

NEW ENGLAND RESEARCH COUNCIL
on Marketing and Food Supply

A voluntary co-ordinating council composed of New England Research Agencies and the United States Department of Agriculture.

Telephone Hubbard 1456
Room 425
261 Franklin Street
Boston, Mass.

October 5, 1931.

Professor Ragnar Frisch
University of Oslo
Oslo, Norway.

Dear Professor Frisch:

During the past ten years or so I have done some work in the field of statistical price analysis. I now want to devote some time to mathematical price theory in order both to direct my own study along logically sound lines and to try to coordinate some of the statistical price research in such a way as to throw light on theory. I intend to apply this fall for a fellowship for European study from July 1932 to June 1933.

After corresponding with Doctor Fisher of Yale, Doctor Ezekiel of the Federal Farm Board and others, I have decided that I want, if possible, to study under your direction, perhaps also spending some time with such men as Professor Divisia of Paris, Professor Amoroso of Rome and Professor Bowley of London.

I would plan to take along with me several studies on which I have recently been working. I would continue work on these while in Europe getting advice and suggestions from you and others concerning theoretical and methodological questions. I would like also to attend lectures and do a good deal of reading.

Before making definite plans on such a program of study, I would like, if possible, to get your suggestions concerning the work I might do at Oslo as well as in other European centers. I would appreciate any advice you may give me.

I know nothing of the Norwegian language. I have a fair knowledge of French and can manage to read German if I take time enough to figure out the technical words. Also, I once had a slight

5/10-1931

knowledge of Flemish and perhaps could learn some Norwegian in time.

I have studied at the Massachusetts Agricultural College (B.Sc.1922), Rutgers University (M. Sc.,1924), Columbia University (Ph.D., 1929) and Harvard University. I have taken work under Mitchell, Mills and the late Allyn Young. Since 1922 I have been employed as economist and statistician by the New Jersey Department of Agriculture, the Connecticut Agricultural College, the Massachusetts Department of Agriculture and the United States Bureau of Agricultural Economics. In my work I have been closely in touch with such men as Ezekiel, Bean and Working of the United States Bureau of Agricultural Economics, Warren and Pearson of Cornell, and Black of Harvard.

Very truly yours,

Fredrick V. Waugh.

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
Room 424, 261 Franklin Street
Boston, Massachusetts.

November 25, 1931.

Professor Ragnar Frisch
University of Oslo
Oslo, Norway.

Dear Professor Frisch:

Thank you very much for your letter of October 26. It seems to me that the work you outline in your letter is exactly what I want and I hope it may be possible for me to spend several months with you.

I applied yesterday for a fellowship and have suggested in my outline that I would probably spend about six months with you at Oslo dividing the other six months with Professors Divisia, Amoroso, Bowley and some of the German economists and possibly a short time with Russian economists.

I am particularly interested in your courses in statistics and in the general theory of price and also would like, if possible, to take some of your course in calculus.

Your plan of small conference groups seems particularly interesting to me as I want to do some statistical work and to discuss the theoretical considerations at the same time. This would give me a fine opportunity to discuss such questions with you and with some of your students.

I have written to Mr. Lunden of the University of Minnesota but have not yet heard from him.

Thank you very much for your kind letter and for your interest in my plans.

Sincerely yours,

Frederick V. Waugh

Frederick V. Waugh
Principal Agricultural Economist.

FWV/BA

U. B. Oslo
Brevs. nr.
761 A

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United States Department of Agriculture.

Telephone Hubbard 1456
Room 425
261 Franklin Street
Boston, Mass.

March 16, 1932.

Congratulations
My dear Doctor Frisch; -

The Social Science Research Council
has awarded me the research fellowship
about which I wrote you some months ago.
I am now making my plans for a year
in Europe starting this July and would like
your suggestions. My present plan is to
sail on the S. S. Pennland arriving at Havre
about July 24. I shall want to stop in
Paris for perhaps two weeks particularly
if I can talk with Professor Divisia
at that time. From then on my plans
depend on what arrangements would
be most convenient for you and
Professor Divisia.

I want if possible to spend several

2014
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In Oslo
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I always
like to
see you
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Two times
seen
approx

months with you at Oslo. Would it be convenient for me to spend the late summer and fall months with you there, going to Paris and Rome for the winter, and probably returning to Oslo in the spring to finish my year's work.

I realize that this arrangement might not fit in very well with the regular lecture courses. Do you think that this would be a serious difficulty? You understand, of course, that I am not anxious to get university credits, but want to do some research and to get a good general background of mathematical price theory. I would appreciate your suggestions.

My wife and two young children will go to Europe with me. I may want to put my six year

16/3-1932

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Telephone Hubbard 1456
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261 Franklin Street
Boston, Mass.

(3)

old girl in school, but I suppose
all the schools in Oslo are conducted
in the Norwegian language.

Mr. Lunder has written to you,
I suppose. He writes me that he
has given up his plans for a year
at Oslo because of financial
difficulties. I suppose this may
spoil your tentative plans for
a course given in English. Perhaps
I can learn a little Norwegian.
I have not tried that language
yet but have been brushing up on

*I know John and my secretary
think you try to make
the nice little you highly -*

*Don't print
him one copy
of his copy*

*Should all your
make copy*

(4)

my French and German.

We can of course make more definite plans after I get to Oslo, but I would especially like to get your reactions to my very general plan of work now. I know that whatever time I can spend with you will be well spent.

Sincerely yours,

Frederick V. Waugh.

U. S. Oslo
Brevs. nr.

761 A

NEW ENGLAND RESEARCH COUNCIL
on Marketing and Food Supply

A voluntary co-ordinating council composed
of New England Research Agencies and the
United States Department of Agriculture.

Telephone Hubbard 1456
Room 425
261 Franklin Street
Boston, Mass.

June 8, 1932.

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

Dear Professor Frisch:

Thank you for your letter telling of your trip to the
United States. I shall plan to attend the meeting of the Econometric
Society at Syracuse and if possible would like to spend an hour with
you some time during the sessions.

I am now planning to go directly to Oslo by the S. S. United States
of the Scandanavian line, leaving New York July 23. This will get me to
Oslo considerably in advance of the fall term at the University. I hope
it will be possible for me to use the library during August.

Sincerely yours,

Frederick V. Waugh



BONN, den 11 Januar 1933
MUNSTERPLATZ 23 u. 24
TELEFON: SAMMELNUMMER 5441
TEL.-ADR.: BERGISCHERHOF BONN

U. B. Oslo
Brevs. nr.
761 A

HAUS I. RANGES
—
NEUZEITLICHSTER COMFORT
AUSSTELLUNGSRÄUME
KONFERENZZIMMER
APPARTEMENTS MIT BAD
STAATSTELEFON IN ALLEN ZIMMERN
ZIVILE PREISE
GARAGE
—
BANKKONTO: DEUTSCHE BANK, BONN
—

Dear Professor Frisch,

We have been so
rushed traveling in Germany and
Holland since I left Oslo that
I have had practically no time
to sit down and write letters.

This will let you know that we are still alive
and happy. Please give our best regards
to Mrs. Frisch and your mother. We shall all
remember their kindnesses and I certainly
shall not forget the inspiration I got from
working with you.

At Berlin I saw many acquaintances
and also met many men whose work I knew
slightly. I was particularly interested in
Dr. Hanau of the Institut für Landwirtschaftliches
Marktforschung and Doctor Danner of the Institut
für Konjunkturforschung. Both are doing good work
in the statistical analysis of demand and I
think neither is a member of the Econometric Society.
If you have some literature of the society it might

be well to read it to them. Is it not true that the European members are almost entirely theoretical and the American members almost entirely statistical? I think both Hansen & Bonner could make important contributions to the work of the society.

I have spent two weeks with Tinbergen at Den Haag and with him have made a preliminary analysis of the cycle of production and prices of Brazil coffee. I am convinced that Tinbergen's work in cycles is an important advance in this field & appreciate your putting me in touch with him with Erich Schneider. In the coffee problem we made a fair estimate - (without the use of a calculating machine) - of the demand curve and the supply curve of coffee, the lag, and the relation of carry over to price and also the changes in these relations from 1850 to date. The cycles are caused by these relations and I think by studying such a problem ~~from~~ ^{with} Tinbergen's methods we can get a much better explanation of cycles than we get by the usual mathematical

11. Jan. 1933



BONN, den.....
MÜNSTERPLATZ 23 u. 24
TELEFON: SAMMELNUMMER 3441
TEL.-ADR.: BERGISCHERHOF BONN

HAUS I. RANGES
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NEUZEITLICHSTER COMFORT
AUSSTELLUNGSRÄUME
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STAATSTELEFON IN ALLEN ZIMMERN
ZIVILE PREISE
—
GARAGE
—
BANKKONTO: DEUTSCHE BANK, BONN

(4)
technique.
I have just finished two weeks with Schneider. He spent his entire vacation with me at Dortmund and we had an exceedingly interesting discussion of the theories of demand, supply, production functions, cost functions, etc. I had anticipated from Schneider's writings that he was a pure theorist without great interest in quantitative facts but was pleasantly surprised to find that we are thinking along much the same lines and had many questions of common interest to discuss. Yesterday I lectured to Schneider's class at Bonn in German for an hour and discussed the difficulties involved in using statistics to measure the usual theoretical concepts. Several of the economics professors attended the lecture & there was a good discussion.

I shall stop for a day tomorrow at Frankfurt and then for about three days with Marschall at Heidelberg. Then we drive to Rome

and should get there about January 20.
Our address will be in care of

Professor Luigi Amoroso,
Università di Roma.

If and when you have any statistical results from the study of scatter coefficients please let me know. Also I shall be interested to know if it is possible to get them with a reasonable amount of work. If so they will be of great value in many kinds of correlation analysis.

Also if you have finished the article on trends and have a copy I should like to see it.

We look forward to seeing you in Paris in a few months and to some further work with you in Oslo.

Sincerely yours,

Federic W. Waugh.



CONTINENTAL HOTEL

V. B. Oslo
Brevs. nr.

761 A

FRANKFURT A. M., den 13 Januar 1933

MILIEU-HOTEL ERSTEN RANGES
GENÜBER DEM HAUPTBAHNHOF
R. GERSTENBRAND

Telegramm-Adresse:
Contihotel Frankfurtmain

Telefon:

mmel-Nr. 30221 · Fernverkehr-Nr. 34570

Bank-Konto:

sconto-Gesellschaft Frankfurt am Main

stscheck-Konto: 40029, Frankfurt a. M.

Dear Professor Frisch -

Just today I ran across a reference to an article on trend elimination which I had been looking for unsuccessfully for some time. If you have not sent in the discussion to the Statistical Journal I suggest some reference be included.

It is: Bradford B. Smith "The Error in Eliminating Secular Trend or Seasonal Variation before Correlating Time Series", Journal of the American Statistical Association, Vol XX, New Series No. 152, December, 1925.

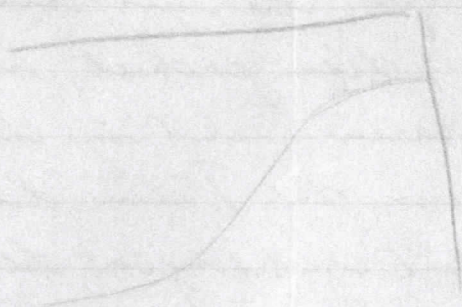
I found the reference in Mordecai Ezekiel's, "Preisvoraussage bei landwirtschaftlichen Erzeugnissen" published in the series of the Frankfurter Gesellschaft für Konjunkturforschung in 1930. ^{S. 23} Ezekiel also says (after discussing the fact that many factors which usually have some influence on prices change in regular manner through time), "Oft wird es notwendig sein, den zusammengesetzten Einfluss dieser Kräfte, die mit dem Kalender variieren, zu messen; dies geschieht, indem man zunächst den säkularen Trend in den verschiedenen Reihen feststellt, dann die Daten davon bereinigt, um endgültig die Beziehungen der Reihen untereinander zu fixieren, allerdings ohne

dasz man dabei der stets vorhandenen Gefahr unterliegen darf, den Einfluss von zeitlich variierenden Faktoren Veränderungen zuzuschreiben, die in Wirklichkeit auf andere Ursachen zurückzuführen sind. "Aus dieser Grunde wird der Einfluss der zeitlich variierenden Faktoren am besten durch eine Bestimmung des Trends in der Relation und nicht durch Feststellung desselben bei jedem einzelnen Faktor gemessen!"

Perhaps a reference to the above statement would also be interesting.

Sincerely

Fredrick G. Waugh.



Frederick V. Waugh

37 bis, Avenue du Roule,
Neuilly s/s, France.
5. March, 1933.

U. B. Oslo
Brevs. nr.

761 A

Dear Professor Frisch; -

We are located here in a little apartment and plan to stay for some time. If possible I want to hear your lectures. Would you please be good enough to let me know the dates they will be given? I am making my plans so that I can if possible make a short visit to London and also stop off to see Schneider again - and perhaps also Timbergen - on my way back to Oslo.

Schneider and Timbergen have been by far the most interesting people I have seen since I left Oslo. I got nothing from Amoroso, although I had some worth while discussions with D'Adario and a few others at Rome. I think I shall get a good deal more from Divisia. He has been very busy the past two weeks and I have not bothered him much, but he is easy to talk to, and I am sure he has some ideas I can use. Tomorrow I shall see him again and discuss some possible work I can do here in Paris.

By the way, Mr. Helgeby wrote me that some of the girls at the Økonomiske Institut are

going to work on the consumption data I
sent him. That is not necessary if they are
busy with something else. I sent it along
to Helgeby mainly so it could be copied
and so that he could do some preliminary
work if he had time. I would like to have
a copy of the data whenever he has it ready,
but I can work out the mechanical
least squares analysis myself whenever I
can find a calculating machine (which are
terribly scarce in Europe). As I wrote Helgeby
I am thinking of the mechanical analysis
by least squares methods as being only
a first step, but I think it may be
necessary in order to simplify the
problem. Sincerely yours,

Fredrick V. Waugh.

410 Greene Avenue,
Aurora Hills,
Alexandria, Virginia,
U. S. A.

September 24, 1933.

Dear Professor Frisch -

Since I got back to the U. S. A. I have been as busy as a one armed paper hanger with the Saint Vitus dance and have really had no time to write letters nor to think further about finishing my European studies. But I want very much to complete our work on the money utility in the U. S. and think I can improve the statistics and add another year and possibly one or two more commodities and get something in shape for publication. Also I am very much interested in the completion of the study of scatter based on the potato quality data. I hope someone in the *Ökonomische Institut* can copy for me the results to date of both

studies and I will try to find time to put them in shape. There probably is not much for me to do on the scatter study - except that I hope it may be possible to experiment with making successive sample studies of the same data and working out a series of regression equations. That combined with the scatter coefficient, maximal roots, and the complete cultivation process ought to give plenty of statistical results to illustrate the difference in methods.

I have received the galley proof of our trend article and sent it back with only a few minor changes in punctuation and wording.

The Bureau of Agricultural Economics found it necessary - as I anticipated - to withdraw its financial support from the New England Research Council, but I found no difficulty in getting something to do. In fact when I came to Washington

24/9-1933

in June I was offered four jobs in one day and three were good ones. I decided to stay with the B. A. C. and am doing some research in Dr. Stuei's division. During the past two months I have started a series of studies of margins between farm and retail prices. At present there is unusual interest in this subject because of the programs of the Agricultural Adjustment Administration and the National Recovery Administration, both of which have features which will tend to increase the costs of processing and marketing. But the subject needs study anyway from a longer time viewpoint. I am planning some other studies which also have a bearing on the government program - particularly a series of studies of consumption and production in the attempt to

measure the ~~influence~~ incidence of the processing taxes, marketing agreements, etc., their effect on consumption and on production. I doubt if this can be done with any high accuracy but we shall do what can be done, and I think it is quite important that we make as good studies as we can along these lines.

There are some plans in the wind for the formation of a research division in the economics of marketing within the Bureau of Agricultural Economics with me in charge. (This is confidential.) I am not anxious for such a job as I would rather do some quiet research than to be constantly bothered with administrative problems - but I do think such a division is badly needed.

In addition to my work at the B.A.E., which is a heavy load, I am planning to teach a course in marketing

at Brookings Institute beginning in about a week.

We are located at Aurora Hills which is three miles from the new Bureau of Agr. Econ. building - down the Potomac River. Margaret is back in school and the family is all feeling fit in spite of some hot weather this summer. If you were never in Washington in July I doubt if you know what hot weather is. We wished many times we could have been back in Norway to make a few little trips with our auto up to the hills and down the fjords.

Margaret and Prudence both remember some of the Norwegian songs they learned and Mrs. Waugh has made a few attempts at

concocting some wienerbrod and
we occasionally have something
resembling a surrogis so you
can see that Norway made a
real impression on us.

Mrs. Wayt asks me to send her
regards to Mrs. Finck and yourself.
When you come to America again don't
fail to see us. Also I hope we may
correspond occasionally and that
you will send me copies of publication
which you think I can understand.

Sincerely

Fredrick V. Wayt.

U. S. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

September 25, 1933

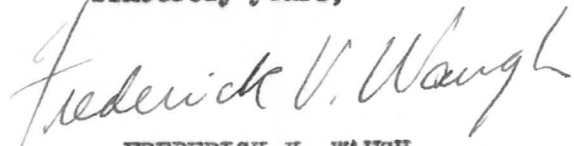
Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

Dear Professor Frisch:

This will introduce to you Dr. F. R. Tomlinson,
a South African, who has recently taken his Doctor's
degree in Agricultural Economics at Cornell University.
Mr. Tomlinson would be very much interested in looking
over some of your work in the fields of demand analysis
and cycle analysis.

With best regards, I am

Sincerely yours,



FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.

U. S. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

September 25, 1933

Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

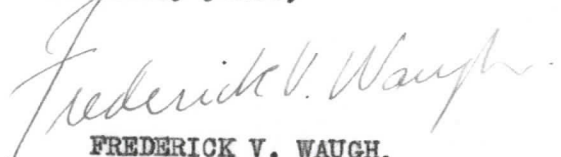
Dear Professor Frisch:

Dr. F. R. Tomlinson, a young South African Agricultural Economist, who has just taken his Doctor's degree at Cornell University is going to be in Europe this fall and will probably go to Oslo to see you some time during the last two weeks of October.

Dr. Tomlinson is not a mathematician although he has done some statistical work particularly in the analysis of trends in production and world marketings of butter and cheese. He would be, I think, particularly interested in seeing some of the results of the work you are doing in the statistical analysis of demand and also the work in cycle analysis.

With best regards, I am

Sincerely yours,



FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

October 28, 1933

Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

Dear Dr. Frisch:

Thank you for your letter. It was very good to hear from you again and to know something about the progress you and Dr. Belz are making.

I should be proud to have the results of my work on marginal utility published together with your work and that of Dr. Belz as you suggested, but hope I can get them in more final form before publication. The mathematical experimenting we did last spring resulted mainly in indicating that there was no value in adding certain factors in a mathematical multiple correlation problem and showed that the study should be limited to the relationship between the three factors- consumption, price and income of each commodity. I think, however, that the relationship between these three variables can best be determined by graphic analysis which would not take a great deal of time and I hope I can get around soon to do something along this line and put the results in better shape.

I find that here in the Department some work is being done to get a monthly index of city incomes and also a monthly index of farm income. This material would be very useful in connection with studies like those we have made since it would be possible to use a much greater number of observations. We have data on the "disappearance" of some commodities each month including meats, eggs, cheese, butter and a few other commodities. We also have monthly prices and it seems to me that we might get much more conclusive results on some of these commodities using the monthly data and studying a period such as 1921 to 1933 when incomes and also prices varied a great deal. It also seems to me that it might be possible from such a study to draw some rather definite conclusions about changes in the utility of money during such a period. This would be particularly illuminating I think in showing what happened to money utility before and after the United States went off the gold standard.

Please give my best regards to Mrs. Frisch and to the men in the Institute.

Sincerely yours,

Frederick V. Waugh

FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of

Statistical and Historical Research.

U. S. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

April 26, 1934

Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

Dear Professor Frisch:

Enclosed is a copy of a paper on marginal utility of money in the United States based on some new studies which I have just completed. This manuscript is not quite ready for publication. I am having some charts made to illustrate it and also I may want to change certain parts of it.

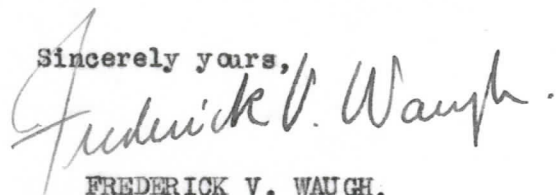
I am sending this to you now hoping that you will have time to go over it soon and give me your criticisms. I am also sending a copy to Professor Fisher and hope that he may have some suggestions.

I received the current copy of "Econometrica" yesterday and was interested to read the summary of the discussion by Mr. Belz. There is a possibility that within a month or so I may be able to get some detailed estimates of annual consumption of all the principal articles of food in the United States since about 1919. This Department is now studying the data on disappearance of food in the attempt to make more accurate estimates of consumption to use in planning adjustments in agricultural production. Possibly this material could be used in making further studies on the basis of single commodities. However, our experience with meats, coffee, butter and sugar indicate, I think, that great care must be used in selecting a commodity for this purpose in order to get one whose utility is entirely independent of other utilities and also one which is used by all groups of population. For this reason the data used in the attached study based on estimates of expenditures for all foods and prices of all foods seems to me to be in many respects ideal and should give results which are much more accurate than could be expected on the basis of data covering any single commodity.

I shall be very much interested in your criticisms of this paper and will be glad to get any suggestions you may have on it.

Please give our best regards to Mrs. Frisch and to the men at the Institute.

Sincerely yours,

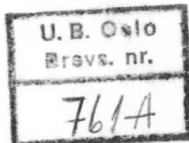


FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.

Enclosure

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

May 15, 1934



Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

Dear Professor Frisch:

Mr. Nelson has asked me to write for the October issue of the "Econometrica" a survey of recent developments in price statistics. I have just written him that I will try to get an article to him on this subject next month. However, I am not very well acquainted with some of the European literature in this field and would appreciate it very much if you would suggest a few of the most important contributions in this field during the last three or four years.

I plan to limit the article to a discussion of available raw data including prices of individual commodities and price indexes and shall not attempt to cover analytical work for the purpose of explaining price movements.

Some of the subjects I shall probably want to cover are as follows:

(1) Historical price series such as the long histories of wheat prices in England and France which have recently been published.

(2) Price indexes including the construction of indexes to measure differences in price movements of different groups of commodities and also the problems of comparing prices at different periods of time and in different places.

(3) Important developments in reporting current prices.

(4) Price spreads or margins. I have in mind, for example, studies of the spread between prices received by farmers for food products compared with prices paid by city consumers. I am doing something here along that line and believe there is some similar work being done in Sweden, Germany and perhaps some other European countries.

(5) Special price reports developed to guide recovery programs or to study the effects of governmental policies. There are some very interesting recent developments in this

15/5-1934

R.F.--5/15/34--2.

field here in the United States and you may know of similar developments in other countries.

I would appreciate it if you could give me a list of some of the recent developments along these lines and also any suggestions you may have as to other topics which should be covered. Also if you have in mind other people in Europe who should be consulted, I would be glad to have you send them copies of this letter asking them to write to me. I am not writing to any one else on this subject.

I should be very much interested to hear what you think of my material on "Marginal utility of money". I believe the charts illustrating this study are now ready.

Sincerely yours,

Frederick V. Waugh

FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.

United States Department of Agriculture
Bureau of Agricultural Economics

WASHINGTON, D.C.

May 15, 1934.

Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

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- (3) Important developments in reporting current prices.
- (4) Price spreads or margins. I have in mind, for example, studies of the spread between prices received by farmers for food products compared with prices paid by city consumers. I am doing something here along that line and believe there is some similar work being done in Sweden, Germany and perhaps some other European countries.
- (5) Special price reports developed to guide recovery programs or to study the effects of governmental policies. There are some very interesting recent developments in this field here in the United States and you may know of similar developments in other countries.

15/5-1934

I would appreciate it if you could give me a list of some of the recent developments along these lines and also any suggestions you may have as to other topics which should be covered. Also if you have in mind other people in Europe who should be consulted, I would be glad to have you send them copies of this letter asking them to write to me.

Sincerely yours,

(Signed) Frederick V. Waugh.

Senior Agricultural Economist,
Division of
Statistical and Historical Research.

U. B. Oslo
Brevs. nr.

761A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS

WASHINGTON, D. C.

June 23, 1934

Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

Dear Professor Frisch:

Thank you for writing to Dr. Marschak and Professor Clark about the survey of price statistics. I have informed Mr. Nelson that I will not be able to write this survey for the October number of *Econometrica*. I find that it is not possible for me to get the necessary material together in time to prepare an article this month as Mr. Nelson requested. I shall, however, be very much interested in whatever material Dr. Marschak and Professor Clark may send and will be glad to send a copy along to anyone Mr. Nelson may find to write the article.

I am enclosing with this letter copies of the charts which go with my study of money utility in the United States. I believe I had not sent you copies and the charts will help, I think, in judging the validity of my conclusions.

I shall be very much interested in your booklet on confluence analysis and am particularly interested to know that you are able to get similar results by using meat and butter for the purpose of measuring money utility. Do you also get approximately the same results when other commodities such as sugar and coffee are used?

In addition to refinements of mathematical and statistical technique for the purpose of analysing data, I am particularly interested in the simpler common sense conditions which seem to me to be very important in judging the utility of money in a country as a whole. It is these conditions that I have discussed mainly in my study and I shall be very much interested in your reactions. I believe that the measurement based on the index of food prices is likely to be the most accurate which could be made. First, because food is used by every person in the country, and second, because the per capita quantity

23/6-1934

R.F.--6/23/34--2.

of food consumed appears to be remarkably stable over considerable periods so that we can fairly assume that the marginal utility of food is approximately constant for periods of several years.

Please give my best regards to Mrs. Frisch.

Sincerely yours,

Frederick V. Waugh

FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.

Charts sent under separate cover.

U. B. Oelo
Brevs. nr.
761A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

August 8, 1934

Professor Ragnar Frisch,
Cowles Commission,
The Mining Exchange Bldg.,
Colorado Springs, Colo.

Dear Professor Frisch:

Thank you very much for your suggestions for revising my manuscript on money utility in the United States. I am making changes along the lines you suggest and would be very glad to submit it for publication in *Econometrica*.

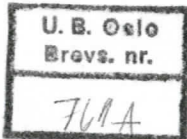
Mrs. Waugh and I both hope you will be sure to stop over in Washington while you are in this country and will be expecting to see you about August 24. I wish it might be possible for you to spend several days here as there are a number of things I would like very much to discuss with you.

Sincerely yours,

Frederick V. Waugh

FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.

Go on with survey



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

September 4, 1934

Professor Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo, Norway.

Dear Professor Frisch:

Under separate cover I am sending you a revised article on money utility in the United States. I have made changes along the lines you suggested. Before this is sent to the printer, will you please finish for me the footnote on page 11, indicating where you will publish your revised analysis of sugar, coffee, meat and butter.

I am afraid I shall not be able to have ready for you by October 1 an article on the results of the Agricultural Adjustment program and the processing taxes on agricultural commodities in the United States. I hope I can do this a little later on and believe there are several very interesting theoretical questions of interest to members of the Econometric Society and an analysis of price and quantity statistics would serve to verify some of the theoretical considerations. However, I am going to be particularly busy this month and do not know of any one else who could prepare the article by October 1. It will take some time to get together the statistical data I would like for such an article and to put the analysis in shape for this purpose.

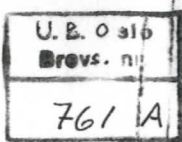
I think I can be reasonably sure of having an article on the results of the Agricultural Adjustment program by the first of next year which would be in time for the April issue of *Econometrica*. Would this be satisfactory to you?

We were glad to see you again and have a chance to talk things over with you. Hope when you come again it will be possible for you to stay longer and to spend some time here in the Bureau of Agricultural Economics.

Please give our best regards to Mrs. Frisch.

Sincerely yours,

FREDERICK V. WAUGH,
Senior Agricultural Economist,
Division of
Statistical and Historical Research.



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

March 20, 1935.

Dr. Ragnar Frisch,
Universitetets Økonomiske Institutt,
Blindern, Oslo,
Norway.

Dear Dr. Frisch:

Thank you very much for your book on confluence analysis which I received a few days ago. This report is, of course, extremely interesting to me, particularly because some of the problems which you have dealt with are among those I was studying when I was working with you in Oslo. I have not had time to go over the whole report as thoroughly as I shall, and it takes me quite a little time to digest material of this kind. However, you certainly have developed a number of interesting and useful ideas which I think can be applied to advantage in multiple correlation problems.

It seems to me that what you have called "bunch analysis" may be one of the best approaches to this problem, and I hope I shall some time have an opportunity to apply this technique and to test it on some of the problems with which we are dealing in our analyses here.

I am not quite sure that your discussion of the inadequacy of the classical sampling theory goes as far into this subject as it might. In Section 33 you come to the conclusion that the standard errors of the regression equations in the four-sets and the five-sets in the constructed example do not give proper warning that these equations are unreliable. I wonder if such warnings are not in both table 33.6 and 33.7. For example, in 33.6 the following beta coefficients would be indicated as unreliable by a comparison in size of the coefficient with its standard error: 12.34, 21.34, 34.21, and 43.21. This would indicate to me that none of the four equations in this table are reliable and that in each of the four equations there is one too many variables. This is the same conclusion that you reach by the technique you have developed. Probably there is some difference of opinion among statisticians as to the use of the standard error of regression coefficients. To me, any regression equation would be unreliable if any of the coefficients in the equation were unreliable, and this would indicate the need for considering certain sub sets of variables.

I wonder if some further experimental work in this general field might not be valuable. I would think one of the most interesting

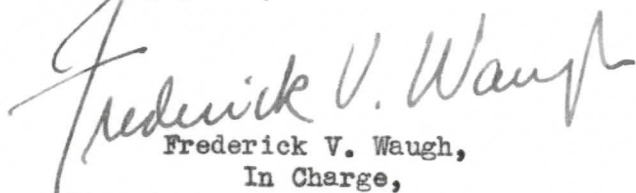
R.F.--2--3/20/35.

20/3-1935

experiments of this kind would be to construct an example similar to the one you have, but with perhaps several thousand observations, and then to draw at random perhaps a hundred samples of a hundred observations each and to work out regression equations for each of the 100 samples. The standard errors of the several regression coefficients could then be worked out empirically and these empirical errors could be compared with the errors as determined by the ordinary sampling formula.

With best regards to you and Mrs. Frisch, I am

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.

U. B. Oslo
Brev. nr.
761A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

Regnyan
Bel. on standard error (1)

April 26, 1935.

Dr. Ragnar Frisch,
Universitetets Okonomiske Institutt,
Blindern, Oslo,
Norway.

Dear Dr. Frisch:

Thank you very much for your letter of April 11. I do not want to appear to be defending the classical method of calculating standard errors of regression coefficients. I believe it has been clearly shown that standard errors calculated in this way do not give a satisfactory measure of the significance of regression coefficients, particularly in cases where such standard errors are needed the most. However, I do think your example, the results of which are given in tables 33.6 and 33.7. indicates that not more than three variables should be taken in this particular problem. I agree that the technique of standard errors has been developed for the purpose of judging each individual regression coefficient. However, looking at table 33.6 with this in mind we would, by the ordinary procedure, come to the conclusion that in each of the four equations one of the regression coefficients is non-significant, and I think if the standard errors were used at all in this case they would indicate clearly that one variable should be dropped from each of these equations. This checks with the results you have obtained by the bunch analysis and by other methods.

In the second paragraph of your letter you say "the technique of testing significance in this case amounts literally to first drawing certain numbers out of a hat and then testing the significance of each of these numbers by drawing numbers out of another hat. Of course the probability that all the numbers first drawn should by this 'technique' turn out to be significant would be very small if the number of variables were great." This comment applies, of course, only to cases in which there is a high degree of correlation among some of the factors which are treated as independent variables. If there were several independent variables, none of which was significantly correlated with others, it is quite likely that the standard error of all the regression coefficients, as calculated by the usual formula, might be small. *man dette er velg et tilfelle som er U.K.*

yes

While it seems to me that in the particular example you have chosen you come to the same general conclusion by the use of the ordinary standard error of regression coefficients that you

R.F.--2--4/26/35.

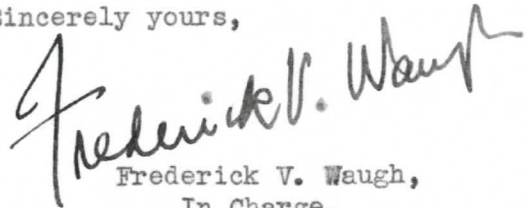
26/4-1935 (1/2)

do by other methods, this is far from proof that the standard error of the regression coefficients is correctly figured. I am very much interested in this problem and one of my assistants, Mr. Edgar L. Burtis, and myself have started to work on this problem along the lines suggested in my previous letter; that is, we plan to work out from 50 to 100 sample problems chosen from the same universe and to observe the actual variation in the regression coefficients as determined by these samples.

I have read through your book several times and find it extremely interesting. The bunch analysis appeals to me particularly and I am sure it will prove to be a useful method of analyzing multiple correlation problems.

With best regards, I am

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.

U. S. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

July 3, 1935.

Dr. Ragnar Frisch,
Universitetets Okonomiske Institutt,
Blindern, Oslo,
Norway.

Dear Dr. Frisch:

Recently I have been playing with the possibilities of different kinds of multiple regression coefficients. I thought I had discovered a pretty good, simple form of equation until I worked out an example, the results of which seem rather peculiar. You can probably tell me what is wrong with the sort of regression coefficient which I have outlined in the attached note.

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The general idea of what I am calling a composite regression equation was probably suggested to me by some of the discussions we had in Oslo and seems to me to be a perfectly good idea. However, in working out an example I get peculiar results which are stated on page 12. One of the coefficients in the composite regression equation is lower than any of the corresponding coefficients in the elementary regression equations. This does not look plausible to me, as I would think each coefficient would necessarily be some sort of average of the coefficients obtained in the elementary regressions. Of course, there is always a possibility that I have made an error in the calculation of these coefficients, but I doubt if this is the difficulty. I worked through the problem twice, and the second time carried the calculations to a greater number of decimal points, but the only change in the result was a slight difference in the fourth and fifth decimal places of the regression coefficients. These changes are made in pencil on the manuscript

I would appreciate it very much if you could find time to look this over and probably you can find a simple explanation for the peculiar results obtained in this problem.

Please give our best regards to Mrs. Frisch. I wish we could arrange to take our vacation in Norway this summer.

Sincerely yours,

Frederick V. Waugh
Frederick V. Waugh,
In Charge,
Division of Marketing Research.

Enclosure.

U. B. Oslo

Brevs. nr.

761A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

October 8, 1935.

Dr. Ragnar Frisch,
The Econometric Society,
Slemdalsveien 98,
Oslo, Norway.

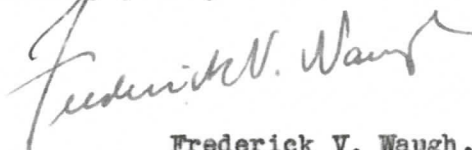
Dear Dr. Frisch:

Thank you very much for your comments on my attempt at a composite regression equation. I am not sure I followed very clearly your geometric picture of the problem, but you have indicated what I suppose is the reason why a given regression coefficient may fall outside the range of the elementary regression coefficients; that is, that some of the weights may be negative. It does not seem to me that this necessarily makes my formulation of the equation illogical or unsound. I agree, of course, that it is mathematically possible that the "true" regression coefficients might satisfy an equation of the form $a_1 + a_2 + \dots + a_n = 0$. While this is mathematically possible, it is statistically practically impossible if enough decimal points are carried.

However, after experimenting with this type of regression equation a little I came to the conclusion some time ago that it was not particularly satisfactory for most relationships. Probably one of the several methods presented in your confluence analysis would be more satisfactory in most cases.

Thank you very much for your comments.

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.

Experiments in Frischi

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U. B. Oslo
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UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

January 29, 1936.

Professor Ragnar Frisch,
Slendalsveien 98,
Vinderen Pr.,
Oslo, Norway.

Dear Professor Frisch:

Your letter of January 4 reached me a few days ago and I have since done what I could to check the amount of time required in working out the complete adjoint of the correlation matrix. I am sending my results along to you and would be very glad for you to check them over.

Evidently you have the impression that my article in the Journal of the American Statistical Association was intended as a criticism of your work on confluence analysis. This is not at all true, and I am very sorry if anything in the article can be construed as an adverse criticism of your method. As a matter of fact, the whole article was intended only as setting forth the mechanics of deriving the adjoint of the correlation matrix and from it deriving the regression equations with their standard errors, the partial correlation coefficients, and the other common and classical measures of regression and correlation. Instructions for this purpose were drawn up for use here in the Bureau. After using them on a few problems it occurred to me that the instructions might be of some interest to the Journal of the American Statistical Association and I sent them along with a short note, the first two paragraphs of which briefly mention some other work in this field. There are in this note only two sentences which refer to your work on confluence analysis. The first of these states that your method is to be preferred whenever a complete and detailed understanding of all relationships is necessary. The second states that in a problem of 8 or 10 variables the work involved in your method is almost prohibitive. Perhaps the word "prohibitive" is too strong. I would agree that there may be many cases in which 8 or 10 variables are involved that perhaps should be studied by your method even though a great deal of time is required.

However, my only comparison between the method I propose and your method which you call tilling (I think your original Norwegian word "ophevning" is more descriptive) was concerned with the amount of time required by the two methods. I am not sure what you mean by saying that my comparison was not just because I say that the work involved in studying a problem of 8 or 10 variables by your method is almost prohibitive and then working out a problem in 7 variables.

29/1-1936

I made no comparison of the amount of time required in working out this 7 variable problem by the two methods, and, in fact, did not even give the amount of time required by my own. However, if I had wanted to show a great deal of difference in the time between the two methods I would have chosen a problem of 10 or 12 variables, in which case the difference is even more striking.

Your letter is divided into three parts. The second part deals with the nature of the standard errors found by the classical method, and the third part deals with the general problem of reliable criteria of confluency. I shall not attempt to go into these last two problems in detail in the present letter. I think they are not involved in my article in the Statistical Journal. I was only concerned in that article with what I think is the simplest and quickest way of deriving the common classical measures of correlation and regression and not with a comparison of the meaning of these measures with the meaning of the measures you proposed. In fact, as I mentioned above, I indicated that your method is to be preferred whenever a complete and detailed understanding of all the relationships is necessary.

I have some questions about the last two subjects in your letter and would like to take them up with you later on after I have had time to study in more detail the potato quality problem which is treated in your book. I am not satisfied that the results you obtained are correct, but I have not yet found time to go into this subject in sufficient detail to write you fully. I shall, therefore, limit this letter mainly to a comparison of the amount of work involved in getting the adjoint of the correlation matrix by your method of tilling and my modification of the Doolittle method.

The total number of multiplications and divisions required in my process of deriving the adjoint of the correlation matrix, including the necessary checks, amounts to

$$\frac{n(n^2-1)}{2}$$

where n represents the number of variables. Of this total number there are n divisions. These are the reciprocals of

$$r_{ii}^2$$

The amount of copying by my method is reduced to a minimum and is very much less than the amount of copying needed by your method. I think, therefore, that it is fair to assume that if our people were well trained in my method they could make about the same number of multiplications in an hour as your people can, - about 100.

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Figured on this basis, a 7 variable problem with 6 decimal places would require by my method approximately 1-2/3 hours, as compared with 41 hours by your method. The 41 hours by your method represents approximately six working days of seven hours each. An 8 variable problem by my method would take about 2 1/2 hours, as compared with 118 hours by your method. The 118 hours by your method would mean approximately 17 days' work. A 10 variable problem by my method would take 5 hours, while by your method it would take 857 hours, or approximately 5 1/2 months, figured on a basis of 1,875 working hours in a year. A 12 variable problem would require by my method 8 1/2 hours, whereas by your method it would take 5,652 hours, or approximately 3 years.

I have attempted to check the time involved by having two of our clerks work out three problems. Their time was more than that which I have indicated above, but I think this is very largely due to the fact that they have had little experience with the method, and it was necessary to interrupt them several times to work on other problems. However, the actual time used was as follows:

First, following your suggestion I repeated the solution of the 7 variable problem in my article. By the way, you asked me for the original correlation coefficients in order that you could work out the same problem. These coefficients are given in Table 1 on page 696 of my article. I gave these coefficients to one of our clerks, asking her to rework the table of P_{ij}, carrying it out to 6 decimal places. Her time on this problem was 3 hours and 15 minutes instead of the 1-2/3 hours which would be required at the rate of 100 multiplications an hour. I am reasonably sure that the clerk who ran through this problem could do it in less than two hours. There are two reasons for the longer time. The first is that she had previously used this method on only two problems. The second is that she is in charge of our statistical unit and was constantly interrupted. I am sure this slowed up her work a great deal.

In order to check further on this matter I gave one of our other clerks two problems. One was the 8 variable potato quality problem which is worked out in your book. It happened that this was the first time this particular clerk had worked completely through a problem by my method, although she had had a little experience with the ordinary Doolittle solution. Her time on the 8 variable problem was 4 hours and 30 minutes, whereas it should be possible to complete the problem in about 2 1/2 hours at the rate of 100 multiplications an hour. This clerk made several mistakes in the back solution for the P_{ij} values and for that reason an unnecessary amount of time was lost in making corrections.

After this clerk had finished with the 8 variable problem I gave her a 10 variable problem in order to see if on her second

R.F.--4--1/29/36.

29/1-1936

attempt she could make greater speed, and found that her time was very much better and averaged approximately the 100 multiplications an hour which you take as normal. She worked the 10 variable problem in 5 hours, which is just about at the rate of 100 multiplications an hour. I am quite sure that if I gave her another 8 variable problem today she could do it in about $2\frac{1}{2}$ hours.

If you would care to check on the 10 variable problem I am enclosing it on a separate sheet. The original zero order correlation coefficients are given only two decimal places, but we carried the computations out to six decimal places.

I think there can be absolutely no question that if what we want is mainly to derive the adjoint or the reciprocal of the correlation matrix it can be done in very much less time by my method than by the method of complete tilling. The difference is more and more striking the larger the problem becomes. Dr. Oscar Adams of the U. S. Coast and Geodetic Survey tells me that his people can work out one of the elementary regression equations in a problem of 100 variables in about 10 to 12 days. So much for the time involved in my process as compared with the process of complete tilling.

I might comment briefly on some of the other points mentioned in your letter. You speak of a method of "one-way determinant computation" which you believe will beat the Doolittle process in deriving a single regression equation. I am not sure I know what your process is, but remember very well the experiments we made on problems of three variables and four variables. In the case of small problems of this kind I think it is quite correct that the solution of the equation by the ordinary determinant process will beat the Doolittle solution. I believe, however, that in the case of problems involving more than about five variables the Doolittle solution has a distinct advantage as far as time is concerned.

Probably correct

You also object to the fact that in the reduction of the problem by one variable at a time, divisions are used, whereas in your process you use multiplications. You point out that it may happen that some of the values by which one is led to divide may become very small. This is, of course, true, but I think here, as in most of your discussion, you assume that in most problems some of these values will become small. This is not necessarily true. In the three problems I have mentioned above the smallest figure used as a divisor in the 7 variable problem was 0.448. In the 8 variable problem the smallest divisor was 0.677, and in the 10 variable problem the smallest divisor was 0.365. Also, I think it is a fairly simple matter to add more decimal places in case some of the divisors should become small.

We must prepare for it

[Handwritten scribbles]

29/1-1936

While I do not feel able at the present time to discuss in detail the relative merits of the usual standard errors as compared with your bunch analysis for the purpose of testing significance, it seems to me that your whole argument, both in your letter and in your book, applies only to cases in which we have a multiply collinear set; that is, to cases in which the scatterance among the independent variables is approximately zero. In this case both the regression coefficients and the standard errors obviously lose their significance. I think it is also obvious, however, that when the scatterance among the independent variables is significantly different from zero, your criticism is not so important.

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In this connection the scatterance among the independent variables in the 7 variable problem in my article was 0.168. When the last two variables were dropped the scatterance among the four remaining independent variables was 0.417. In the potato quality problem of 8 variables the scatterance among the 7 independent variables was 0.396. In the 10 variable problem on milk production the scatterance among the 9 independent variables was 0.084. These scatterances can be determined readily in the tables used for reducing the equations. The total scatterance in the big set is simply the product of the elements in the diagonal of the first set of reduced equations; that is, it is the product

$$r'_{11}r'_{22}r'_{33} \dots r'_{nn}$$

The various (n-1) order sub-scatterances can be found by multiplying the total scatterance by the values of P_{ii} .

In the 10 variable problem the scatterance among the independent variables is fairly low. However, it can be readily seen from the table that the smallness of this scatterance is due mainly to variables 8 and 9. If these two were dropped the scatterance among the remaining 7 independent variables would be fairly high.

You say that if we followed my rule there would not be a very great chance of accepting a wrong equation, but that the rule becomes distinctly misleading if used as a criterion of which variable or variables to eliminate. It seems to me that this, as well as the rest of your argument, applies only to cases in which the scatterance among the independent variables actually is close to zero. The standard errors of the regression equations in the potato quality problem are of interest in this connection. You will remember that your analysis indicates that the most significant set of variables is 1245. You then come to the conclusion that we cannot include all four of these in the same set, but that we can take either the set 124 or the set 125. The standard errors of

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the regression coefficients indicate that the regressions of 1 on 2, 1 on 4, and 1 on 5 are all significant and that none of the other four regression coefficients are significant. You will note that in this case the standard errors pick out the same set, 1245, which you would pick out by your technique, although they do indicate that it is possible to include all four of these variables in the same analysis. I doubt very much that it is by chance that the standard errors picked out these same four variables which were chosen by your technique and dropped the same four.

|| alpha
galt

After rereading your letter I note one or two minor points which I have not covered. First, my formula 9 is correct in its present form, I think.

You have evidently not understood my method of deriving the values for P_{ij} . This does not amount to n independent Doolittle solutions. The reduction of equations by the Doolittle method is done only once for the set and the values of P_{ij} are determined by the formulas given at the top of page 699. I think you can follow these formulas all right, although they were condensed somewhat by the editor of the journal.

In following my method of getting the reciprocal matrices it is necessary to copy only the figures shown in tables 1 and 2 on page 696. All the figures in these tables are obtained directly on the calculating machine. No work sheets are needed except the forms for tables 1 and 2.

B! ||

It would be possible, of course, to use my method to get regression coefficients in all the sub sets. The amount of work involved would, I think, be practically the same as that necessary for your method, although it would be possible to modify my method to save perhaps from 1/3 to 1/2 the time. Some time ago I experimented a little with a modification of the Doolittle process which makes it possible to get the scatterances in all the sub sets in a fairly simple and direct way. The same modified process could be used to get the adjoint matrices or the reciprocal matrices, but the complete solution would take from two to three times as long as it would to get the scatterances alone.

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This letter is necessarily rather long. I think my position could be summed up about as follows: Whenever the usual measures of correlation and regression are satisfactory, the method I proposed will give the necessary results with the least possible work, particularly in cases where we are dealing with a large number of variables. Whenever we are dealing with sets which really are multiply collinear your technique of bunch analysis can be used to advantage. The last paragraph of your book, I think, is particularly good. You say in that paragraph that your technique will not solve

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method is multiply collinear?

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29/1-1936

all problems of significance, but that it will be helpful in a great number of cases and that for many kinds of problems it will be indispensable. I agree with this statement, but I do not think you would wish to argue that the usual measures of regression and correlation never have any meaning and that your technique should be used for all kinds of problems.

Please give my best regards to Mrs. Frisch.

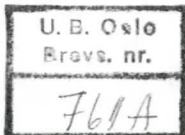
Sincerely yours,

Frederick V. Waugh

Frederick V. Waugh,
In Charge,
Division of Marketing Research.

*Personally, I
would not use it for
practically all
problems. If the
situation is thus
clear, they use known
means of
regression,
correlation
and significance*

Enclosure.



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

adj. of Reciprocals *Reputable* (11)

March 11, 1936.

Professor Ragnar Frisch,
Slemdalsveien 98,
Vinderen Pr.,
Oslo, Norway.

Dear Professor Frisch:

In your recent letter you asked for some further information about the possibility of calculating a complete set of scatterances by the Doolittle method, or the Gaussian method, if that name is preferred. I have commonly called it the Doolittle method simply because it is usually known by that name in this country.

I am enclosing three photostatic sheets. The first of these outlines a method of obtaining scatterances by a modified Doolittle method. I think the method will be fairly obvious to you, although, as you will note, I included no proof, as this was drawn up only as a guide for the mechanical work. I do not think the notation on this page is very good. Perhaps you could suggest something better. However, as far as the method goes, it works all right, although it is obvious that the amount of work increases rapidly as the number of variables increases. Each time a new variable is added it doubles the number of lines which must be computed.

I think that while this modified method may be useful in some problems, you will be more interested in the second and third pages which can be used in connection with what you call the "Tilling" process. The process outlined here, as you will notice, is first to get the reciprocal correlation matrix in the big set of n variables and to work back from this to somewhat similar tables in the sub sets, leaving out one variable at a time. The tables in the sub sets are neither reciprocal matrices nor adjoint matrices, but they are rather the adjoint matrices with each element multiplied by a constant. I think these tables are probably just as useful as the adjoint tables themselves. If not, of course, the adjoint tables could easily be derived by dividing each figure by a constant.

I think that if a complete Tilling analysis is made in a problem of several variables the method outlined here is probably somewhat shorter than the method outlined in your article. Of course, it may not be necessary in all cases to make a complete

R.F.--2--3/11/36.

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(11/3)

analysis in all sets. In the potato quality problem it might take less time by the method you outlined than by the one presented here because, for example, you decided from your analysis that it was not worthwhile to include more than three variables in the problem and therefore would not need to proceed to study the sets of four variables. Of course, on the other hand, in many problems we might arrive at the opposite kind of conclusion; that is, for example, we might start with a set of 8 variables and decide that we could take 6 or 7 of them. In that case I think the method outlined here would save a good deal of time because it would not be necessary to study sets of less than 6 or 7 variables.

I think that you can follow the mathematics on pages 2 and 3 without any mathematical proof. The formulas are derived from Sylvester's theorem concerning the determinant values of adjoint matrices.

With best regards to you and Mrs. Frisch I am,

Sincerely yours,

Frederick V. Waugh

Frederick V. Waugh,
In Charge,

Division of Marketing Research.

Enclosures.

U. B. Oslo
Brevs. nr.
761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

Professor Ragnar Frisch,
Slemdalsveien 98,
Vinderen Pr.,
Oslo, Norway.

April 22, 1936.

Dear Professor Frisch:

I am very much interested in your letter of March 30, particularly the part which discusses a simple and quick method of computing all the scatterances and sub-scatterances among a set of several variables. When you have this worked out I would appreciate it very much if you would send me an outline of the method.

It occurs to me that if there is a method of computing all the scatterances and sub-scatterances quickly and easily that this might offer a simple method of making a complete solution similar to that you have developed in your Tilling process.

The squared regression coefficients in any sub-set can be calculated by the following formula based on Sylvester's theorem:

$$B_{ij}^2 = (\Delta_{ij} \Delta_{ijc} - \Delta_{ijc}^2) / \Delta_{ijc}^2$$

I think this formula can be computed readily on a calculating machine without copying any intermediate steps. This can be done easily if, in addition to the scatterances, a table is prepared giving the reciprocals of the squared scatterances, then by three multiplications of the machine the squared regression coefficients can be obtained directly.

Of course, in some problems there may be a question as to the sign of the regression coefficients, but I wonder if in most cases we could not select the most reliable set of variables by calculating the scatterances and sub-scatterances in each set and sub-set, and also calculating in each case B_{ij}^2 , B_{ji}^2 , and $B_{ij}^2 B_{ji}^2$

I would think the expression

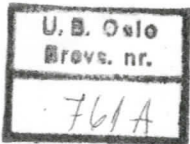
$$B_{ij}^2 B_{ji}^2$$

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would be a good measure of the "regression spread." If the relationship were perfect this expression would be equal to unity.

Sincerely yours,

Frederick V. Waugh
Frederick V. Waugh,
In Charge,
Division of Marketing Research.



200

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

August 29, 1936.

Professor Ragnar Frisch,
Slemdalsveien 98,
Vinderen Pr.,
Oslo, Norway.

Dear Professor Frisch:

You will be interested in the last few pages of the attached paper which I am submitting for publication in the Journal of the American Statistical Association. If my discussion does any injustice to the bunch analysis technique, I hope you will call it to my attention. I suppose it will be some time before this paper is published, and I would be glad to make changes in it if it is at all incorrect, or to have you write a reply to be published with the paper.

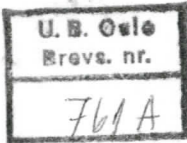
With best regards, I am

Sincerely yours,

Frederick V. Waugh

Frederick V. Waugh,
In Charge,
Division of Marketing Research.

Enclosure.



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

September 21, 1936.

Professor Ragnar Frisch,
Slemdalsveien 98,
Vinderen Pr.,
Oslo, Norway.

Dear Professor Frisch:

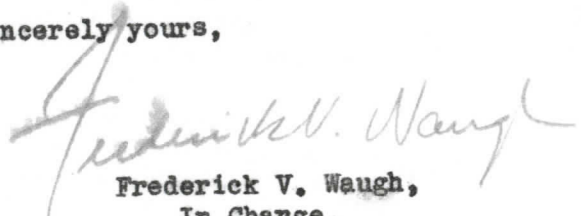
A couple of weeks ago I sent you a paper, "The Complete Analysis of Regression Systems in Several Variables." I have just received a note from the editor of the Journal of the American Statistical Association suggesting that the material in this paper be written as two separate articles; the first one dealing with the mechanics of getting the cofactors in minors of all possible orders, and the second discussing possible ways of testing the reliability of the various sets.

I think the second paper would require a much more comprehensive treatment than I have given it and I rather doubt whether I shall attempt it, - at the present time, at least. Probably I shall submit to the Journal a short note referring to my article in the December, 1935 issue of the Journal and indicating how the P tables in the various subsets can be derived from the P table in the big set of n variables.

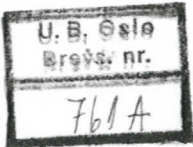
I would, of course, be very interested in any comments you may have on my general criticism of the bunch analysis technique, and perhaps later on I may attempt a rather serious paper on this subject, but unless you have done so I suggest that you do not write out any careful reply to the paper I sent you because it is not likely to be published in that form.

With best regards, I am

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

May 4, 1937.

Prof. Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo,
Norway.

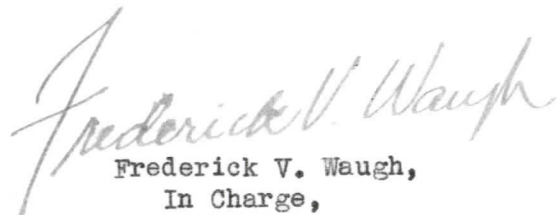
Dear Prof. Frisch:

In your "Confluence Analysis" you mentioned a mimeographed summary of lectures given in Leyden in 1923 in which you discuss matrix algebra and its application to statistics. I am enclosing a check and would like very much to get a copy of these lectures if you still have one available.

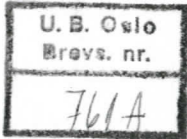
I still have not finished revising my statement on the determinateness of regressions, but hope I will be able to do so within a couple of weeks. I have been intending to send you a copy as soon as it is ready, but perhaps the best thing to do now would be to wait and talk it over with you when you are in this country. A recent card from Mrs. Frisch indicated that you probably would come to Washington on your trip to the United States, and I hope you are going to be able to find time to discuss some of these problems with me and also to spend some time with my family, if you can possibly do so.

With best regards, I am

Sincerely yours,


Frederick V. Waugh,
In Charge,
Division of Marketing Research.

Enclosure.



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

May 15, 1937.

Prof. Ragnar Frisch,
Universitets Økonomiske Institutt,
Blindern, Oslo,
Norway.

Dear Prof. Frisch:

If you plan to stop off in Washington this summer I would like very much to get together a small group of statisticians for an informal conference with you. I would appreciate it very much if you would let me know when you plan to be in Washington and whether or not it would be convenient for you to spend an hour or two with such a group.

Also, it might be well to schedule ahead of time the general topic which is to be discussed. In this connection I think there are at least three topics which you could discuss with little or no preparation and which would be interesting to a number of people in the Department of Agriculture and elsewhere in the Government. These are: (1) confluence analysis; (2) the measurement of utility and its relation to problems of price analysis, to statistics of levels of living, to taxation problems, etc.; and (3) the statistical evaluation of trends and cycles.

There is another statistical problem which recently has been interesting a number of us here who are dealing with time series. I do not know how much work you have done on it, but doubtless you would have some ideas. I refer to the problem which is sometimes called serial correlation. As you know, when we deal with such time series as prices and supplies, the successive observations of price are commonly correlated with each other as are also the successive observations of supplies. This tends to reduce a number of degrees of freedom in the problem and makes the results in many cases much less reliable than might be anticipated on the basis of the usual estimates of error. Some of the statisticians in the Department have been experimenting with the possibilities of reducing the amount of serial correlation by differencing the original series and by similar means. There has also been some attempt to estimate the number of degrees of freedom. Up to the present time, however, attempts to find a good solution to these difficulties has not been any too successful, and for that reason this would be a very timely subject if you care to discuss it.

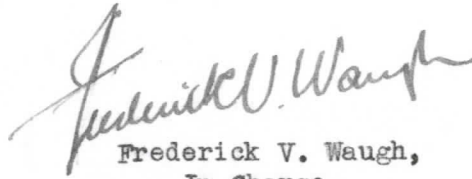
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15/5-1937

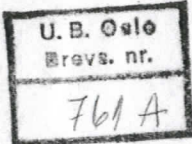
The main question is, however, whether or not we could get you to confer with a small group for an hour or two when you are here. I am sure a number of the men here would like to meet you and would be interested in any statistical problem you wanted to discuss.

With best regards, I am

Sincerely yours,

A handwritten signature in cursive script, reading "Frederick V. Waugh". The signature is written in dark ink and is positioned above the typed name.

Frederick V. Waugh,
In Charge,
Division of Marketing Research.



UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

September 2, 1937.

Prof. Ragnar Frisch,
Vinderen,
Oslo, Norway.

Dear Professor Frisch:

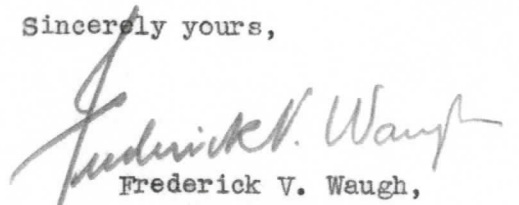
We have been having a lot of rush typing work here and do not yet have your report mimeographed. However, we are working on it and I hope we will have it ready for distribution in a week or so.

Under separate cover I am sending you a copy of the paper I told you about when you were here. I hope you can find time to go over it and give me your criticism of it before it is published. I am also sending a copy of the paper to Mr. Stephan of the American Statistical Association. I should be glad to get in addition to your criticism of the paper any suggestions you may have as to an appropriate place to publish something of this kind.

We were very glad to see you and I hope next time you come to the United States you can spend more time with us.

With best regards,

Sincerely yours,


Frederick V. Waugh,
In Charge,
Division of Marketing Research.

U. S. Oslo
Brevs. nr.

761A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

October 9, 1937.

Professor Ragnar Frisch,
Vinderen Pr.,
Oslo, Norway.

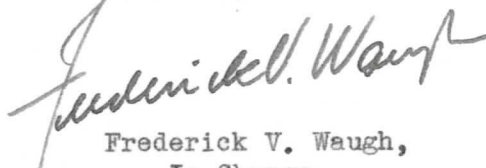
Dear Professor Frisch:

The copies of your talk, "Methods of Measuring the Relative Cost of Living", have just come from the Mimeograph Section. I am sending you under separate cover 25 copies. I am sorry this was delayed so long, but we have been particularly busy in my office during the past few weeks and I was unable to get the material mimeographed anywhere else. I have supplied copies to the following persons: Dr. Cowles, Mr. Bean, Dr. Stine, Miss Monroe, Dr. Copeland, Miss Kneeland, Mr. Wilcox, and Mr. Elliott. I shall be glad to send copies to anyone else you suggest. I could also send you a few more copies if you have any use for them.

I just received your letter of September 18 and appreciate very much the thoroughness with which you went over my manuscript, "On the Validity of an Estimate from a Multiple Regression Equation." I really had not realized how exactly the mathematical treatment in this manuscript followed that in part of "Confluence Analysis." I intend to rewrite the manuscript, making the first part of it dealing with the significance of zero scatterances very short, simply referring to the mathematical development in "Confluence Analysis" and proceeding to show how the idea might be applied to practical problems. Thank you very much for your criticism of this part of the manuscript and also for your suggestions on the rest of it.

With best regards, I am

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.

Note: The men mentioned in the 1st TP have each been given several copies of your paper & have been asked to distribute it to others in their organizations FVW

U. S. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

January 7, 1938.

Professor Ragnar Frisch,
Vinderen,
Oslo, Norway.

Dear Professor Frisch:

Mr. Been and I have rewritten the paper "On the Validity of an Estimate from a Multiple Regression Equation." I plan to submit this to some journal for publication in the near future. I am inclined to think the best place would be *Econometrica*, and if you think the article is suitable for that journal I shall be glad to submit it.

The major revision is on the section from pages 9 to 18 dealing with the test of significance of an observed scatterance. We have gone over very carefully your treatment in "Confluence Analysis." It seems to me that while we use some of the same mathematical procedure here the application is quite different from the one you had in mind in most of the treatment in "Confluence Analysis." I would appreciate it if you would read particularly the part of the present draft beginning at the bottom of page 15 and ending at the bottom of page 17c.

I hope you can find time to look the whole paper over again and would greatly appreciate any further comments you may have.

With best regards to you and Mrs. Frisch, I am

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.

Enclosure.

U. B. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

April 15, 1938.

Professor Ragnar Frisch,
Vinderen,
Oslo, Norway.

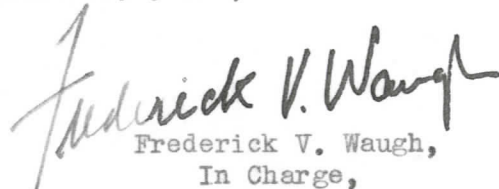
Dear Professor Frisch:

I am anxious to get your comments and suggestions on the manuscript by Mr. Been and myself which was sent to you in January. I realize, of course, that you are very busy and I do not want to impose upon your good nature, but I really believe the ideas in the manuscript are rather important and represent quite a different point of view from that expressed in "Confluence Analysis." I think we should offer this material for publication in the near future, and I should appreciate very much any suggestions from you which will help to make the presentation more clear or more correct.

I happened to think of this subject again because Mr. Mendershausen was in to see me a few days ago and I have since read his article in the current issue of *Econometrica*. While I have not yet had a chance to discuss this in detail with him, I am inclined to think that the application of your principles of bunch analysis to this problem proves nothing, and that in this case, like most others, it would be very easy to apply your former ideas of scatterances along the lines suggested by Mr. Been and myself in a way which would provide a real test.

With best personal regards, I am

Sincerely yours,



Frederick V. Waugh,
In Charge,
Division of Marketing Research.

U. S. Oslo
Brevs. nr.

761 A

UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF AGRICULTURAL ECONOMICS
WASHINGTON, D. C.

May 27, 1938.

Professor Ragnar Frisch,
Slemdalsveien 98,
Vinderen Pr.,
Oslo, Norway.

Dear Professor Frisch:

Thank you very much for your letter of May 11 commenting further on the paper by Mr. Been and myself. I appreciate very much your willingness to look over two different versions of the rather long paper and give us your views.

Your letter makes one important point which I had not realized and which calls for a revision of part of our discussion. That point is that our procedure of finding the smallest root which would make the correlation vanish is unnecessary, and that all we need is to show that the estimated true correlation matrix is positive definite and non-singular.

It still seems to me, however, that you misunderstand the nature of our proposed test of the significance of an observed scatterance. Although we do use the same mathematical procedure as that developed in "Confluence Analysis", I think our application is entirely different and usually leads to decided differences in conclusions. I would suggest, for example, that you reread the discussion beginning with the last paragraph on page 17a of our manuscript and ending with the middle paragraph on page 17c. Briefly, this shows that the application of our procedure to the potato quality problem, treated in "Confluence Analysis" indicates that it is legitimate to use all eight quality variables in making an estimate of expected prices. Whereas, your analysis by the confluence technique indicated that it was legitimate to use only two. I believe our analysis is correct, and, moreover, it seems to me quite apparent that in a problem of this kind the composite regressions, like those you develop in "Confluence Analysis", have no meaning.

Your letter of May 11 indicates that you finally agreed that we use the corrected correlation matrix for a different purpose; that is, to find out whether or not the calculated scatterance in a set of independent variables is significant. However, you do not appear to recognize the other important difference between this procedure and yours. You say, for example, "But your line of approach to this is in my opinion not quite satisfactory, particularly because you seem to consider the error variances only as some sort of errors of observation, while they express in fact the parts of the variables that are not systematically connected." We tried to state

as definitely as we could that we were concerned here only with errors of observation and not with your broader notion of "disturbances." We tried to make this clear on page 3 of the manuscript, and to speak consistently throughout the manuscript of errors of observation or errors of measuring or estimating each of the separate variables. It seems to me that you ignore here one of the most important differences between our procedure and yours. We are concerned entirely with errors of observation, and in using our estimates of such errors in testing the significance of an observed scatterance among a group of variables we wish to use as a set of independent variables any one of the ordinary classical multiple regressions. We claim this gives a satisfactory test and that it is not necessary to consider broader "disturbances". Thus, our λ is not your λ

I am not sure yet what we should plan to do with this paper, but my present inclination is to condense it rather drastically, but to include in it a discussion of all three of the topics; that is, a test of the observed scatterance in terms of the errors of observation of the separate variables, the definition of the region of extrapolation, and the method of computing the error of an individual forecast. In line with your comment, the discussion of our test of a scatterance can, of course, be simplified a good deal, and I greatly appreciate your calling our attention to this fact.

and I would just as soon use some other terms

With best regards to you and Mrs. Frisch, I am

Sincerely yours,

Frederick V. Waugh

Frederick V. Waugh,
In Charge,
Division of Marketing Research.

P.S. I am vaguely considering the possibility of taking another year off and going to Europe in 1939-40. If so I hope I shall have an opportunity to do some further work with you. Again, many thanks for your suggestions. I appreciate them all - including the ones I think are wrong.
God Day og Adj. F.V.W.

THE AMERICAN FARM ECONOMIC ASSOCIATION

Office of the President

F. V. WAUGH, PRESIDENT
United States Department of Agriculture
Washington, D. C.

WILLIAM G. MURRAY, VICE-PRESIDENT
Iowa State College
Ames, Iowa

G. H. AULL, VICE-PRESIDENT
Clemson Agricultural College
Clemson, South Carolina

WARREN C. WAITE, EDITOR
University Farm
University of Minnesota
Saint Paul, Minnesota

ASHER HOBSON, SEC'Y-TREAS.
University of Wisconsin
Madison, Wisconsin

October 27, 1947.

Dear Professor Frick,

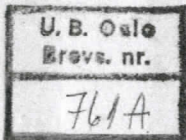
I just finished reading your paper on Multilateral Trade Balances. It is extremely interesting, and I hope you will continue your analysis of this critical problem.

Please drop me a line and tell me how you - and your family - are. I was sorry not to see you at Atlantic City. I would have gone there if I had known you were attending. I saw Divisia and Tinbergen recently in Washington, and have seen a few people who have seen you since the war. So I know you really are alive and well, but it would be fine to get direct confirmation.

I am on the staff of the Council of Economic Advisors. It is the most interesting job I have ever tackled. Certainly, it is the most important. In fact, I sincerely think we have the most important assignment ever given to a group of economists. My own responsibilities cover consumer problems, food and agriculture, and statistics. You will realize the breadth of these subjects, and the impossibility of doing much original work on them.

My family is well. Margaret graduated, with high honors in mathematics, from Oberlin this year; and is a teaching assistant in the University of Wisconsin. Prudence has entered the University of Massachusetts. Douglas (who was born after our trip to Oslo) is in the 8th grade of school.

With our very best regards to you and your family,
Sincerely,
Frederick V. Waugh
1506 So 26 Street, ...



EXECUTIVE OFFICE OF THE PRESIDENT
COUNCIL OF ECONOMIC ADVISERS
WASHINGTON 25, D. C.

August 2, 1948

Professor Ragnar Frisch
Vinderen
Oslo, Norway

Dear Professor Frisch:

I found your outline, "A System of Concepts Describing the Economic Circulation and Production Process", very stimulating. Careful theoretical work of this kind certainly should help us to understand problems of economic stability and instability.

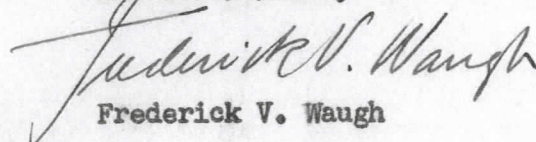
I suppose, however, that this discussion of concepts is only a start toward a much more important undertaking. I must confess that after I had read this document I had some difficulty in seeing how you intended to apply the concepts to the work of the Sub-Commission. In a general way, of course, it is important for any such commission to start with meaningful concepts and with analytical tools such as those you have provided in your ingenious diagrams. Yet I would think that the group should drive forward as rapidly as possible to item (iii) "to advise the Commission on the most appropriate methods of promoting full employment and economic stability."

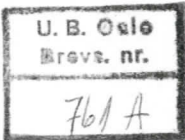
Presumably, it would be possible to take available data for such countries as Norway and the United States; subject them to the kind of analysis indicated in your documents; and reach some tentative conclusions concerning policies that would promote economic stability either in a single country or in the world as a whole.

In other words, I hope the document you sent me is simply an introduction to some concrete econometric analysis dealing with the most important economic problems now confronting the world. I sincerely believe that the document you prepared will help steer research along the right lines.

Mrs. Waugh and I both send best regards to you and Mrs. Frisch. We often speak of the delightful year we spent in Oslo and hope we will see you when you come to the United States again.

Sincerely yours,


Frederick V. Waugh



UNITED STATES DEPARTMENT OF AGRICULTURE
AGRICULTURAL MARKETING SERVICE
WASHINGTON 25, D. C.

January 17, 1958

Air Mail

Professor Ragnar Frisch
University Institute of Economics
Karl Johansgt. 47
Oslo, Norway

Dear Professor Frisch:

Thank you for sending me your very interesting report of September 1957, "Linear Dependencies and a Mechanized Form of the Multiplex Method for Linear Programming."

*Lunter
Frisch*
"In my opinion, the problem of linear dependencies is very important in many practical cases involving linear programming. For example, some of us in the Department of Agriculture have been interested in linear programming as a means of discovering the least-cost mixture of ingredients satisfying certain minimum nutritive conditions for animal feeds. We will soon publish an analysis of broiler feeds (that is, feeds for young chickens), meeting some twenty minimum requirements. In cases of this kind, the feed manufacturer can often choose between several ingredients, each having approximately the same nutritive contents. Thus, among the ingredients may be whole corn, cracked corn, and corn meal. Naturally, each of these ingredients has about the same content of productive energy, protein, calcium, etc. In other words, two or three rows of the matrix are practically identical."

It is good to know that you are working on this problem. It reminds me of the work you were doing back when I first met you in 1932. At that time you were studying the significance or non-significance of observed "scatterances." Probably I have not much to contribute to this subject. However, I thought you might be interested in the enclosed paper by Paul S. Dwyer and me, "On Errors in Matrix Inversion." This, I think, is not unrelated to the problem discussed in your report.

The general idea of your multiplex method appeals to me. My own recent attempts have been in another direction. I will have a paper in an early issue of the Journal of Farm Economics showing how our rather elaborate broiler feed problem can be analyzed by a process of simple arithmetic which used to be called "alligation."

With best regards I am

Sincerely yours,

Frederick V. Waugh
Director
Agricultural Economics Division

Enclosure

U. B. Oslo
Brevs. nr.

761A

UNITED STATES DEPARTMENT OF AGRICULTURE
ECONOMIC RESEARCH SERVICE
WASHINGTON 25, D.C.

April 12, 1962

Air Mail

Professor Ragnar Frisch
Economic Institute
University of Norway
Oslo, Norway

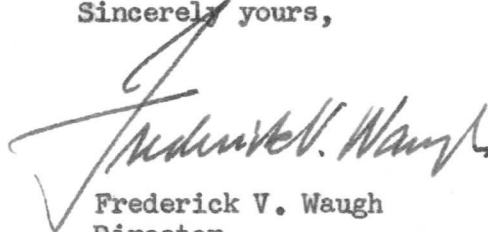
Dear Professor Frisch:

Do you know what has happened to Professor Divisia? I wrote to him recently at his old address in Paris. The letter was returned to me stamped "Unknown at this address." About a year ago Professor Divisia wrote me from southern France and indicated that his health was not good.

Mr. Koffsky and others who saw you recently in Geneva and in Paris report to me that your health and spirits are good. I am certainly happy to hear this, and have been glad to see that you are continuing to turn out important econometric papers.

With best regards.

Sincerely yours,



Frederick V. Waugh
Director
Economic & Statistical Analysis Division

Enclosure



Growth Through Agricultural Progress

Wedg. at the Dept.



UNITED STATES
DEPARTMENT OF THE INTERIOR
FISH AND WILDLIFE SERVICE
BUREAU OF COMMERCIAL FISHERIES
7338 BALTIMORE AVENUE - ROOM 209
COLLEGE PARK, MARYLAND 20740

May 19, 1969

U. B. Oslo
Brevs. nr.

761 A

Professor Ragnar Frisch
Universitet Økononiske Institutt
University of Norway
Oslo, Norway

Dear Professor Frisch:

You probably recently received a letter from Professor Stiglitz indicating that the Econometric Society is publishing some selected articles from the first thirty years of Econometrica. He wrote that they plan to include the old Frisch-Waugh Paper about partial time regressions. This suits me fine and I hope you approve. I have always been proud of my collaboration with you on this paper. It is also a pleasant memento of the very productive year I spent with you at Oslo.

I have gone over the paper rather carefully and have marked a number of suggested changes on the enclosed reprint. Will you please go over it, make final changes and send them to Professor Joseph Stiglitz, General Editor, Econometrica Reprint, Box 2125, Yale Station, New Haven, Connecticut 06520.

We have not heard from you in a long time. I hope everything goes well with you. I retired from the Department of Agriculture in 1965 but have been busy ever since with a series of studies. And the past two years I have been working with the Bureau of Commercial Fisheries, Department of the Interior on an analysis of fish prices. Dr. Virgil Norton and I have a bulletin on this subject **ready** for printing.

My children are all grown and away from home. We have ten grandchildren. We would greatly appreciate news from you. With best personal regards from Irma and me to you and Mrs. Frisch,

Sincerely yours,

Fred W.
Frederick V. Waugh

Home address:
1006 South 26th Street
Arlington, Virginia 22202

U. B. Oslo
Brevs. nr.

761 A

Letter from F. V. Waugh to R. Frisch 19/5-1969

May 19, 1969

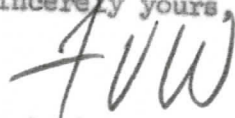
Professor Joseph E. Stiglitz
General Editor
Econometrica Reprint Series
Box 2125, Yale Station
New Haven, Connecticut 06520

Dear Professor Stiglitz:

Thank you for your letter of May 8. I am happy to know that you are planning to re-publish the old Frisch-Waugh article about "Partial Time Regressions Compared to Individual Trends."

You asked about typographical corrections. I have gone over the article carefully and have noted several changes. They are indicated on the enclosed copy. I am also sending a marked copy to Professor Frisch asking him to make final changes and to send them to you. Please accept Professor Frisch's changes as final.

Sincerely yours,



Frederick V. Waugh
Economist

Enclosure

cc: Professor Frisch