lower-case letters for magnitudes would give a somewhat better appearance than the indiscriminate use of capitals for magnitudes as well as persons and points.

Very sincerely,

Harold Hotelling

Temporary address until Sept. 2: 42 West Cache la Poudre, Colorado Springs, Colo.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

October 1, 1935

Professor Ragnar Frisch
University of Norway, Oslo

Dear Professor Frisch:

I am returning herewith the manuscript by Dr. Gerhard Tintner received this morning. It contains several interesting ideas, and merits attention. The result expressed in equation (44) and the subsequent sentence, and the use of derivatives of order higher than the first, are particularly novel. It appears to me that it should be published in approximately its present form.

In spite of the very considerable merits of the paper, there are many details requiring revision. The English could be considerably improved. Certain words such as "similarly" are consistently misspelled. At the bottom of p. 4, "determined" should be changed to "determinate." I have not attempted a complete list of such points. One rather serious error of expression is the use on pp. 6 and 9, and elsewhere, of "hyperbolic function." This expression is universally understood to refer to certain combinations of exponentials, such as sinh x, cosh x, etc. Dr. Tintner uses it erroneously for a constant multiple of 1/x.

In discussions such as this, "Case I" and "Case II" would ordinarily refer to different sets of hypotheses. In this paper they refer to different forms of the same equations, related by a purely mathematical transformation. I suggest that these be called "forms" instead of "cases" throughout.

On pp. 1 and 13 the author appears to think that in using derivatives of order higher than the first he has treated "the general case" of demand functions. But demand functions have been treated by C. F. Roos, G. C. Evans, and I think others, which involve integrals; and other types of functionals are also possible. Hence it is not proper to represent complete generality as attained merely by the use of a sequence of derivatives.

Near the bottom of p. 13 it is suggested that second-order conditions are not of much consequence. This is distinctly erroneous. Numerous economic arguments turn on inequalities such as these expressing the rising supply curve and the falling demand curve; these inequalities on the first derivatives of demand and supply functions are derived essentially from inequalities on the second derivatives of profit, utility and cost functions. Cf. "Demand Functions with Limited Budgets," Econometrica, vol. 3, p. 66. The final paragraph on p. 13 should be modified in the light

1/10-1935

of the work of C. F. Roos, particularly "A Mathematical Theory of Competition," Amer. J. of Math., vol. 47 (1925), p. 163.

The transformation of the manuscript to a form suitable for publication will be difficult because it is single-spaced. It will probably be necessary to re-copy it in order to incorporate the changes in legible fashion.

With these modifications, chiefly verbal in character, together possibly with a little shortening, the paper would seem to make an interesting contribution.

Very sincerely yours,

Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

January 10, 1936

Dear Frisch:

I am greatly obliged to you for your letter and for the mimeographed outline of the lectures given by Koopmans. While I have not yet had an opportunity to read through this material, I take it to be an excellent presentation of modern sampling theory, which is of course the central thing in the theory of statistics.

Thank you for your kind suggestion that my remarks on Generalized Multiple Correlation for Pairs of Sets of Economic Variates be published in Econometrica. As a matter of fact, I have not written out these remarks, but I have sent an abstract to Roos. The theory on which these applications is based is embodied in a long paper, not primarily economic in character, which I am just now completing.

With cordial regards, I remain

Sincerely yours,

Harold Hotelling

Hotelling

Columbia University
in the City of New York
FACULTY OF POLITICAL SCIENCE

March 20, 1936

Dear Frisch:

Your plan of holding a session of the Econometric Society in connection with the international mathematical congress is very appealing; the occasion would seem to be a very suitable one for getting together. I am not familiar with the machinery of these congresses; it may be that an econometric program held as a part of the congress itself would be equally successful, and less troublesome to arrange. This is a matter of administration, on which I cannot express an opinion without knowing more about the organization and general scheme. One question relevant to this would be as to whether other societies interested in applied mathematics are meeting with the congress. Econometrics would seem to be in about the same category as mathematical physics from this standpoint.

It is most kind of you to suggest a dinner meeting at your home. I should be delighted to attend, except that I shall apparently have to give up, with great reluctance, the idea of going to Oslo this summer, as I have on hand work which I cannot well leave until it is completed -- namely the writing of the treatise on statistical theory on which I have been engaged so long.

With the most cordial thanks and regards, I remain

Sincerely yours,

Harold Hotelling

P. S. Roos, Evans, etc., might well be asked to give papers on the Econometric program; also, of course, European econometricians.

Columbia University
in the City of New York
FACULTY OF POLITICAL SCIENCE

April 19, 1936

Professor Ragnar Frisch
Slemdalsveien 98
Oslo, Norway

Dear Professor Frisch:

Many thanks for you kind letter of March 31. I greatly appreciate your congratulations.

The suggestion you communicated last fall to the council members regarding the election of the editor-in-chief for a fixed term of years seems eminently reasonable. I have not yet learned whether anything has been done about it; if it has not been acted upon I will undertake to lay it formally before the Council.

What would you think of the following proposal? Suppose each member were asked, either by circular or through a general announcement in Econometrica, to send in immediately at the close of each calendar year a list of contributions to econometrics he had published <u>during that year</u>; then these could be published in Econometrica as an annual bibliography. This would be of interest to readers, and would also be an inducement to membership in the society, in order to get one's bibliography published. Perhaps the request to send in the bibliography could be on a printed slip accompanying the annual bill for dues sent out in the autumn -- unless this would have an adverse effect in causing postponement of payment of dues. If anything besides the bill for dues were sent out at this time, there might be included both the bibliography notice and a suggestion to make nominations for fellowship.

Cordially yours,

Harold Hotelling

2°

Carmel, California
July 21, 1936

My dear Frisch:

Thank you for your letter of July 2. I am glad that you, like me, are enjoying a vacation in a place of natural beauty.

If Roos wrote that I was making a historical study of statistics he was mistaken. I am now, as I have been for several years, ~~xxxx~~ engaged in writing a book on the theory of statistics in relation to probability, a rather encyclopedic and comprehensive treatise. The history of the subject is not my primary concern. Hence I can scarcely prepare a historical article of the type you suggest, at any rate in time for publication a year hence.

To survey current developments in mathematical statistics is the object of an enterprise begun several years ago in behalf of the English Royal Statistical Society by J. O. Irwin, whose annual surveys have come to be read everywhere with great interest. They are very ably conducted. Meanwhile, independently, the need for such a survey occurred to several members of the American Statistical Association; it was proposed within the Association to publish an annual survey of current developments in statistical technique. When this proposal came to me, I called attention to Irwin's excellent work, the duplication of which would be a waste of effort; I therefore suggested that his reports, possibly with American collaboration in their preparation, be published both in the Journal of the American Statistical Association and in the Journal of the Royal Statistical Society, or else that reprints from the same type be circulated to members of both organizations. The American Statistical Association thereupon commissioned Paul R. Rider, who was leaving for a year in England, to negotiate with Irwin and the Royal Statistical Society for some such arrangement. When last I heard from Rider a few months ago, and from Irwin somewhat earlier, they seemed to have come to some sort of an understanding that Rider should undertake to review new statistical methods of particular applicability to economics, while Irwin handled the rest. However the theoretical work in economic statistics is on the whole very trivial in comparison with the more general work in the theory of statistics; this fact was so obvious both to Irwin and to Rider that they may make some change in the arrangement. In any case, there is a large amount of work involved.

With cordial regards, I remain

Sincerely yours, Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

October 29, 1936

Dear Frisch,

Many thanks for your letters of the 8th of October. In accordance with your suggestions I am writing Roos, and I enclose herewith a copy of my letter to him.

I am much interested in your remarks on the doubts regarding Irwin's annual reviews of statistical progress. These reviews have been of great value to me, and from several sources I have heard only praise for them. It is of course easy to understand that not all of the rapidly increasing volume of literature of theoretical statistics could be read with really critical attention, and that some points might escape a reviewer.

The surveys that have so far appeared in Econometrica seem to me also to be excellent pieces of work, and it is to be hoped that there will be more of the same character. Really careful and critical discussion of published contributions should be of immense value both in developing and in disseminating our subject.

I hope that the writer on seasonal indices is up to his job. There has been an immense amount of writing and work on this subject which is quite futile, largely because of the authors' ignorance of statistical theory. The method of link relatives in particular seems to me immensely laborious, inaccurate, and altogether objectionable.

Most cordially yours,

Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

May 28, 1937

My dear Frisch:

Your idea of national chapters of the Econometric Society, with meetings to be reported in Econometrica, seems excellent. The American Statistical Association has I think gained a great deal of life from the activities of the local chapters, and the reports of their meetings make interesting reading in the Journal.

In some places it may be more expedient, instead of national chapters, to have regional ones, regardless of political boundaries. Thus the great distances in the United States might sometimes lead to the organization of numerous local chapters. On the other hand, in small countries, chapter activities might well overlap national boundaries.

It seems to me that whenever a group of members in any country or region wishes to organize a chapter, they might well proceed to do so, and that this chapter might then be accepted, or chartered, by the Econometric Society as a matter of course. The organization's charter might well be made contingent on due notice in advance of organization having been given all members of the Econometric Society living in the region in question, so that there could be no accusation of a minority group having unexpectedly seized control. But this would be a very easy requirement to fulfil, and should cause no difficulty.

I hope to see you when you come through New York. I shall not be able to go to Colorado this summer. Please let me know when you are coming through this city, and I will arrange to meet you; otherwise there is a good chance of my being out of town in hot weather.

With cordial regards, I remain

Sincerely yours,

Harold Hotelling

Ranlagges.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

April 6, 1938

Professor Ragnar Frisch
Vinderen, Oslo, Norway.

My dear Frisch:

I am greatly indebted to you for your careful reading of my paper and for your well-directed comments. I have sent it on to Mr. Leavens by air mail, after making some revisions suggested by your remarks. These revisions consisted in part of the correction of some bad slips, such as the inadvertent implication that Marshall meant by his supply curve a marginal cost curve, and the unfortunate inclusion of the word "implicit" in referring to your assumption in "New Methods of Measuring Marginal Utility" that the marginal utility of the particular commodity considered is a function only of the quantity of that commodity. Your statement of this assumption on p. 8 is of course perfectly clear; I regret that I overlooked this explicit statement in first reading the book. I trust that the matter is rectified by deleting the word "implicit" from my manuscript; if you think it better I shall be glad to change the form of reference or delete it, as you think proper.

Several of your comments, and also those of Francis McIntyre, make it evident that I failed to clarify certain fundamental points adequately. I therefore inserted in the manuscript some more detailed explanation of these points, designed to meet these questions. These insertions, apart from a few words written between the lines, are on the separate sheets of which carbon copies are enclosed. The net effect of these and of some deletions is to increase the length of the paper by about one-twelfth. This lengthening of an already long paper I regret, but I do not see any other way to prevent obscurity.

One of your comments, striking quite a fundamental point, is as follows: "P. 14, line 4. 'that this man's new state is worse than his old.' This can't be correct. (7) only shows that any displacement under <u>a given</u> system of prices must make him worse off, but on p. 14 you assume a change in prices. Perhaps I have misunderstood the argument." I have tried to clarify this in the revision, and I think a careful reading will convince you that the argument is correct. Throughout it, different symbols are used (primed and unprimed letters) for the new and old prices, quantities, and income. There is therefore no confusion between new and old prices, and the demonstration is rigorously correct.

With cordial regards, I remain

Sincerely yours,

Harold Hotelling

Address until Oct. 1: Mountain Lakes, New Jersey.

 Vedlegg til brev fra H. Hotelling til R. Frisch 6/4-19

FOOTNOTE FOR ASTERISK ON P. 13:

*A friendly critic writes: "It is not clear to me why δp_i should be the exact per-unit revenue of the state from an excise tax which raises the price by δp_i from its old level. ... I should expect (referring to Figure 1) an increase in price of GL, and a revenue to the state of NL." The answer to this is that the summation of δr over all persons includes the sellers as well as the buyers, and that the government revenue per unit of the commodity is derived in part from each -- though it must be understood that the contribution of either or both may be negative. Intthe classical case represented by Figure 1, the buyers' δp is the height GL, while the sellers' is NG in magnitude and is negative. Since q' is positive for the buyer, and negative for the seller, the product $q'\delta p$ is in each case positive. The aggregate of these positive terms is the total tax revenue from the commodity.

 Vedlegg til brev fra H. Hotelling til R. Frisch 6/4-1938

INSERT IN P. 14:

This argument may be expressed in geometrical language as follows. Let $q_1, \ldots, q_n$ be cartesian coordinates in a space of n dimensions. Through each point of this space passes a hypersurface whose equation may be written $\Phi(q_1, \ldots, q_n)$ = constant. The individual's satisfaction is enhanced by moving from one to another of these hypersurfaces if the value of the constant on the right side of the equation is thereby increased; this will usually correspond to moving in a direction along which some or all of the q's increase. The point representing the individual's combination of goods is however constrained in the first instance to lie in the hyperplane whose equation is (4). In this equation the p's and m are to be regarded as constant coefficients, while the q's vary over the hyperplane. A certain point Q on this hyperplane will be selected, corresponding to the maximum taken by the function Φ subject to the limitation (4). If the functions involved are analytic, Q will be the point of tangency of the hyperplane with one of the "indifference loci." The change in the tax system means that the individual must find a point Q' in the new hyperplane whose equation is $\Sigma p_i' q_i' = m'$. If we denote the coordinates of Q' by $q_1', \ldots, q_n'$, we have upon substituting them in the equation of this new hyperplane, $\Sigma p_i' q_i' = m'$. If the changes in prices and m are such as to leave the government revenue unchanged, (12) must vanish; that is,

$$\Sigma p_i q_i' = \Sigma p_i q_i .$$

Since $\Sigma p_i q_i = m$, this shows that $\Sigma p_i q_i' = m$; that is, that Q' lies on the same hyperplane to which Q was confined in the first place. But since Q was chosen among all the points on this hyperplane as the one lying on the outermost possible indifference locus, for which Φ is a maximum, and since we are putting aside the infinitely improbable case of there being other points on the hyperplane having this maximizing property, it follows that Q' must lie on some other indifference locus, and that this will correspond to a lesser degree of satisfaction.

The fundamental theorem thus established is that <u>if a person must pay a certain sum of money in taxes, his satisfaction will be greater if the levy is made directly on him as a fixed a amount than if it is made through a system of excise taxes which he can to some extent avoid by rearranging his production and consumption.</u> In the latter case, the excise taxes must be at rates sufficiently high to yield the required revenue <u>after</u> the person's rearrangement of his budget. The redistribution of his production and consumption then represents a loss to him without any corresponding gain to the treasury. This conclusion is not new. What we have done is to establish it in a rigorous manner free from the fallacious methods of reasoning about one commodity at a time which haveled to false conclusions in other associated discussions.

The conclusion that a fixed levy such as an income or land tax is better for an individual than a system of excise taxes may be extended to the whole aggregate of individuals. In making this extension it is necessary to neglect certain interactions among the individuals that may be called "social" in character, and are separate and distinct from the interactions through the economic mechanisms of price and exchange. An example of such "social" interacti ~~ux~~ is the case of the drunkard who, after adjusting his consumption of whisky to what he considers his own maximum of satisfaction, beats his wife and makes his automobile a public menace on the highway. The restrictive taxation and regulation of alcoholic liquors and certain other commodities do not fall under the purview of our theorems because of these social interactions which are not economic in the strict sense. With this qualification, and neglecting also certain possibilities whose total probability is zero, we have:

If government revenue is produced by any system of excise taxes, there exists a possible distribution of personal levies among the individuals of the community such that the abolition of the excise taxes and their replacement by these levies will yield the same revenue while leaving each person in ~~xxxxxxxxxxxx~~ a state more satisfactory to himself than before.

It is in the sense of this theorem that we shall later sections speak of "the maximum of total satisfactions" or "the maximum of general welfare" or "the maximum national dividend" requiring as a necessary, though not sufficient, condition that the sale of goods shall be without additions ~~ix~~ to price in the nature of excise taxes. These looser expressions are in common use, and are convenient; when used in this paper, they refer to the proposition above, which depends only on rank ordering of satisfactions; there is no connotation of adding utility functions of different persons.

 Vedlegg til brev fra H. Hotelling til R. Frisch 6/4-1938.

INSERT IN P. 16:

It is remarkable, and may appear paradoxical, that without assuming any particular measure of utility or any means of comparison of one person's utility with another's, we have been able to arrive at (19) as a valid approximation measuring in money a total loss of satisfactions to many persons. That the result depends only on the conception of ranking, without measurement, of satisfactions by each person is readily apparent from the foregoing demonstration; or we may for any person replace Φ by another function ψ as an index of the same system of ranks among satisfactions. If we do this in such a way that the derivatives are continuous, we shall have $\psi = F(\Phi)$, where F is an increasing function with continuous derivatives. Upon writing the expressions for the first and second derivatives of ψ in terms of those of F and Φ it may be seen that the foregoing formulae involving Φ are necessary and sufficient conditions for the truth of the same equations with ψ written in place of Φ. The result (18) is independent of which system of indicating ranks is used. The fundamental fact here is that <u>arbitrary</u> analytic transformations, even of very complicated functional forms, always induce <u>homogeneous</u> <u>linear</u> transformations of differentials.

Not only the approximation (19) but also the whole expression indicated by (18) are absolutely invariant under all analytic transformations of the utility functions of all the persons involved. These expressions depend only on the demand and supply functions, which are capable of operational determination. They represent simply the money cost to the state of the inefficiency of the system of excise taxation, when this is arranged in such a way as to leave unchanged the satisfactions derived from his private income by each person.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

Mountain Lakes, N. J.
May 26, 1938

Professor Ragnar Frisch
Vinderen, Oslo, Norway

My dear Frisch:

I have your letter of May 11, stating that you were writi ng a note of a few pages based on the final galley proof of my paper for the July issue of Econometrica. This is quite suitable; discussion of papers adds much to an understanding of the issues involved; and in a society such as ours, whose members cannot all assemble in one place, the oral discussion upon presentation of a paper may well be supplemented by published notes.

If however a critical note is appended to a carefully prepared paper without an opportunity for the author to reply in the same issue, it detracts seriously from the force of the paper. Very brief and informal notes, published without adequate study of alternative points of view, may indeed be dealt with in this way in some instances. My paper is not however of this character. Its preparation has been of the most painstaking character, extending over six years and requiring an average of several weeks to the page, of full-time work. It has been submitted to numerous economists and students in preliminary forms, has been the basis of a large number of lectures and informal talks, and has undergone frequent revision and rewriting. Critics of various schools of thought have been consulted, and modifications have been made to meet their objections when these seemed well founded. I spoke on the subject at Colorado Springs in 1935, at Chicago in 1936, at Atlantic City in 1937, and at various local meetings before and since these occasions. Each time comments and criticisms were made and met, either by refutation or by modification of the paper. Under these circumstances, I do not think it likely that any criticism conceived within a few days and published immediately is likely to have much force. It is indeed probable that my exposition has not always been as clear and felicitous as might be desired, and verbal and bibliographical errors are possible, such as those which you corrected in the letter you so kindly wrote me some time ago. It is also probable that some of the problems around the periphery of the main question have not been dealt with to an adequate extent; this would have been impossible in anything shorter than a book. But the central thesis is I think quite secure.

There may be other comments and criticisms of my paper. Would it not be well to hold your note, and publish it in the next issue with any other remarks by economists on the subject, and with a chance for a reply by me in that issue?

Very sincerely yours,

Harold Hotelling

Mountain Lakes, New Jersey
July 25, 1938

My dear Frisch,

I am enclosing a note for Econometrica, "The Relation of Prices to Marginal Costs in an Optimum System," inspired partly by your comments on my paper on "The General Welfare...", and partly by some verbal comments of Lerner and others. As you will see, I agree with you on most points, though not on all.

The sixteenth paragraph of the galley proof which you kindly had sent to me of your note ("The Dupuit Taxation Theorem") is one with which I do not agree. If upon consideration you should decide to remove this, the paragraph on pp. 6 and 7 of the enclosed manuscript should also be removed.

To keep Mr. Leavens informed, and to facilitate matters in some possible contingencies, I am sending him a copy of this manuscript and letter.

Very sincerely yours,

Harold Hotelling

Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

September 8, 1938

My dear Frisch:

I have your letter of August 11, but have delayed somewhat in answering because I wished not only to be sure of having the right point of view on the questions at issue, but to have the opportunity to submit them to a third party. Dr. Tintner has just been visiting here, and after looking over carefully what we have both written, and puzzling for some time over the "entropic" paradox you set forth, he concurs with me in the following view of it.

The reasoning you give in your galley proof ("The Dupuit Taxation Theorem") seems to show that every change of prices, or imposition of taxes and bounties of the kind you there call "self-financed," puts the individual in a worse condition than before. Even a reversal of an original mistake only makes matters worse. This, as you point out, is paradoxical and absurd. The question then arises of placing one's finger on the point in the argument at which the fallacy enters. This point seems to be in the definition of "self-financed." It is indeed plausible to apply this term to a system of taxes and bounties such that the sum of the products of the rates by the new quantities is zero. But it is necessary to consider for the present purpose, not the <u>total</u> rate of each positive or negative tax, but the part of it falling on <u>a particular individual</u>, in the sense of the change in the price he pays or receives for the ith commodity as a result of a system of taxes and bounties. From the standpoint of the individual we may, as we have more or less been doing, consider the government as buying from him and selling to him at prices <u>that it fixes</u>. Ja

Under such conditions the government, to decide whether a certain change in prices is self-financed, must decide (a) whether the new prices will yield as great a <u>revenue</u> as the old, and (b) what will be the effect of the <u>change in production rates on the costs of the government as producer</u>. The latter question leads to considerable complications. But the question (a), on the assumptions we have been making, yields a perfectly determinate answer. Since by hypothesis the individual spends his whole income <u>m</u> on the commodities in question, the money receipts of the government from him are absolutely constant, no matter what prices it sets. Thus <u>every</u> change of prices, not merely those satisfying the definition given, is actually self-financed from the standpoint of the government, if we ignore costs. But the ignoration of costs is involved in the entropic argument. If the government sells at the same price at which it buys, we are back at the situation of sales at marginal

nei – jeg har jo selv ment hvor paradoksalt ...

Det er min innvending

Det tror jeg er ganske enkelt. Hva er vi kaller ... det opprinnelige

8/9-1938.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

My theorem holds without any reference to a cost concept. If self finance is by my definition as $t = 0$ in (1). You will of course admit that I have the right to use this definition, but, it is quite probable that it does not correspond to your problem.

cost; it is only then that the definition of "self-financed" changes becomes appropriate; and as we have seen, they are in this case changes for the worse.

As to your second question, regarding the definition of marginal cost, my definition is simply the increment, or differential (the latter when we are using strictly continuous hypothetical situations) of the total cost in money associated with the operation of an enterprise. If the enterprise produces the quantities $q_1, \cdots, q_n$ of n commodities, and spends \$M, where M is a continuously differentiable function of $q_1, \cdots, q_n$, then the marginal cost of an increment dq_i of the ith commodity is

Function M [illegible].

$$\$ \frac{\partial M}{\partial q_i} dq_i,$$

apart from possible terms of second and higher order in dq_i. Among the costs is rent, as I have previously set forth in some detail. M does not involve directly the utility function of a particular purchaser of the products of the enterprise. It does involve the utility functions of a large number of persons indirectly, in the sense that the prices at which the enterprise can buy the services of these persons are related to their utility functions. But monotonic transformations of these utility functions will not change the prices when these are determined, e. g. through a Walrasian system of equations of general equilibrium. Such transformations can have no influence whatever on prices, money costs, (therefore marginal costs), or any of the conclusions regarding the inefficiency of the current notion that sales should cover average costs.

I regret that there is a possibility that my note "The Relation of Prices to Marginal Costs in an Optimum System" may not appear in the October issue. If it must be held over until January, I believe that your note, "The Dupuit Taxation Theorem," should likewise be held to appear in the same issue. Dragging the discussion of a paper through several issues of a journal is very tiring, and the readers quickly lose interest. It seems to me that the entire discussion ought so far as possible to appear in a single issue. Also, it is customary to allow the author of the original paper the final rejoinder. The final note in the series discussing my presidential address should be by me.

Very sincerely,

Harold Hotelling

Copy to Mr. Leavens.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

November 14, 1938

Professor Ragnar Frisch,
Vinderen pr. Oslo,
Norway.

My dear Frisch:

Mr. Baster's note, which I am returning herewith, is based on a mathematical misunderstanding of an elementary nature. My reply to it is also enclosed, - in duplicate so that you can send a copy to him and keep one, if you wish.

With cordial regards,

Harold Hotelling

Harold Hotelling.

Ad brev fra H. Hotelling til R. Frisch 14/11-1

Mr. Baster has misunderstood the reasoning leading to the inequality (7). Defining

$$\delta\Phi = \Phi(q_1+\delta q_1, \ldots, q_n+\delta q_n) - \Phi(q_1, \ldots, q_n),$$

and supposing that $q_1, \ldots, q_n$ had been chosen so as to make the utility or indifference function Φ a maximum subject to the condition

(4) $$\sum p_i q_i = m,$$

where the prices p_i and the income m are fixed coefficients, we have without difficulty the result

(7) $$\delta\Phi < 0$$

whenever the increments δq_i are such that the new quantities $q_i' = q_i + \delta q_i$ satisfy the fixed condition (4); that is, whenever

(6) $$\sum p_i \delta q_i = 0.$$

I showed on p. 251 that (6) will actually hold if excises taxes come to replace an income tax of the same amount on the person considered; hence in this case $\delta\Phi < 0$. Here the p_i are the prices originally prevailing. Mr. Baster objects to the inference of (7) from (6) on the ground that a new set of prices will in the new state of affairs replace the old. These new prices I have called p_i', not p_i, and they do not enter into the condition (6) on the δq_i. He seems to feel that the p_i in (6) should be replaced by the p_i', thus yielding a condition from which (7) would not follow. The argument as originally stated is however correct; I have used different symbols for the old and the new prices, thus avoiding the confusion into which Mr. Baster falls on account of his thought that prices after the change as well as before it are denoted by p_i.

2) Ad brev fra H. Hotelling til R. Frisch 14/11-1938.

The geometrical interpretation of (7) is not what Mr. Baster indicates, but is outlined on p. 251 of my article. For two commodities it means simply that if Q is the point at which a straight line touches the outermost of the indifference curves which it meets, then points other than Q on the line lie on indifference curves that are not so far out from the origin, and so do not represent such a high degree of satisfaction.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

November 29, 1938

Professor Ragnar Frisch
Vinderen pr. Oslo
Norway

My dear Frisch:

I am returning herewith Dr. Preinreich's letter with your annotation. I regret that I seem to have given him the impression that I agreed with his formulation of the problem of depreciation completely and considered Bain's work completely unsound. The truth is that I have not even yet fully made up my mind as to how this problem, or rather group of problems, ought to be handled.

Preinreich's paper in the July issue of Econometrica was in two distinct parts, of which one is Lotka's source of objection and the other Bain's. As to the former, it is clear that Lotka published a large part or all of the mathematical theory, in the form of a treatment of mathematical biology many years ago, though Preinreich did not know this when he wrote the article. It seemed to me that Lotka was justified in not only wanting his earlier contribution recognized, but also in holding that he could do a better job than Preinreich of summarizing this earlier work. It was this conclusion at which I had arrived after hearing from Lotka and Leavens. When we lunched together, Preinreich informed me that his recent work, partly incorporated in the second manuscript he had sent Econometrica, contained additional material not in Lotka's work, in the form of a different method of solution yielding itself to much more convenient numerical treatment. Upon looking over this material with him somewhat hastily I was inclined to agree, on his statement of the facts, that his additional work was relevant and ought to be published. I have not, however, made a search of the literature to find out whether anything equivalent has been published.

As to the other part of the July article, I do not agree with Dr. Preinreich except to a very limited extent. This problem may perhaps be subdivided into that of calculating depreciation and that of deciding when a machine (or other property) should be replaced. Now my idea of the proper criterion for valuation of an old property, at least under static economic conditions and with everything known in advance, is that the value should be fixed at such a price that the buyer at this price would get as good a bargain as the seller. Preinreich's idea of valuation seems to include a considerable element of monopoly value of the business, or "good will," and this must of course give a different value.

Det kan være rimelig

29/11-1938.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

On the question of replacement Bain's argument seems quite cogent, but only under very limited conditions. He assumes that the rate of output is fixed, and that the replacement of the machine is to be at a time determined by this rate. But actually the rate of output is a function that varies through the life of the machine in response to the varying profitability of operation, and drops to zero when the machine is scrapped. Bain's solution will not be the correct one either if (1) the property is operated as a monopolistic unit of production, so that the rate of operation is one of the variables at the disposition of the owner in the process of maximizing his profit, or (2) if there is a condition of quasi-monopoly or imperfect competition, as there nearly always is in large industry, or (3) if the machine is one of a number owned by the same owner, among which the work to be done can be redistributed from time to time to obtain the most profitabl utilization in accordance with the varying efficiencies. On this point it seemed to me that Preinreich had approximately the correct point of view, though obscured somewhat by the allied difficulty about valuation.

The weakness of Dr. Preinreich's idea of valuation seems to me to be revealed by his statement that the value of a new property is different from its cost. It is quite hard to accept such a theory of value. But we must remember that "value" is not a concept of which there is any generally accepted definition applicable under conditions of monopoly, quasi-monopoly, etc., or under conditions of completely unanticipated changes in conditions. In fact it does not seem that any real definition of value, applicable under the conditions actually obtaining in the world, is possible. Bonbright's recent two-volume work on "Valuation for Different Legal Purposes" brings out clearly the ambiguities in the concept of value, and the contradictions in which the courts have become involved in attempting to give suitable definitions. There is also a voluminous literature on the concept of value written by economists, almost theological in nature and verbosity. Under these conditions it is not surprisin that different approaches to the problem of calculating depreciation in value give different results.

Very sincerely yours,

Harold Hotelling

Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

Nov. 30 [1938]

Henry Schultz' sudden death was a terrific shock to all of us. I hope an adequate obituary notice will appear; & shall discuss this with the Chicago people this week end.

H. H.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

Jan. 10, 1939

Dear Frisch:

Here are carbon copies of things I have sent Leavens.

I regret that the rush of the Christmas meetings has prevented my giving proper attention to the "marginal cost" discussion; but I will send you (and Leavens) a brief final note in a few days.

Cordially yours,
Harold Hotelling

Prof. Ragnar Frisch
Vinderen
Oslo
Norway

B. O
brev
761 A

Copy for Prof. Frisch

Vedlegg til brev fra H. Hotelling til R. Frisch 10. jan. 1939

January 6, 1939

Dear Mr. Leavens:

I am enclosing an abstract of my paper on "The Selection of Variates for Use in Prediction," and also your copy of the Detroit program, marked with my observations on the various papers. These observations are supplemented by the following remarks. My paper on Henry Schultz is now being typed, and I expect to send it to you Monday.

Paul Douglas' paper on Schultz contained material which seemed to me highly appropriate for publication in Econometrica. Hence I suggested that he send it to you for publication in conjunction with mine and a photograph of Schultz. This may perhaps seem like a good deal of space for Schultz, but I am convinced that it is space well used, for several reasons. Schultz' method of attack on the problems of demand functions is of great value, and deserving of emulation. He was one of the original moving spirits in econometric activities, and an energetic member of the society. His biography also is one of interest to younger workers in econometrics. He leaves numerous friends among econometricians in this country and Europe, including a large body of devoted former students and assistants. And Douglas' mention of his liberal views is highly appropriate at this moment in an international journal like Econometrica.

I will send a carbon copy of this letter and of my paper on Schultz to Frisch, as you suggest.

The paper of A. T. Court impressed me favorably in several respects; but the whole automobile session was of course rather suggestive of "ex parte" proceedings, and publication might be held up until critical opponents can be found and heard.

Lotka's work I think should be published. Shewhart's is excellent, though I am not sure how much of it has been published. Bain showed he had some good ideas, but his paper ought to be carefully examined. The paper of Roos and Lange seemed to me good. I did not care for Lederer's, and Clark's was not his best work, though highly appropriate, and fitting in nicely with Millikan's. Millikan's paper I thought a good one, but too long. Perhaps he should be invited to send an abbreviated form of his paper to Econometrica. In that case it might be suggested that, to save costs in printing, he modify his symbol for the "conjectural derivative," perhaps using d'x/d'p, etc.

McIntyre's paper seemed to me excellent; it ought to be invited for Econometrica, but sent to a referee, e. g. Roos, Lederer, or Gerhard Colm. Colm, who is at the New School for Social Research, 66 West 12th st., New York City, would be particularly

(over)

suitable because he has written on the undistributed profits tax.

The papers at the Thursday morning session all impressed me favorably, though my recollection of them is a little blurred. Offhand, I should rate them in approximately the following order: (1) Mendershausen (best), (2) Wald, (3) Douglas and Lewis, (4) Gilboy.

Bennett's paper seemed to me particularly appropriate for Econometrica, which has published several papers using difference and differential equations. Before definitely recommending it I should want to read it carefully, but my impression of it was favorable. I also liked Chawner's paper.

Very sincerely yours,

Harold Hotelling

Vedlegg til brev fra H. Hotelling til R. Frisch 10. jan. 1939.

HENRY SCHULTZ*

By Harold Hotelling

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*This paper and that of Paul H. Douglas were read at a special joint meeting of the Econometric Society, American Economic Association, and ~~the~~ American Statistical Association at Detroit, December 29th, 1938. Ward L. Crum presided and spoke briefly of Henry Schultz.

After a life devoted with singular intensity to the advancement of economic theory and its statistical complement, Henry Schultz was suddenly killed on November 26th, 1938, when the automobile he was driving shot off from a mountain road near San Diego. The accident was the more appalling because it means that he leaves no direct descendants to carry on his genius; for his wife and his two daughters, Ruth, aged 15, and Jean, aged 11, were also killed.

Henry Schultz was born in Russian Poland September 4th, 1893. After attending the public schools of New York City and the College of the City of New York he received the Bachelor of Arts degree in 1916. In the same year he matriculated at Columbia University, where he studied with Professors Seligman, Seager, Moore, Mitchell, Willis, Mussey, Chaddock, Giddings, Dewey, and others. These studies were however interrupted in 1917 by the war. After the armistice he was awarded an army scholarship which enabled him to spend the spring and summer terms of 1919 at the London School of Economics and Political Science and at the Galton Laboratory of University College. During this time he attended the lectures of Cannan on economic theory, of Graham Wallas on political theory, of Hobhouse on philosophy, and of Bowley and Karl Pearson on statistics. Returning to the United States, he conducted various statistical and economic investigations for the War Trade Board, the United States Bureau of the Census, the United States Bureau of Efficiency, and the Institute of Economics.

When he completed his work for the doctorate at Columbia in 1925 he was director of statistical research in the Children's Bureau of the United States Department of Labor. His thesis dealt with "The statistical law of demand as illustrated by the demand for sugar," and was published in two instalments in the Journal of Political Economy. Its total length was 81 pages, coming closer in this respect, as well as in method and spirit, to dissertations in the sciences than to those in the more verbose types of economics. In it he acknowledges the kindness of Isador Lubin, F. C. Mills and W C. Mitchell in reading the manuscript. His main inspiration was in the work of Henry L. Moore, whose pioneer attempts to derive demand curves from time series stirred his enthusiasm.

After entering the Department of Economics at the University of Chicago in 1926, Schultz gradually developed a large program of research of a kind definitely projected by his earlier studies. Beginning with sugar, he studied numerous commodities whose demand curves he attempted to obtain from the records of consumption and price. He also made some attempts to obtain supply functions, and shortly before his death discussed plans for extending his work in this direction. Convinced that economics could not become a science without careful quantitative studies, he examined one commodity after another with the primary object of exemplifying, checking and extending economic theory. The neglect of theory so often evident in studies of particular industries and commodities was not a fault of Henry Schultz. While making numerous and extensive excursions into the forests of detail surrounding the consumption and production of the several commodities, he always regarded these excursions as means of purifying the representation of theory derived from his examination of the demand and supply schedules by removing or allowing for particular sources of disturbance.

Obtaining from the social science research funds of the university the means for assistants and machines, Schultz trained a staff of workers, many of whom have since become prominent by their own work. He kept them busy searching out and correcting the data regarding the long list of commodities, drawing graphs, making calculations, translating relevant material, and otherwise building up the mass of results of which a large part appears in his final book. This book, "The theory and measurement of demand," which I reviewed in the Journal of the American Statistical Association for December, 1938, in some detail, is a massive achievement after which, apparently for the first time, Schultz paused in his research for a few months. He had left Chicago for a semester to teach at the University of California in Los Angeles while making a fresh start after the task that had absorbed him for so long. He jestingly remarked after the completion of this book that it was a good time to die.

At first Schultz used the inefficient statistical methods then current, deriving demand functions from link relatives and percentage deviations from trend without adequate tests of significance in terms of probability. But as the light of modern developments of theory and technique began to spread he was quick to utilize it. He made an intensive study of the great new work in mathematical statistics and used the results accurately and efficiently in his later investigations. He even made contributions to mathematical statistics himself. In this later work his usual procedure was to fit a regression equation in which one of the variates, price and quantity, would appear as a dependent and the other as an independent variate, with time and polynomials in time as additional independent variates. Sometimes the logarithms of prices and quantities would be taken as the variates. He called the result "the dynamic law of demand," and in a sense this appellation is appropriate. For any fixed value of the time variate these regression equations give approximations to the demand curves of classical theory, approximations whose accuracy is limited by the weaknesses of the "ceteris paribus" assumptions of that theory. From another standpoint, however, it will be observed that both cumulative and speculative effects are absent from Schultz' equations, which thus belong essentially to static rather than to dynamic economics, with time as a passive parameter, carrying along the gradually changing influences of a mass of unspecified sources of variation. His results do not, for example, offer an explanation of business cycles. He was aware of this, and preferred to study static theory, as more fundamental, rather than joining the great procession into monetary and business-cycle theory. He was extending the work of the classical economists in a different direction from that most popular at the moment.

But Henry Schultz' study of demand was not only statistical, nor was it even confined to the purely empirical. He studied general economic theory intensively, and probed deeply into the phases of this theory on which mathematics could throw a specially clear light. He worked through Pareto's extensive writings, which he greatly admòred, though later recognizing certain weaknesses in them, such as Pareto's blunder in defining competing and complementary commodities by means of the ambiguous sign of the second derivative of a utility function not fully determined. He considered Pareto's most valuable work his forty-page article in the <u>Encyclopédie des Sciences Mathématiques</u>. He studied Cournot, Edgeworth, Cassel, Walras, Slutsky, and other mathematical economists thoroughly.

For the full study of economic theory and of statistical methods Schultz, like most economists, found his mathematical knowledge inadequate. In spite of his intense preoccupation with his teaching and economic research, he undertook while at Chicago to fill this gap, and actually did so to a remarkable extent. By reading, attending mathematics classes with young students while himself a professor, and consultation with colleagues, he mastered advanced calculus and considerable areas of the theory of differential equations and the calculus of variations.

Relations between different commodities engaged Schultz' attention in his later years. In 1932 he read for the Journal of Political Economy a manuscript of mine on this subject, and made some suggestions, based on his study of Pareto, that resulted both in its improvement and in my starting on a new train of thought that eventually led to further results. When these were published in Econometrica in 1935, Schultz pointed out to me the work of Slutsky published in Italian in 1915 in the same field, of which I had been unaware but which, as it happened, my work complemented without duplicating. His discovery in 1935 of this previously ignored work of Slutsky is an instance of the wide dragnet with which he sought out original ideas from all sources. The theory of demand for related commodities predicts under certain assumptions relations of symmetry will be satisfied by the related demand functions. Schultz set out to determine from the data whether these relations are satisfied by actual demand functions. Though his results are somewhat inconclusive in this respect, owing to paucity of good data, they seem to indicate significant discrepancies in certain cases, and point the way to more definitive tests which will eventually become possible through the accumulation of data and the improvement of statistical methods.

Of his warm-hearted interest in individual students and other young economists and his encouragement of their researches it is impossible to give an adequate idea. This personal characteristic is dwelt upon again and again by former students and assistants. The following extracts from letters from them give some indication of his sympathetic understanding, of his strenuous devotion to work, and of his wide knowledge.

Harry Pelle Hartkeimer writes:

> I value my experience as his research assistant much more than the opportunity of being a student in his classes, for Professor Schultz was primarily a research worker. His courses were partial aspects of his research program and were never a survey of existing knowledge, which the students were expected to obtain themselves from the library. His courses were always a step forward into unexplored regions of statistical knowledge. This was evidenced by his frequent remark that he never repeated a course. He would say that next year ~~I will~~ he would have such and such article or book written (he was then working on it while teaching the course) and the students could then read the book, article, or monograph. It would no longer be necessary to give the course. At first he had no idea of the tremendous load this placed upon the average student who was taking other courses. Later on he did realize this somewhat, for he introduced at the University of Chicago the practice of allowing the student to register for double or triple credit in one course. Thus the student could take one course for 3 majors credit and devote his whole attention to the work under Professor Schultz, for 3 majors constitute full time work for one quarter. Professor Schultz worked from early in the morning until late at night in his office and when the school building was closed at 10 P. M. he took a large brief case of work home with him. He worked Saturdays and holidays the same as week days, and Sunday afternoons also found him in the office. I know, for I worked with him during all those times, and after I began teaching at the University of Missouri I could always see Professor Schultz in his office on any week end or holiday when I visited Chicago.
>
> Professor Schultz's most outstanding characteristics were thoroughness and accuracy. His research work was checked in every way possible. A research worker repeated every computation, by a different process on a different machine during a different day. Later, a third person would repeat the entire problem independently as a check. All entries of data were checked three times by different persons on every one of the repeated series of computations. Each worker signed his name or initials on each computation sheet and responsibility for mistakes was fixed. No penalties were ever imposed for errors, but high praise was given by Professor Schultz whenever anyone discovered a mistake made

earlier in the process of computation. After I had been working for Professor Schultz only a short time I discovered an error in the index number used to deflate the price of corn. This error had been made about four years earlier and an enormous amount of computing work would have to be repeated if I called attention to the error. After some hesitation I told Professor Schultz about it. He was very pleased and became very much interested in how I happened to detect the error. It happened that I had plotted on the same chart the original price data, the deflated price data, and the index number. The deflated price data revealed a large fluctuation in one year, while neither the original data nor the price index showed any marked deviation. This was caused by the use of an index number for that one year which was in error by 20 points. A 7 had blurred so it was mistaken to be a 9. From then on the graphic comparison became a standard procedure and I was given much more checking work to do.

***

Professor Schultz was a careful student of current economic theory and he would frequently forecast developments in economic theory. I recall an incident in which he forecast a speech made by a president of the American Economic Association. He was at dinner with his colleagues who began to discuss forecasting. Professor Schultz smilingly remarked that he would not discuss general business forecasting but he would forecast the presidential speech which was to be given some days later. He mentioned that he did not know the president personally, he had not talked with him, or read the speech, but he was a careful student of current tendencies in economic theory. His forecast was amazingly definite and correct.

A letter from H. Gregg Lewis contains the following passages:

> Whether in economics or statistics he began by building a mathematical-logical foundation of theory -- a model to serve as a frame of reference for further analysis. He was an extremely able logician, widely read and trained in modern philosophy, mathematics, and the natural sciences. He impressed upon his students the necessity for thoroughly understanding all the premises upon which the logical model was built.
>
> But he didn't stop with the strict logical pattern; he proceeded to show us how we could test the assumptions underlying it, first by <u>a priori</u> reasoning -- in which he exhibited his wide range of knowledge, and then by statistical techniques. Finally he placed the whole pattern in the broader social-economic matrix to which it referred. In this last step he insisted that his students employ the whole of the social sciences and philosophy. None of his students ever doubted that he was at ease in practically all fields of economics, a master of statistical techniques, an accomplished logician and mathematician, and widely read in the natural sciences, the arts, as well as the social sciences. And despite the herculean amount of work he did besides teaching, he always kept

his lectures abreast of the most recent literature.

* * *

Schultz was an untiring worker, forever driving himself much more than any of his assistants. His own work day was a twelve hour one or longer, and his work week, seven days.

But more than any of these, Mr. Schultz was to us an unselfish friend and adviser. He was always interested in all aspects of our personal well-being, and never in a patronizing way.

Jacob L. Mosak writes:

The fatherly interest which he showed in me was typical of the warm interest he had in all who were associated with him. He always gave freely of his time to advise, encourage, and aid promising students. Altogether he was one of the kindliest and most warm-hearted of persons whose friendship it was my privilege to enjoy.

* * *

He always welcomed, indeed insisted on, honest criticism, no matter how severe. He could discuss matters for hours and, as Professor Millis said, shed only light and no heat. His ripe judgment, open-mindedness, and fairness, his good taste and tactfulness commanded the respect and admiration of all who were associated with him.

Of Schultz' striking personal characteristics as seen by his colleagues at Chicago Professor Douglas writes in an interesting note. I shall conclude with a list of ~~Schultz's~~ publications.

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Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

January 16, 1939

Professor Ragnar Frisch
Vinderen, Oslo, Norway.

Copy sent to Mr. D. H. Leavens
Mining Exchange Building
Colorado Springs, Col.

My dear Frisch:

I regret very much not having been able to give proper attention at an earlier date to your three letters of Nov. 28(reply to Baster), Dec. 13 (Schultz' death), and Dec. 2 (our discussion regarding marginal costs.) The first I think needs no further comment.

The session at Detroit in memory of Schultz was largely attended. From comments and letters it was plain that his passing is sincerely lamented by a large number of those most able to appreciate the present position of economic science. I have already sent my manuscript, slightly enlarged by quotations from letters which seemed to me noteworthy, to you and also to Mr. Leavens, and recommended the publication of Paul Douglas' talk and (in accordance with your suggestion) Schultz' photograph.

I agree with the modifications you suggest in our notes on marginal cost; that is, the modification you indicate in your sixteenth paragraph (since I take it we are now agreed regarding the definition of marginal cost), with the deletion of the corresponding part of my manuscript on "The Relation of Prices to Marginal Costs in an Optimum System;" that is, of the part beginning on p. 6 with "The sixteenth paragaph of ..." and ending on p. 9 with "previous paper (p. 267)." I enclose also one copy of my new manuscript, "A Final Note," sending another to Mr. Leavens. In accordance with your suggestion the four notes will thus appear in the order:

1. Frisch: The Dupuit Taxation Theorem.
2. Hotelling: The Relation of Prices of Marginal Costs in an Optimum System.
3. Frisch: A Further Note on the Dupuit Taxation Theorem.
4. Hotelling: A Final Note.

With cordial regards, I remain

Sincerely yours,

Harold Hotelling

Harold Hotelling

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

Summer address: Mountain Lakes, N. J.
June 7, 1939

Professor Ragnar Frisch,
Vindern,
Oslo, Norway.

Dear Frisch:

Your review of the work on replacement rates I have found quite illuminating. It seems to me to help considerably in clarifying the differences among the various treatments.

I have indicated on the margin various minutiae which need revision. Most of these appear to be mere slips in copying. There is one question of principle, - I believe it wise to include the year of publication in all references, e. g., that to Norton on page 2.

I have not discussed this paper with Preinreich or with Lotka, but I do not see how either of them could object to it.

I received also the carbon copy of your letter of May 9th to Bain. Furthermore, a new MS, entitled "The economic life of industrial equipment" has been sent to me by Preinreich, and I have just finished reading it. He does not make clear whether he plans to sent this paper to ECONOMETRICA. I am enclosing herewith a copy of my letter to Preinreich, with remarks on this paper.

On account of my preoccupation with statistics, I think I shall not write a paper this year on depreciation. At any rate, don't count upon it and don't delay other people's papers on account of it. I expect to work all summer on my long-delayed book on statistics and then in November go to France and India to lecture.

With cordial regards, I remain
Sincerely your friend,

Harold Hotelling

Ad brev fra H. Hotelling til R. Frisch 7/6 - 1939.

Copy

June 7, 1939

Dr. Gabriel A. D. Preinreich,
17 East 42d Street,
New York.

Dear Dr. Preinreich:

I am returning herewith your paper "The economic life of industrial equipment" which I have read with great interest. It seems to me to contain a good deal of valuable material. In particular, the idea of an infinite chain of replacements which you have developed seems to me well worthy of consideration. It is interesting that we have in the various dates of retirement an infinite number of variables with respect to which the discounted profit is to be a maximum. Under static economic conditions, or under economic conditions varying in a preassigned fashion, the infinite chain seems to be merely another way of looking at the problem to which I gave chief attention in my paper of 1925 and to yield the same numerical results.

I can now see how it is that you can in some cases arrive at a valuation of a machine that is higher than the cost. Such cases seem to call for a limitation of the supply of renewals. In that case, the existing machines will have a scarcity value which might enhance the value above the cost. This limitation of supply of new machines at the same price, however, violates the assumption of static economic conditions which was made explicitly in the relevant part of my paper.¶ The most vulnerable part of this MS is, I think, the first paragraph on page 5. As to its first sentence I think no one would deny that the capital value of a machine might be greater than its cost new, provided economic conditions are changing sufficiently. As to the last sentence of this paragraph, "Since no income whatever can be had without the machine, the entire value of the enterprise must be imputed to it, to determine the proper date of scrapping," this argument might be applied to declare that each essential part of an enterprise has a value equal to the whole. Many fallacies of this type are cited by Bonbright in his treatise on value. The example

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and subsequent material on page 5 and the following pages make the differences between the various points of view clearer. In distinguishing between market price z and the value w of a unit of product or, as I called it in my 1925 paper, "theoretical selling price", you may not have observed that in that paper I treated the case in which the market price of the product of the machine was given by market conditions beyond the control of the owner of the machine. As I pointed out, near the beginning of the paper, this is a case of somewhat limited applicability, in which the entrepreneur might find himself "resting precariously on the judgment of his competitors". It seemed to me that a more generally important problem was that connected with cost determination in conjunction with the attempt to minimize cost or maximize profit, "by doing something to the machine rather than to the books". It was in connection with this more general case that I used the ideas of unit cost and unit cost plus, which had existed for a long time but had been applied in a slightly inexact manner to this problem by Taylor.

In many cases the market price of the product of a machine has no definite meaning because nothing is actually sold which is the product of one machine alone; the articles sold are the product of many machines under the same ownership, each of which is essential to the finished product. Even the price of the finished product may well contain an element of monopoly profit or rent in addition to special advantages which cannot be assigned unambiguously to any particular physical property. This is the typical situation in industry. ~~It is~~ In such cases ~~that~~ we cannot speak of the value of the product of a machine as determined by external market conditions alone. "The theoretical selling price" used by Taylor and myself becomes a practical tool in connection with cost accounting which should have considerable practical utility under these conditions. Value must be assigned to the service of a machine by the owner of a complicated industrial plant on the basis of the best possible alternative to that service. Under static economic and technological conditions the best alternative to a machine is typically another machine of the same kind. The problem of determining depreciation is essentially that of comparing the values of machines of the same kind, but of different ages.

The whole theory of value and of valuation indeed needs revision. In particular, the role of marginal costs needs increased attention;

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G. A. D. Preinreich H. Hotelling

thus instead of writing for the rent of a machine

$$R(t) = xY(t) - O(t)$$

as I did, or

$$wQ(t) - E(t)$$

as in your paper, we might well write

$$xY(q,t) - O(q,t), \quad \text{or} \quad wQ(q,t) - E(q,t)$$

where q is the number of units of output at time t and then observe that q is a function at the disposal of the owner in his attempt to maximize the integral, or rather, a complicated function representing his aggregate profit, in which this integral appears. This is a decidedly more general approach to the problem than by variation of T alone. On page 17 your MS states "the theory of public utility regulation is thus quite definite and leaves no room for equivocation". This is so drastic a statement that it is quite likely to be challenged. I can, e. g., imagine Bonbright and Hale, who have spent many years of perplexity over public utility problems, inclined to take exception to it on various grounds. My own feeling in the matter is that if the statement is true, it is true only as a part or as a proposed addition to current legal theory of utility regulation, but it seems to me that the entire basis of this current theory is extremely shaky and that it will ultimately have to be discarded in its entirety in favor of the operation of utility plants in the genuine interest of maximum public service. As I pointed out in ECONOMETRICA, for July, 1938, this means a fundamental and drastic change from current theory and practice. The third paragraph on page 25 appears somewhat strange, and likewise subject to possible challenge.

There is one change which I should be particularly glad if you would make, in view of a misunderstanding of my paper on Henry Schultz, which I am told has gained some currency. In the first line of footnote 14, will you please change "criticism" to "observation".

I regret that you are going away so soon that we shall not have an opportunity for another talk before you leave. I hope you will enjoy your vacation. My address this summer is Mountain Lakes, New Jersey.

Very sincerely,

Harold Hotelling

Copy Ad brev fra H. Hotelling til R. Frisch 7/6-1939.

Columbia University
in the City of New York

FACULTY OF POLITICAL SCIENCE

September 15, 1939

Dr. Gabriel A. D. Preinreich
17 East 42nd Street
New York City

Dear Dr. Preinreich:

I am returning herewith the manuscript of the appendix of your paper, embodying some remarks of mine in an earlier letter, with your replies to them. This all seems quite suitable and I should have no objection to the publication of this material. The various points involved would I think be illuminated for readers by this discussion, and would add interest to your main paper.

I have received from Prof. Frisch a copy of his letter to you of Aug. 19. Unfortunately I cannot remember sufficiently well the relevant parts of your manuscript to make any intelligent comment on the points he raises. In view of my preparations for my impending sailing to India from San Franscisco I would rather not try to settle these questions. However I imagine that the matter is quite an objective one, and that you can arrange the terminology, etc., satisfactorily.

A copy of this letter is going to Professor Frisch.

Very sincerely yours,

Harold Hotelling

Harold Hotelling

The University of North Carolina
Institute of Statistics
Chapel Hill

DEPARTMENT OF MATHEMATICAL STATISTICS

January 3, 1947

Professor Ragnar Frisch
Vinderen
Oslo, Norway

Dear Professor Frisch:

I heard about the very bad time you had during the war and have been intending for months to write to you to express my gratification that you are now safely out of it and my good wishes for the future. Please accept them now although somewhat belatedly.

As you will observe, I have now left Columbia University and have organized a new Department of Mathematical Statistics within the Institute of Statistics of the University of North Carolina. This is devoted primarily to the development of statistical methods.

Mr. C. Radhakrishna Rao of King's College, Cambridge, has sent me a quite ingenious paper in which he undertakes to answer a query which you propounded at the Oxford meeting of the Econometric Society in 1936 regarding linearity of regression. I am sending the manuscript to Mr. Rao with several suggestions for clarifying the presentation and am advising him, after doing so, to send the manuscript to you for publication in Econometrica.

With cordial regards, I remain

Sincerely yours,
Harold Hotelling
Harold Hotelling

HH:a

The University of North Carolina
Institute of Statistics
Chapel Hill

DEPARTMENT OF MATHEMATICAL STATISTICS

April 19, 1951

Professor Ragnar Frisch
Universitetets Socialøkonomiske Institutt
Oslo, Norway

Dear Frisch:

It is good to hear from you again, and to have your reflections on the inversion of matrices and solutions of linear equations.

The inversion of a matrix of large order n seems necessarily to involve a number of multiplications exceeding that required for solving a system of linear equations by a factor of the order of n. This appears to be true no matter what method is used, though the multiplier of n in this factor may be made only slightly greater than one-half if advantage is taken of the symmetry. However, it seems inescapable that the inversion of a large matrix involves considerably larger numbers of arithmetical operations than the solution of the system of linear equations. Furthermore, the application of an iterative method may mean that the required number of multiplications is very large unless a really good approximation was used to begin with.

This arithmetical difficulty is greatly alleviated if a good automatic matrix multiplier is available. I have recently been told that the International Business Machine Corporation now has a really efficient machine for doing this which, I think, they call C T C, but I have little detail regarding it as yet. We shall probably get one for our Institute of Statistics Laboratory.

With electronic digital computers it will be possible, according to John von Neumann, to multiply together a pair of fifty-rowed square matrices in a time which he thinks may be as little as four minutes, but which he says will certainly not in any case exceed twelve minutes. Moreover, it will be possible to give such a machine instructions to make repeated multiplications of such matrices, substituting in the iterative formula until an approximation to the inverse of a matrix is obtained with an error norm less than a pre-assigned value. All this can be accomplished without human intervention in a very brief time once the data and the instructions are fed into the machine. It is true, that these electronic computers are not yet generally available at low cost, but the indications are that they will be very soon.

Von Neumann, who has spent most of his time in the last six years on this subject, holds that with such high-speed computers the only good way to invert a matrix (and I think he would say also to solve a large system of linear equations) is by iteration, using the formula

$$(1) \quad C_{m+1} = C_m(2-AC_m).$$

Moreover, he considers that the initial approximation must, for efficiency, be the same for all matrices and proposes to use the identity matrix for it. Then to assure convergence, the matrix to be inverted is divided by its own trace, supposing this matrix to be positive definite, a condition that can be attained readily through matrix multiplication, even though the matrix originally given does not have this property. By this method, convergence is assured, if only the given matrix is non-singular.

Where, as with you and me, high-speed electronic computers are not immediately available, we cannot afford to be so prodigal with matrix multiplications. The division by the trace, while guaranteeing convergence, has a tendency to slow down the process in many cases, and I do not think that ordinarily, with desk computers, von Neumann's method is to be recommended.

In deciding between solving a system of linear equations and inverting a matrix, the most important question is as to the uses to be made of the results. Here it is often forgotten, but should be borne in mind in laying out the computations, that many or even all of the elements of the inverse matrix are likely to be wanted after the equations are solved, and if the inverse matrix must be computed at that stage, instead of at the beginning, the total amount of time is increased. Thus, after solving the normal equations of least squares, it is usually appropriate to perform tests of significance and find confidence intervals. These stages require elements of the inverse matrix which are also useful for numerous other purposes connected with least squares, including the studies of several predictands with the same predictors (as in the study of butter and margarine prices); also, in adjoining or removing predictors in other cases; likewise in physical and engineering problems connected both with mechanical and electronic situations in which large systems of linear equations are to be solved, it frequently turns out that either the inverse matrix or its canonical form is also needed, and had therefore better be computed before the equations are solved, instead of afterward.

Incidentally, I find a very convenient way of using the formula (1) is to put it in the form

$$(2)\quad C_1 = C_0 + C_0D,$$

where

$$(3)\quad D = 1 - AC_0 .$$

The reason for this is if C_0 is good approximation, the elements of D will be quite small and are therefore ~~appropriately~~ carried out to more decimal places than those of C_0.

Then the last term in (2) provides a correction to C_0 which usually takes the form of merely extending the number of digits in each entry to more decimal places.

Another advantage of inverting a matrix is the ready availability of a definite upper bound for the errors in the elements of the inverse matrix and in the unknowns through the norm of D, as explained in the 1949 Volume of the <u>Proceedings of the Berkeley Symposium</u>, page 281. There is no known corresponding bound for the errors in solving systems of linear equations. This question of definite bounds becomes particularly important where the number of unknowns is large since the loss of accuracy through the long-drawn-out direct method by rounding may then be great, while with iterative methods of solving equations the trial solutions have a disconcerting habit of sometimes appearing stable and then showing a wave of variation, possibly as a result of computing errors.

No method analogous to (1) is known for solving linear equations without inverting the matrix.

If you should chance to be in Great Britain this summer we may meet, for example, at the British Association Meeting at Edinburgh, August 8 to 15.

With best wishes, I remain,

Sincerely yours,

Harold Hotelling

Harold Hotelling

HH/jdo

STANFORD UNIVERSITY

DEPARTMENT OF MATHEMATICS

STANFORD UNIVERSITY, CALIFORNIA

Feb. 6, 1930

My dear Dr. Frisch:

I have just finished reading wit the greatest interest the two reprints I received from you yesterday. The paper "On Approximation to a Certain Type of Integrals" I am loaning to a student who is working with such approximatio Mr. J. A. Reid. I have been greatly struck by the generality and value of your generalizatio of Rolle's and Hadamard's theorems, and by this use of them.

The treatment of statistics by mean of matrices is an interesting subject, at which I have made one or two inconsequential attempts. I usually begin with a matrix X of observations, the element x_{ij} being the jth individual's value of the ith variable. Then XX' is a symmetrical square matrix of product moments and sums of squares. The study of correlation between two variables can be extended in this way to a "correlation" between two sets of variables; for example to the relation between the crops X of a group of agricultural products and the prices Y. The matrices XX', YY', XY' and YX' would all come into consideration. I like your discussion of the "scatter coefficient," and believe that it will eventually prove highly important in practice.

6/2-1930

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As you indicate on pages 96 and 97, your work raises a new set of problems in the theory of sampling distributions. I wonder whether light will not be thrown on them by the paper of R. A. Fisher, more recent than yours, "The General Sampling Distribution of the Multiple Correlation Coefficient," in the Proceedings of the Royal Society. A, vol. 121 (1928), p. 654.

With sincere regards,

Harold Hotelling

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February 26, 1931

Dear Dr. Frisch:

Thank you for your letters of February 21, and particularly for your suggestion of "half-invariants."

In the treatise I shall discuss partial and multiple correlation, both in the usual way and with reference to the sampling distributions of the quantities which are used. Most of these distributions have been discovered by R. A. Fisher, some by J. Wishart. Fisher's discovery of the exact general distribution for the multiple correlation appears in the Proceedings of the Royal Society of London, A, vol. 121 (1928-9), pp. 654-673. A list of his works is in the back of the third (1930) edition of his "Statistical Methods for Research Workers." These sampling distributions I am trying to elucidate in the book. Whether to use the matrix method of presentation I am undecided; I was very enthusiastic about it six or seven years ago, and have used it repeatedly in my classes and in short unpublished talks at the meetings of the San Francisco Section of the American Mathematical Society. Your paper in the Nordic Statistical Journal seems to me to serve a very useful purpose. I do not believe that you need worry about most American statisticians not knowing it, since most of these gentlemen know nothing

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whatever of a theoretical nature.

As I shall probably be at Columbia University after this year, I shall look forward to seeing you more frequently.

With cordial good wishes,

Harold Hotelling