Storgt.9
Oslo, Norway

Oct.26,1931

Dr. Frederick V.Waugh
New England Research Council on Marketing
Room 425, 261 Franklin Street, Boston Mass.

Dear Dr. Waugh:
I was interested to learn about the possibility of your coming to Oslo for the purpose of study during part of the academic year 1932/33. Needless to say if you come, I will do what I can to help you, both scientifically and otherwise.

There are two terms at the University of Oslo: the fall term from the beginning of September until Christmas, and the spring term from the midle of January until the beginning of June. There is a possibility that I may, as an experiment, from the beginning of September 1932 give one of my seminars in English. This would be an advanced seminar on economic theory, discussing such things as the numerical determination of utility curves, the place of these curves in a general price theory etc. In some month I will have a book out on the subject of utility curve constructions. The book will be published in English.

While I was in the States Mr. Lunden of the School of Business Administration, University of Minnesota, told me that he might come to Oslo for study some time next year. I would suggest that you write to him about your plans. Possibly he or you may also know of somebody else in the States who would be interested. If there would be as many as three or four Americans I can say definitely that I would give the advanced ~~xxxxxx~~ theory seminar in English. Otherwise it would be in English only if the experiment proves successful also from the point of view of the Norwegian students.

My two main seminars are at present: one on the theory of interest and the other on statistics, primarily time series. I should think you, and also Lunden, wou,d be most interested in the statistics course. This is planned as a course extending over two terms. A new course would begin in September 1932. About half the time in this course is devoted to my lecturing, while the other half is devoted to discussing with the students the actual statistical work they are doing. The students in this course are divided in teams of two to four and a large part of the time is given to discussions with each separate team. If some of your problems involve time series, it would be perfectly feasible for you to arrange your work on these problems in connection with the time series course. I should think it would even be possible to organize a little team for this by appointing one of two of the Norwegian students to help you with the numerical work. The conversation with your particular team could be carried on in English.

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The semigar on interest theory is arranged this way: I lec~~tu~~ture about half the time. At the beginning of each meeting one of the students act as a reporter for what happened at the last meeting. And in addition to this the word is given free as much as possible. I think the system works very well. There are about 30 or 40 in the group, and the discussions here are, of course, carried on in Norwegian. From September 1932 the topic will be changed, probably to the general theory of prices. But the manner of discussing the problems will remain the same. Mathematics are used both in the statistics course and in the theory course. I have found it impossible to go to the bottom of the problems without this tool.

I am at present also giving a course on tensor calculus for the mathematics students and a few of the advanced statistics student, both I suppose you would be less interested in this.

If you are interested in studying the economic situation in Norway, you would want to follow some of Professor Wedervang's courses.

My advise would be that you should plan your work in suc h a way as not to devote too much energy on the Norwegian language. And I should think you would be able to get something out of your stay here even without any thorough knowledge of Norwegian. In my general theory course the mathematical symbols would convey to you the main idea. And, if necessary, I may after the meeting give you some supplementary explanations in English. If there will be as many as three or four Americans present we could perhaps even put in an extra hour on this. In the statistics course you would probably also, on account of the team organization, be able to get along fairly well without much Norwegian. And the same applies, of course, still more to the advanced theory seminar, in case this is given in English.

I think your idea of spending some time with such men as Bowley, Divisa and Amoroso, is excellent. I would add that you should by all means also meet Professor Schumpeter of Bonn if you have not already met him at Harvard. How much of your time you should devote to studying with these men will depend on the nature of your special problems. I would like to discuss this with you more thoroughly than can be done in a latter. Perhaps the best thing would be, if you come to Europe, to go to Oslo in the beginning of September, and then make up your final plan after we had discussed the matter. This applies also to your reading program. In case you decide to spend a major part of the year at Oslo you would find here a fairly good library. I have also myself quite a few books on mathematical economics.

If you decide to come, you would meet a small congenial group of teachers and students. I would, of course introduce you to the faculty members and other ecomists around. With the present exchange rate you would probably find living in Oslo about half as expensive as in New England.

I am sending a copy of this letter to Mr. Lunden, and enclose also a copy for your convenience, in case you want to get in

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touch with some other American who would be interested.

With best regards, I am

Sincerely yours

Ragnar Frisch

Storgaten 9,
O S L O. Norway.
March 31st 1932.

Dr. Frederick Waugh,
New England Research Council
on Marketing and Food Supply,
261 Franklin Street,
BOSTON, Mass.

Dear Dr. Waugh,

Tahk you for your letter of March 16th. Please accept my best congratulations on the occasion of your having been awarded the Research Fellowship of the Social Science Research Council.

I suggest that ou spend the whole of the month of August in France, England or Italy, seeing such men as Divisia and Roy of Parus, Bowley of London, Amoroso of Rome and Schumpeter of Bonn.

From about September 7th I will be in Oslo starting on my lectures and seminars for the following term. If you want to take some work with me I think the best thing would be for you to come to Oslo about this time, staying here until the beginning of December. Then, if you want to, leave for other European places. I would suggest that you arrange your work of 1933 so as to be in Paris about Easter time. Around this time I will be in Paris for about a fortnight lecturing in French at the Institut Poincaré. In this series of lectures I will develop more or less systematically the modern attempts at applying mathematics or statistics to the problems of economic theory. Then if you care to take still more work with me we may return together to Oslo after the lectures in Paris are finished.

As to the language in which my lectures in Oslo are given, I can tell you that I made an experiment this semester of giving one of my lectures in English, namely a lecture on tensor calculus given for a small group of advanced students in economics and mathematical statistics. The experiment proved very successful. As a matter of fact the students understand every bit of what I am saying, but of course they have some difficulty in taking part in the discussion in English. I am certain that I shall repeat this experiment perhaps on an enlarged scale from the beginning of the fall term in September. In addition to this the seminar work with the small groups in statistics can of course easily be arranged so that the work in the group (or groups) where your work is going on is done in English.

I notice that you are bringing along your wife and two young children and that you want to put your six year old girl into school in Oslo. I do not know about the possibilities in this regard, but my secretary has promised to find out about it and write you directly.

I have had a letter from Mr. Lunden telling me that he had to defer his going to Europe till next year in part because of financial difficulties and in part because he still had some work pending on his doctor's thesis in Minneapolis.

Sincerely yours,

Ragnar Frisch.

RF/NB

Storgaten 9,
O S L O.

May 24th 1932.

Dr. Frederick Waugh,
New England Research Council on Marketing & Food Supply,
261, Franklin Street,
BOSTON, Mass.

My dear Dr. Waugh.

This is to inform you that I shall arrive in New York on June 6th for a three weeks' stay in the States. Amongst other things, I shall attend the joint meeting of the Econometric Society and the American Statistical Association to be held in Syracuse, N.Y. on June 24th. In case you want to see me to talk over matters regarding your coming to Oslo this fall you can reach me c/o Professor Irving Fisher, 460 Prospect Street, New Haven, Conn.

Sincerely yours,

RF/NB

Ragnar Frisch.

Storgaten 9,
O S L O.
July 12th 1932.

Dr. Frederick V. Waugh,
261 Franklin Street,
BOSTON, Mass.

Dear Dr. Waugh,

As I promised you in Syracuse I am writing to give you some information about the facilities for studying in Oslo.

At the Economic Institute you will meet the following people:

Mr. Einarsen, the ecomist engaged in my work on time series. He will be out of town the first 3 weeks of August, but after that time you will probably find it interesting to let him explain to you some of the work going on at the Institute.

Miss Sæland, who is doing statistical work on time series. She is also a student of economics.

Miss Bryn-Henrichsen and Miss Karin Sohlberg, who are computors.

Occasionally you may perhaps meet Mr. Alexander, the mathematician in charge of the time series work.

Since Mr. Einarsen will not be present when you arrive, I have made arrangements with Mr. Kierulf, Thos. Heftyesgt.13 (telephone 45349), who will help you to find your way around town and also help you to find a place to live in, if you need any assistance in this respect. Mr, Kierulf will be busy at his office up to 4 or 5 o'clock, but after that time will be able to assist you.

At the University Library you will find it convenient to ask at the desk in the main reading-room to have a card authorising you to use the seats marked "reservert til videnskabel bruk". I think there will be no difficulty in obtaining such a card by presenting your credentials. The seat in the main reading-room will probably be most convenient for you if you expect to use reference books or to have books brought to your place. Downstairs Mr. Kierulf has a seat in the "Musiksamlingen" where he is doing some research work for me. Mr, Kierulf will show you the place and will be willing to let you have the key to this room. If you decide to use this place, you and Kierulf should go to the office of the Library and present the enclosed letter to the Chief Librarian.

I also enclose a general letter of introduction for you.

Good luck to you.

I shall be back in Oslo in the beginning of September.

Sincerely yours,

RF/NB

Ragnar Frisch.

Ad brev fra R. Frisch til Frederick V. Waugh 12.7.1932

Storgaten 9,

O S L O.

July 12th 1932.

Mr. Munthe,
Chief Librarian of the University Library,
O S L O.

My dear Mr. Munthe,

As you will recall, Mr. Kierulf has at present a key to the Musiksamlingen, where he has a seat doing some research for me.

I should be very much appreciate it if you would kindly consent to temporarily transfer this key to Dr. Frederick V. Waugh, who holds a fellowship from the American Social Science Research Council.

Thanking you in advance,

I remain,

With best personal regards,

RF/NB

Ragnar Frisch.

Ad brev fra R. Frisch til Frederick V. Waugh 12.7.1932

TO WHOM IT MAY CONCERN.

This is to introduce Dr. Frederick V. Waugh, holding a fellowship from the American Social Science Research Council.

Dr. Waugh expects to do research work in mathematical economics and statistics at the University of Oslo during the fall semester of 1932.

I shall highly appreciate any facility extended to Dr. Waugh during his stay in Oslo.

Ragnar Frisch,
Professor of Economics in the University of Oslo.

July 12th 1932.

11th October 1933.

Mr. Frederick V. Waugh,
U.S. Department of Agriculture,
Room 424,
261, Franklin Street,
Boston, Mass.

My dear Dr. Waugh,

Thank you for yours of September 24. It was good to hear from you and to hear that you are getting along with so much success. I wish you all possible good luck in your further work.

Dr. Belz and I have been talking about copying for you the ~~kind of~~ data for which you ask, but the pressure of work has been very heavy and we have not yet been able to do it. On looking into the matter we have found that it is not simply a matter of letting an assistant copy the figures. Some discrimination has to be made, so either Mr. Belz or I will have to supervise the work.

There is also another reason why it may be an advantage to postpone somewhat the copying of the material: we are at present experimenting with a new approach to the problem of Scatter. Possibly this may lead to more definite conclusions about the significance of the various methods of Scatter, and if more definite information is obtained this way, it would probably not be necessary to ~~keep some~~ copy all of the old results.

On the whole much work has been done on your material and we all hope that something worth while will come out of it. If you have some new and better data we would be glad to receive them in order to see whether they check or not with the original data.

Mr. Belz has also been busy carrying utility measurements through along other lines using budget data and the method of translation. The comparative analysis which will grow out of this should be highly interesting.

As the work you, Belz and I have done on this problem is so interwoven, Belz and I have discussed the possibility of working out a comparative study in joint authorship by you, Belz and me. The title may perhaps be something like "A comparative study of money flexibility in Norway, Sweden, France and the United States." If we succeed in working out a really worth while study of this sort, I should be very pleased if it could be published as a contribution from the University Institute of Economics in Oslo. Please drop us a few words telling us your reaction to this.

With best wishes, Cordially Yours,

Ragnar Frisch.

Storgaten 9,
OSLO.

16th January 1933.

Dear Dr. Waugh,

Thank you for your letters of the 11th and 13th instant.

I regret to say that we have not beenable to do much work on correlation determinants as we have had the Christmas vacation and later some of the staff have been ill. However, we hope to be able to take the work up again soon.

I also regret to say that I have not yet attended to making the final correct copies of our joint paper, but I shall try to do in the near future. I shall insert references to the works you mention.

Thank you and Mrs. Waugh ever so much for the sweet Christmas greetings. Please accept our best wishes for 1933.

I am so glad that you enjoyed your meetings with Tinbergen and Schneider.

Best regards,

Sincerely Yours,

Dr. Frederick Waugh,
c/o Professor Luigi Amoroso,
Universita di Roma,
ROMA.

18th November 1933.

Mr. Frederick V. Waugh,
410, Greene Avenue,
Aurora Hills,
Alexandria, Virginia.

Dear Waugh,

Thank you for yours of October 28th.

I think it would be excellent if you on your side could try to carry the utility analysis a little further by using better data,& thus checking the results you obtained in Oslo. Belz is also continuing his work and we are experimenting with certain refinements on the method of translation and also comparing the results of the translation method by those obtained by the time series approach. One thing which we have done recently is to add price itself in the case of meat. We thought that possibly this would bring meat (which showed a lower flexibility than the rest) up to the level given by the other commodities. This was not the case. Actually the flexibility was lowered by taking into account price itself. We are now doing the same in the case of butter and are very anxious to see what the result will be. Belz has also drawn Norwegian budget data into the analysis.

If all these investigations were collected, digested and presented in a nice little volume, I think it would be interesting.

Sincerely and Cordially Yours,

Ragnar Frisch.

June 6th 1934.

Dr. F. Waugh,
Senior Agricultural Economist,
Division of Statistical and Historical Research,
Bureau of Agricultural Economics,
WASHINGTON, D.C.

Dear Dr. Waugh,

Thank you for yours of April 26th. I was tremendously interested to see that you had followed up further the utility analysis. You may like to hear that I have now worked out much better methods of confluence analysis than I had when you were here. As a matter of fact the last month or so I have been exceedingly occupied with completing the MS, which has now grown into a little book on the subject. It will appear soon as publication No.5 from our Institute. I shall be glad to send you a complimentary copy. As examples treated in the book I have taken amongst others meat and butter in the data we completed while you were here. You will be interested to know that, by taking price into consideration, we finally found that both meat and butter agree, giving a money flexibility of something between 0.9 and 1.0.

I have not yet had time to read your paper thoroughly, but as soon as I can do so you shall hear from me again.

Thank you also for yours of May 15th. I have to-day written to Dr. Marschak, Colin Clark, of Oxford and to the Secretary to the Beveridge Committee asking them to pass on to you whatever suggestions they may have. I have sent them copies of your letter.

Cordially Yours,

Ragnar Frisch.

July 5th 1934.

Dr. Frederick V. Waugh,
Senior Agricultural Economist,
Division of Statistical ahd Historical Research,
Bureau of Agricultural Economics,
WASHINGTON, D.C.

My dear Waugh,

I am wondering how your survey paper for "Econometrica" is getting along. At your request I communicated immediately with various people asking them to send you information. Some time ago I had a letter from the Private Secretary to Sir William Beveridge telling me that she was collecting information and would pass it on to you (her letter of June 12th). In writing out your paper please make a specific acknowledgment to Sir William Beveridge. During my visit at the London School of Economics this Spring I had long talks with Beveridge and he was very obliging. On that occasion I made preliminary pourparler in order to obtain information from him regarding the work of his Price Committee. If there is a substantial amount of information regarding this which is now being sent to you, and if you think it appropriate, you may perhaps turn a phrase which more or less expresses also the thanks of the Editor-in-Chief for his courtesy. Of course this is only in case you think it fits in with your other formulations.

I have just completed a MS. of a book on Confluence Analysis (it will be about 200 pages). In this is included also several of the results we obtained together. Much of the interpretation is, however, different. I think I have now a much better and a more standardised technique. I hope you will enjoy it when you see it. The printing will probably be completed about November this year. It will appear as part of the Nordic Statistical Journal. It is a big relief to have the MS. off. At the same time this publication will be listed as No.5 from our Institute (as mentioned in my letter to you of June 6th).

I am sorry that I have not yet had time to work through your paper on Money Flexibility very thoroughly, but I am taking it together with other papers along for the summer vacation. You will soon hear from me in this respect.

Cordially Yours,

Ragnar Frisch.

July 24, 1934

My dear Dr. Waugh:

I have brought your ms. along for the summer vacation and have now had time to go through it carefully. I have found it exceedingly interesting and would be glad to accept something along these lines for Econometrica. Your ms. is enclosed. I have made pencil mark suggestions here and there, one paragraph I have crossed out because I have the feeling that this omission would improve the general character of the paper. When you have it in shape for publication please send it on to me together with charts.

It has recently been decided that I shall make a trip to the States. I will arrive in New York on ~~J~~ Aug. 9, going straight to Colorado Springs and staying there about ~~first~~ two weeks. On my way back to N.Y. I may stop over in Washington D.C. on August 24. You may reach me in this interval of time c/o The Cowles Commission, Mining Exchange Bldg., Colorado Springs.

Mrs. Frisch sends her best regards and many thanks

Yours [illegible]

August 13, 1934

Mr. Frederick V. Waugh
Senior Agricultural Economist
Division of Statistical & Historical Research
United States Department of Agriculture
Washington, D.C.

My dear Waugh:

Thank you for your letter of August 8.

I am glad that you are to complete your manuscript on Money Utility and submit it for publication in ECONOMETRICA.

On my way East I shall probably stop over in Washington on August 25 or 26 (the date first given as August 24 will probably be changed).

I do hope that you will go on as soon as possible with the survey on statistical source material, which you did not get time to finish for the October issue. Please remember the things I said in a previous letter regarding acknowledgment to be given to the Beveridge Committee. Please complete this manuscript and pass it on to me as soon as possible.

Cordially yours,

RF:GD

Ragnar Frisch

Slemdalsveien 98,
OSLO. Norway.
October 24th 1934.

Dr. Fred. V. Waugh.
State Department of Agriculture,
WASHINGTON, D.C.

My dear Waugh,

Thank you for yours of September 4th, as well as for your manuscript on money utility in the United States. Thank you very much for submitting this to "Econometrica". It is accepted for publication. You will in time receive galley direct from the printer. I am afraid however that some time will elapse before it can appear. At the present moment there is a great pressure of material awaiting publication. I have taken the liberty of omitting your too grateful acknowledgement to me in the beginning of your paper. I think the paper will carry more weight as an <u>independent</u> check on the method if the connection is not rubbed in in this way. Furthermore it does not give a true picture of the situation. You have really worked on this independently and during your stay in the States.

With best wishes to yourself and Mrs. Waugh,

Your sincere friend,

(RAGNAR FRISCH)

11th April 1935.

Dr. Frederick Waugh,
410, Greene Avenue,
Aurora Hills,
ALEXANDRIA, VIRGINIA.

My dear Dr. Waugh,

Thank you for yours of March 20th.

I think that very few statisticians use the rule of ~~the~~ regarding ~~to consider~~ a regression coefficient as non-significant on account of the mere fact that this coefficient occurs in an equation where some other coefficient has a standard error more than, say, three times the standard error on this coefficient. In most cases I believe that each coefficient is considered separately. By going through the text books I think one will also find evidence that the technique of standard errors are as a rule developed with a view to judging each individual coefficient.

But, even though one wauld adopt the rule of disregarding a whole equation by the mere fact that one of its coefficients had a large standard error, it seems to me that the whole procedure is logicially very untenable. Indeed, the technique of testing "significance" in this case amounts literally to: First drawing certain numbers out of a hat and then texting 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. The probability of getting a complete set of "significant" coefficients would decrease as the number of variables increase. It would even be possible to calculate the probability of getting a completely "significant" set. Since this probability decreases with an increasing number of variables one would of course claim that no great risk attaches to this procedure, because there is a very small probability of a positive conclusion; but this result is rather an accidental one. It does not seem to me that one can claim that such a procedure penetrates to the essence of the problem. A criterion of real significance ought to test the coefficients on their own merits, not depend on the randomness of their standard errors.

I hope Mrs. Waugh and the children are well.

Best personal regards.

Cordially Yours,

Ragnar Frisch.

18th September 1935.

Dr. Frederick V. Waugh,
The Division of Marketing Research,
Bureau of Agricultural Economics,
WASHINGTON, D.C.

Dear Dr. Waugh,

Thank you for yours of July 3rd with which was enclosed your paper: "A composite Regression Equation." I venture to offer the following remarks in this connection.

It is quite true that the "true" regression equation may be looked upon as an average of the elementary regression equations, but the weights need not be positive quantities. As a matter of fact the weights are nothing but the cross moments between each of the variables and the shift of the regression equation, as appears from formula (9.25) in my book on Confluence Analysis. Obviously these cross moments may be either positive or negative. Therefore, in point of principle it is possible that regression equations may fall outside of the range of the elementary regression equations, although it is in practice very improbable that this will happen. Indeed, an average of several quantities will fall between the highest and lowest, unless there are one or more extreme negative weights.

Thus the mere fact that you have in a single instance obtained a result outside the range of the elementary regressions is in itself not illogical, bht for other reasons Ithink your result is unsound. Your formulation of the regression problem as a problem in minimising the sum square of the whole left member of the regression equation when written in the symmetric form $a1x1 + a2x2 + \ldots anxn = 0$ is all right. Indeed, practically any least square minimalisation process can be formulated in this way (Cf. page 68 of "Confluence Analysis."). The whole thing resides just in formulating the side condition that will prevent all the a's from vanishing. To do this by imposing the condition that the direct sum of the a's shall be equal to unity is fundamentally unsound. As a matter of fact this only means that one imposes the condition that the normal of the regression plane shall lie in the plane that does not go through originand whose own normal is "directed at 45^o with axis", which simply means that all its direction cosines shall be unity. There does not seem to be any logical reason for selecting such a principle: it is purely artificial. Indeed, it is more than that, because there is one important case which is actually excluded, viz. the case where the "true" regression normal actually lies in a plane of the above sort that does go through origin, in other words, the case where the "true" regression coefficients satisfy an equation of the form $a_1+a_2+\ldots a_n = 0$. Of course such a case is quite conceivable and if this should be the case, the technique you suggest must necessarily lead to a wrong result, because such values of the coefficients cannot be produced by your method, and in cases that come close to this case the values of the regression coefficients will be very sensitive to the erratic elements in the material. I think that this is the explanation of your curious numerical results.

As a matter of fact, if I remember correctly, I tried myself in some of my earlier attempts (way back in 1921 or 1922) a regression equation of the sort you now have tried, but I disregarded it for the above reasons.

If a composite regression equation is wanted and one does not know anything a priori of the nature of the variables (such as the size of the ~~desired~~ disturbing intensities, or something of that sort), the most plausible mean regression equation is the diagonal mean regression equation. That is to say the signs of the regression coefficients are determined by the whole appearance of the adjoined correlation matrix and the size of the coefficients are determined as the square roots of the corresponding scatterances. (See (10.6) in "Confluence Analysis". This is the one I always use if there is no particular reason to select any other.

With all best wishes for yourself and the family.

Cordially Yours,

Ragnar Frisch.

4th January 1936.

Dr. Frederick V. Waugh,
410, Greene Avenue,
Aurora Hills,
Alexandria,
Virginia.

My dear Waugh,

I have just received the December 1935 issue of the Journal of the American Statistical Association and have read with much interest your paper. There are some points, however, to which I must take exception, if I have understood them correctly. Before making any public statement, I should like to get quite clear about your meaning. I therefore hope that you can find time to answer the present letter somewhat fully.

There are three questions I should like to take up:

1) The amount of work involved in the computation.

2) The nature of the significance criterion provided by your method.

3) The type of analysis which is unavoidable if reliable criteria of confluency shall be obtained.

I shall take these questions up in turn.

1) <u>THE AMOUNT OF WORK INVOLVED IN THE COMPUTATION.</u>

When the original gross correlation coefficients are computed there are three kinds of determinant work that can be envisaged.

a) Compute the value of the complete correlation determinant. This is essentially a job which, in the amount of work involved, is similar to making one solution of the normal equations. The Doclittle, Ezekiel and your methods only seem to be minor variations of the original Gaussian method. The essential characteristics of all these methods is the reduction of the problem by one variable at a time, and the further fact that divisions are used in each step of the computation. I have found it undesirable to perform divisions in the course of the work because it may happen that some of the values by which one is led to divide become very small. And if they do, the work cannot be continued according to the same mechanical rules, but must either be interrupted or an extraordinary number of decimal placed carried, or some other special device used. I therefore prefer to use methods that only involve multiplications and additions. Such a method is entirely possible. I think you will remember [illegible] Institute

our method of "one way determinant computation". Here it does not matter at all whether some of the quantities become small, because a small quantity never does any harm as a factor or an additive term. As a by-produce in this "one way determinant computation" we get the minors corresponding to one row in the correlation determinant, that is to say, we get the coefficients of one of the elementary regression equations. In other words, this is equivalent to one solution of the normal equations. The method we use for this has, as I say, the advantage that divisions are not used (until at the very end when we pass from the homogeneous to the inhomogeneous form of the elementary regression equation considered), but other wise it has no decided advantage over the Doolittle, Ezekiel, Waugh or Gaussian methods. If ~~a~~ some difference in the time spent is involved, I think however that ~~our~~ my method of "one way determinant computation" will beat the others. You remember perhaps the time experiments we made, particularly the one where you found the solution extremely rapidly, using my method and an electric baby Monroe.

b) Adjunction of the correlation matrix. This amounts to finding all the elements $\hat{r}_{ij}$ in the adjoint correlation matrix. Apart from the division by the value R of the correlation matrix, these elements are the same as your quantities P_{ij}. I am not sure whether I follow exactly the way in which you determine these coefficients. It seems to me that the work involved amounts to performing n independent Doolittle solutions. Is this correct? Or if this is not correct, what is the amount of work involved in order to find all your coefficients P_{ij}? The amount of work involved in making a complete adjunction by my method (not using divisions at any stage) is about 2 1/2 times the work involved in making "one way determinant computation". In other words, it amounts to about 2 1/2 times one Doolittle solution. The multiplicator 2 1/2 is practically independent of the number of variables.

The amount of work involved in passing from P_{ij} to the other parameters needed is of course approximately the same as the amount of work involved in passing from $\hat{r}_{ij}$ to the parameters needed. (Incidentally, is there a misprint in your formula 9? Should it be $P_{ii}P_{jj}$ instead of P_{ii}? Also, should the sign be + ?)

c) A complete tilling means, as you know, to find the adjoint matrices in all sub-sets. To do this by my method (which does not involve divisions at any stage) takes a little over twice as long as to make the adjunction of the complete correlation determinant And this multiplicator, too, is independent of the number of variables. Thus the increase in the amount of work with the number of variables is proportional to the increase that takes place in the work involved

4/1-1936

in a single Doolittle solution. The time involved in a complete tilling is given in Table (15.13) of "Confluence Analysis...". I am not sure that, in your paper in the Journal of the American Statistical Association, you have made a just comparison of the amount of work involved in your method and in the complete tilling. Your method - if I have understood it correctly - is as follows. First compute the adjoint correlation matrix, or rather the <u>reciprocal</u> correlation matrix P_{ij} according to your instructions, page 698. Then, if one of the equations is non-significant (according to the criterion discussed below), make a re-computation, leaving out the variable (or variables) that have turned out non-significant. Even considering only the first step in this instruction, I doubt whether there is an enormous saving of work as compared with the complete tilling. My impression is that the amount of work is only slightly more than twice the amount of work according to the instructions you give. This belief is strengthened by the fact that in the introduction you say that the amount of work in my method becomes almost prohibitive when 8 or 10 variables are involved, and then you proceed to give an example of your method only in 7 variables. This does not seem to be just. If it is really true that the amount of work in say an 8 variable problem is almost prohibitive according to my method, but not prohibitive according to your method, why did you not try an 8 variable problem? Would it be possible for you to give me the exact time it has taken you (or your assistants) to compute the table of the P_{ij} in the example given in Table II, page 696? I mean ~~of course~~ the number of man-hours, including the time needed for all checking, re-computations, occasional rests, etc. In other words, the entire time used in passing from the 7-rowed table of the original gross correlation coefficients to the 7-rowed table P_{ij}, page 696, as the latter table appears in its correct and checked form. If you are able to give me exact information about this, I should like to get from you the original gross correlations and perform on them a complete tilling, comparing the time used with the one you give. If it is true, as I suspect, that the time used in a complete tilling is only between 2 and 2 1/2 times the one needed to get P_{ij} by your method, it seems to me that in practically all cases it would be advisable to do the complete tilling, not simply compute the P_{ij} table.

2. THE NATURE OF THE SIGNIFICANCE CRITERION PROVIDED BY YOUR METHOD.

I now come to the nature of the significance criterion provided by your method. May be I am wrong, but it seems to me that your criterion is not based on sound principles. If I have understood your criterion correctly (see (7) page 700 in the article quoted), it is one that will lose its meaning exactly in those cases where there exist a multiply confluent situation. I do not mean to say that your criterion will <u>frequently</u> lead to the acceptance of a wrong equation, but this <u>may</u> happen, and it is, so to speak, only by pure chance that it does not. More precisely, if we have a multiply confluent situation, your criterion is automatically transformed to a criterion equivalent to deciding the "significance" of your regression coefficients by throwing dice, and fortunately the rules prescribed by your method are such that it is rather improbable that the dice will answer yes if the equation is wrong. Let me explain. Suppose that we introduce the rule of testing the significance of a single regression coefficient by throwing one die and reject the regression coefficient whenever the die shows 3 spots or more. The probability of rejecting a single coefficient would then be one-half. Further, suppose that we introduce a rule of testing the significance of a complete equation involving n coefficients by throwing simultaneously n dice and rejecting the whole equation whenever at least <u>one die</u> shows 3 spots or more. Of course the probability of accepting an equation would then decrease rapidly as the number of variables increase. Therefore this rule would, if the number of variables is large, not imply any <u>large</u> probability of accepting the equation. But the probability does exist and at any rate you will probably agree with me that this is not a very logical way of testing the significance of the equation. But this sort of testing is just to what your method amounts in the case of multiply confluency. Indeed, in this case each of the standard errors of the regression coefficients just becomes numbers comparable to those determined by dice throwing. Even if we admit that there is not a very great chance of accepting a wrong equation by following your rule, the rule becomes definitely <u>misleading</u> if used as a criterion of <u>which</u> variable (or variables) to eliminate from the equation. I should like to mention two points in this connection.

In the first place in a multiply confluent situation the standard errors of the regression coefficients are determined by pure chance (Cf. the analogy to dice throwing). It is, therefore, a pure chance phenomenon <u>which one</u> (or which ones) of the regression coefficients will get the danger sign attached to it by standard errors. Take for instance my constructed example, Table (33.6) page 190. Consider the first elementary regression. The standard error of the regression coefficient No.2 is of the same order of magnitude as the regression coefficient itself, while the two other regression coefficients appear significant. According to your rule this should be interpreted to mean th[at] the variable No.2 should be eliminated. This, however,

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In this case we know indeed, from the way in which the example is constructed ~~in~~ that any set of three variables, ~~that~~ contained in the big set of four variables is an admissible - and indeed a very good - set. For instance the set (1,2,3) that contains 2 is just as good a set as the set (1,3,4) that would be picked out by your rule. Therefore I am not at all convinced that the variables 6 and 7 are the ones that should be eliminated in the example you treat in the article of the Journal of American Statistical Association. It may quite well be that the elimination should be done in some other way. This would be brought out by a complete tilling.

The second point I want to mention in this connection is that your rule does not give consistent results when applied to different elementary regressions in the same big set. Take for instance again my constructed example Table (33.6). In this example you will see that, following my rule, we get the conclusion that the variate No.1 should be rejected according to the second elementary regression, but not according to the third and fourth. The variate No.2 should be rejected according to the first, but not according to the third and fourth equation. Variate No.3 should be rejected according to the fourth, but not according to the first and second. Finally, the variate No.4 should be rejected according to the third, but not according to the first and second equation. In your article you have only computed the first elementary regression. If you had computed all the elementary regressions, I doubt whether you would have found - by your rule - consistently that it is the variates Nos. 6 & 7 that should have been eliminated.

3. THE TYPE OF ANALYSIS WHICH IS UNAVOIDABLE IF RELIABLE CRITERIA OF CONFLUENCY SHALL BE OBTAINED.

If reliable criteria of confluency shall be obtained, I think it is necessary in some way or another to compare the situation existing in the big set with the situations in the various sub-sets. I believe that this is the only way by which it can be effectively decided whether the big set is multiply co-linear or not. It is not sufficient - as you and most statisticians do - to try to squeeze such information out by considering only the big set in its entirety. A systematic comparison must be made between the big set and its several sub-sets by my method or by some other method. If this is the situation, it appears to be unavoidable to compute regression coefficients also in a number of the sub-sets. In other words, it appears necessary to determine just the information furnished by the complete tilling. Conceivably an important amount of information may be obtained by considering only some, not all sub-sets. But in most cases if a thorough analysis is to be made, I believe it is not worth while bothering about the selection of these sub-sets, since a complete tilling only means an addition of between 100% and 150% of the work needed in order to compute the information regarding the big set alone. And even if, for a priori reasons, some particular sub-sets should be suspicious, it will probably be useful to utilise the tilling technique, namely by carrying through the building-up process (that leads to the final adjoint matrix) through the suspicious sets.

I should be very glad if you could some time in the near future find time to give these questions your thought.

Best personal regards.

Cordially Yours,

Ragnar Frisch.

P.S. Thank you and Mrs. Waugh heartily for the sweet picture of the children. Mrs. Frisch and I like it very much.

(4)

Febr. 11th 1936

Mr. Frederick V. W a u g h,

In Charge,

Division of Marketing Research,

Bureau of Agricultural Economics,

Washington D. C.

My dear Waugh,

Thank you for your letter of January 29th. I am sorry if my letter gave you the impression that I considered your paper in The Journal of the American Statistical Association December 19th 1934 as a criticism of my confluence method. I did not consider it that way, and even if it had been a criticism, that would of course have been quite all right. I am concerned with one thing only : To get at the bottom of the matter and find the method that is the soundest, the most effective and the simplest in computation, whether that method is to be called yours or mine or anybody else' . Since in your letter you only consider the question of the amount of work involved, I shall at present also confine myself to these questions. I want to mention however that the two other problems I raised in my letter to you of Jan. 4th , are more farreaching. I wish you could find time some time in the future, to go into them too.

You compare the time involved in computing the reciprocal matrix by your method and by my tilling process. If you do that you will of course find that the tilling process takes more time, but this is not the comparison to make. Perhaps I did not explain the matter sufficiently clearly in

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my last letter, but the fact is that if I only want to compute the adjoint (or the reciprocal) of a given matrix, I never use the tilling process. In this case I use a branching process, which is a further elaboration of the "one way determinant computation".

Even using this method, which is considerably shorter than the complete tilling, I think I shall not be able to beat the time data mentioned in your letter. I think that if you can compute - including all checks - the reciprocal of a ~~time road~~ 10 rowed matrix with 6 decimal ~~process~~ places in about 5 hours, that is a remarkable performance. I am going to look further into this matter.

One fact remains however, which troubles me considerably, and that is the difficulty that may occur when small minors are present. I have myself made considerable experimental work along the lines of the Gaussian methods (compare f.i my ~~minor~~ mimeographed paper "The Optimum Regression", University of Minnetsota, May 19th 1934; I am asking my secretary to send you a copy if one is still available). I have always found that in order to treat the perfectly general case, in which I am interested, difficulties arise when some of the minors become small. I think Henry Schultz of Chicago also said to me once that he has experienced the same difficulty.

On page 2 of your letter you mention the potato data. This is really not a good example. To have a very clear case you should rather look into a constructive example to begin with, f.i. the one I gave in my confluence book.

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III

On page 6 you refer again to the potato data. The fact that the standard errors indicate that the set 1245 is admissible, is I think a very strong argument <u>against</u> the use of the standard errors for this purpose.

I am exceedingly interested in what you say about the possibility of computing all the scatterances by your method. Please give more information about this. As you will see from my book I have a method of determining all scatterances that is much simpler than the tilling. Actually this method is at present being improved upon.

With kindest regards to you all,

cordially yours

Ragnar Frisch

Professor Ragnar Frisch
Slemdalsveien 98,
pr . Oslo, Norway

March 30th 1936

Dr. Frederick V. W a u g h,

In Charge, Division of Marketing Research,
United States Department of Agriculture, Bureau of
Agricultural Economics, Washington D. C.

Dear Dr. Frederick V.Waugh,

Thank you for yours of March 11th with which was enclosed photostatic sheets of the pages describing your method of computing scatterances and adjoint correlation matrices.

The methods were perfectly clear from the description given in these sheets. I also think that the notation you have used is excellent. I won't try to improve on it as you suggest.

Your method of computing all the scatterances seems all right. I think however that it can be considerably reduced. I have looked into the possibility of using the Doolittle Waugh method (the method you explained in the paper in the Journal of the American Statistical Association) for a systematic computation of all the scatterances. I am sure the work will be considerably less than the one explained in your sheet nr. 1. But the trouble is the method is now so short that I do not get a continuous check. As soon as I have worked this out to a standard process, I shall let you know.

The method suggested in sheets 2 and 3 of going <u>down</u> from the big set to the adjoints in the subset is excellent I think. If the number of variabbs is very great, this way of obtaining the adjoints in all the $(n-1)$ dimensional subsets will undoubtedly be shorter than the tilling process. Combining the tilling process and the one you now suggest we actually have a method of attacking the problem both from "below" and from above.

Best regards to yourself, Mrs. Waugh and the children,
Cordially yours
Ragnar Frisch

September 19th 1936

Mr. Frederick V. Waugh,
In Charge,
Division of Marketing Research,
United States Department of Agriculture,
Bureau of Agricultural Economics,
W a s h i n g t o n , D.C.

My dear Waugh,

Thank you for your letter of August 29th with which was enclosed carbon of your manuscript: "The Complete Analysis of Regression Systems in Several Variables".

I have read your manuscript with extreme interest. Will you allow me to make a few comments in this connection. In the first place I would like to say that we have used, with excellent results, your lay out of the work sheets and your process of computation in computing the value of symmetric determinants and adjoints along the Doolittle-Gaussian line. We consider your lay out of this problem as superior to any other we know. Allthough the main ideas are the same as in the Doolittle-Gaussian process, we consider the details you have added so important in practice that I should prefer to call the method the Doolittle-Waugh method. We have worked out a technical memorandum on this for use at the Institute. Here we have called the method "The Doolittle-Waugh Method".

Also your procedure of working back into the lower subsets by starting from the reciprocal matrix in the big set I consider valuable. In certain cases it may be easier to attack the complete confluence problem by first considering the big set of all variebles and then tentatively drop one variable at a time. If only a few variables are to be dropped, your procedure is superior to the tilling technique, but if many variables have to be dropped, my procedure will be better. I understand that we are in perfect agreement on this point. The matter can simply be stated by saying that if you want to reach a point on the lower half of a line it is easier to start from the bottom, but if you want to reach a point on the upper half, it is easier to start from the top.

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On p.9 in your ms. you say : " ... it may often be desirable to add a variable even though the regression results are less precise if it measures more nearly what we wish to know and if we have reason to believe that the results are precise enough for our purpose".

To this I agree entirely. But I do not agree when you use this as an argument against my confluence method. I really think that on this particular point you are doing injustice to my presentation. On p. 101 of my "Confluence Analysis" I say : " The new variate may be useful allthough the bunch becomes somewhat more open, namely if the general slope of the sector changes so definitely that, even taking account of the poorer precision, one gets a clear impression that the new slope is significantly different from the old". I give a figure illustrating this and elaborate more on the point. Moreover, in the numerical examples later in the book I use this kind of criterium in several instances. In other words I accept an intercoefficient in a higher set although its precision here is less than in a lower set. I therefore think that your statement on p.9 : "The addition of a variable is considered as superfluous or detrimental ..." is not correct but is doing injustice to my presentation. May I hope that you will agree to this.

With regard to the value of the usual standard errors in judging significance you say on p.11 that:" Whenever the standard error of a single regression coefficient in an equation is more than half the size of the corresponding regression coefficient ... the whole equation is of doubtful validity ...". This, I think, is <u>not</u> the way in which the standard error criterium is used in general. The standard procedure is, I think, to judge each coefficient separately. In the standard textbook "Methods of Correlation Analysis" by Ezekiel it is f.i. said on p. 368 (of the 1930 edition) :

> "The net regression coefficients may then be stated
>
> $$b_{12.34} = -0.810 \pm 0.221$$
> $$b_{13.24} = 0.180 \pm 0.030$$
> $$b_{14.23} = -0.309 \pm 0.203$$
>
> Just as in the illustrations discussed in Chapter 18, some of the net regression coefficients are much more reliable than are others. Assuming the conditions of simple sampling to be fulfilled, there is some possibility that the regression for $b_{14.23}$ is really positive instead of negative; but there is only a very slight chance that $b_{12.34}$ is really positive, and it is almost a certainty that $b_{13.24}$ is really positive, and above 0.1".

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Notice that in spite of the fact that one of the coefficients has a very large standard error , Ezekiel says : "... it is almost a certainty that $b_{13.24}$ is really positive and above 0.1". It is quite obvious that Ezekiel here takes the standard errors as a criterium of significance for each individual coefficient, not for the equation as a whole. I should be much mistaken if this is not the fundamental idea which is involved in the theory by which the standard errors are derived. I do not think it would be difficult at all to find any number of examples where the statistician has used the standard errors in the way indicated by Ezekiel. Whatever can be said for your method of handling standard errors, it seems at least to be true that it is not in conformity with the usual procedure, and it is not based on the usual principles of sampling theory.

My own interpretation of what the standard errors do in the case of multiple confluency I have already stated in a previous letter to you, but I may recapitulate it briefly. The method will then essentially come down to deciding upon the significance of each individual regression coefficient by throwing head and tail. You claim that by using your criterium on the tables (33.6) and (33.7) in my "Confluence Analysis" you get all these equations rejected, which is the correct result one should get. To this I will answer that you will in all probability obtain the same result by the head and tail method. I actually did this. I took the digits in one of the tables of regression coefficients in your last ms, letting an even number stand for "rejected", an odd number for "accepted". By doing this I got all the equations rejected. The reason is very simple : It is ofcourse very improbable that one will get all coefficients accepted. In an equation with n coefficients the probability of accepting the whole equation is $\left(\frac{1}{2}\right)^n$. In order to show that your method essentially amounts to throwing head and tail I have started a constructed experiment of 25 samples, each embracing 20 observations on 3 variables, there being in each case an exact linear relation between any two of the variables, so that a regression between the three of them will be non-sensical. I am going to apply your criterium and I predict that the result will be approximately the same as that which would be expected by throwing head and tails, namely that about 1/4 of the equations will be retained and the other 3/4 rejected. As soon as the result is available I shall forward it to you.

Cordially yours

Ragnar Frisch

8th October 1936.

Mr. Frederick V. Waugh,
410, Greene Avenue,
Aurora Hills,
Alexandria, Virginia.

Dear Waugh,

Thank you for yours of September 21st. You will by now have received my last letter regarding your method of testing the significance of equations by the regression coefficients.

I talked with Jerzy Neyman at the Meeting of the Econometric Society in Oxford. He agreed entirely with me that the standard errors on the regression coefficients do not give criteria for that property which I tried to discover by the confluence technique. Indeed, the equations which I am after are the intrinsic, "true" relations, that is the relations obtained by disregarding a part of the variates, while the ordinary regression equations of one variate on the others is an expression for an estimate made by taking everything into account. Thus, standard errors of the regression coefficients measure the precision of a different kind of equation from the one which I am after. It would indeed be possible to construct examples where structural relations have no sense (which ought to be shown by the confluence technique), but where still the ordinary regression equations would have a sense for that particular purpose for which they were invented, namely to estimate the average value of one variate which prevails in that sub-group of the material where the other variates have certain ~~different~~ given values. In such a situation - if the sample were large enough - the standard errors on the regression coefficients ought to be very small, while the structural relation that expresses the confluence situation would still be meaningless. If I get time I shall construct an example in several variates.

The experiment which I told you about in my last letter is now ready. I have not analysed it myself, but one of the assistants told me to-day that out of 75 equations in three variates six equations were such that the standard errors on the regression coefficients were everywhere less than one-third of the corresponding regression equation. The count has not yet been made by adopting your rule of one-half. I suspect strongly that when this count is made we will come very close to what I predicted, namely about one-fourth of the equations being accepted.

Best regards,

Cordially Yours,

Ragnar Frisch.

Oct. 15.

P.S. The count rising your rule of one half is now made, the number turned out as 28 equations accepted out of 75.

december 7th. 1936

Dr. Frederick V. Waugh,
In Charge,
Division of Marketing Research,
United States Department of Agriculture,
Bureau of Agricultural Economics,
W a s h i g g t o n, D.C.

Dear Waugh.

Thank you for yours of november 24th and the enclosed MS.

I like this MS much better than your earlier one.

The one you have now prepared should by all means be published as soon as possible in some such journal as the joutnal of The American Statistical Association or the annals of Mathematical Statistics.

I think I can subscribe to all you say. I may perhaps only add a remark to the effect that your estimate of the errors of observation seem to be essentially the same thing as to attribute given value, or rather limits, to the λ's in the matrix on page 54 in "Confluence Analysis". Of course if these λ's are known, everything is known because the F-matrix on page 54 is the matrix of the true variates.

Yours faithfully

Ragnar Frisch. R. F.

June 2. 1937

My dear Waugh!

Thank you for yours of May 15.

I should have been very pleased to stop over in Washington and discuss some of the topics you mention, but I am inclined to think that this will not be possible. I really ought to go straight to Colorado on my way out. Possibly there might be a chance to stop over in Washington on the way back, but this can only be decided later. I shall write to you from Colorado if the possibility materializes.

Cordially yours R F

[illegible]

[illegible]

[illegible] US. [illegible]

[illegible]

[illegible] on the [illegible]

[illegible] if
you think you can spare them.
Best [illegible]
[illegible] Yours
R. H.

May 11. 1978

My dear Dr. Wang Li:

I certainly owe you an apology for not letting you know earlier my reaction to you and Beers' new paper in "Cross Validity of an Estimate from a Multiple Regression Equation". — Please forgive me.

There are two very different ideas in the paper, one regarding the sphere of extrapolation, the other regarding the use of scattering. The first idea: the sphere of extrapolation is in my opinion very fruitful and very important. Furthermore it contains something which was not treated in "Confluence Analysis" at all. I would recommend that this part of the study be published separately without

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any reference to the scattrance approach. The two ideas are entirely different, that it would only be ~~confusion~~ confusing to treat both in the same papers. I have not read the part regarding regions of extrapolation very carefully and will therefore form no final opinion on the details, but I consider it very likely that this part contains something very valuable. It is too much purely statistical for Econometrica. I would suggest ~~the~~ the Journal of the Am. Stat. Ass. as the proper place, or the Annals of Mathem. Statistics.

In regard to the other idea: that of scattrances — which is discussed in pages 9 – 18 I think very differently. I still believe that this part contains nothing that is essentially new. And I further think that the elaborate process you develop for fixing your estimate of the

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error variances does not lead to anything which is not revealed much simpler by the methods of "Confluence Analysis". If an estimate of the error variances is available "Confluence Analysis" tells, I believe, the story. But "Confl. Anal." does not say anything about *how* to make such estimates. This is a really important and ~~a~~ new problem. If you can ~~see~~ contribute something to this it would be very important. But ~~[illegible]~~ your line of approach ~~to~~ 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~~ that are not *systematically* connected with the other variables. In other words the error in ~~each~~ a given variable *cannot* be estimated simply by inspecting the original data of *this* variable but will depend on the nature of this variables connection with the

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Wilks. —

So much for my view in general. Now let me state more specifically some particular points.

p. 1. „All statisticians agree ...". No, not all. See for instance the paper (I don't remember its title) which Mudgett and I criticised.

p. 13. The ~~[illegible]~~ inequality $\lambda_k \geq \frac{\Delta}{\Delta_{kk}}$ can't exist if the assumptions are fulfilled. See „C. A." (p. 12)

p. 16. I quite agree that you use the relation which I wrote $F(\lambda_1 \ldots \lambda_n) = 0$ (p. 66) for a different purpose. I really did not realize that you used this „proportionality" characteristic equation to ~~[illegible]~~ find out whether an unscrambled set of error variances, $\lambda_2 \ldots \lambda_n$ in your notation, (you consider ~~the~~ ~~[illegible]~~ collinearity in the set (2, 3 ... n) while I as a rule used (1 ... k), but of course is immaterial).

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taken in conjunction with the observed (r_{ij}) matrix indicates a significant deviation from collinearity.

I did not realize that your characteristic equation only has this purpose; because no such analysis is in fact needed. If a set of error variances is known, or estimated, the whole story is told already by the matrix (r_{ij}). This is the whole point of ~~the~~ the analysis in Confl. Anal. based on (8.1). Indeed

$$\begin{pmatrix} 1-\lambda_2 & r_{23} & \cdots \\ - & - & - \\ - & - & 1-\lambda_n \end{pmatrix}$$

is then (apart from a factor) the true moment matrix.

The whole problem is therefore just to see whether the system $\lambda_2 \ldots \lambda_n$ makes this matrix a positive definite and nonsingular one. If it does, one may ~~then~~ assume the scatter to be

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Significant (provided the estimate of $\lambda'_2 \ldots \lambda'_m$ is reliable).

One can also see ~~from~~ ~~that~~ immediately that your criterion $\bar{\lambda} > \lambda'_s$ (p. 15 of the MS) is identical with the criterion that $\begin{pmatrix} 1-\lambda'_2 & & \\ & \ddots & \\ & & 1-\lambda'_m \end{pmatrix}$ is positive definite. Indeed, if $\bar{\lambda} > \lambda'_s$, the matrix is constantly positive definite as $\bar{\lambda}$ increases from zero up to λ'_s. (Same sort of argument as on pp. 36-37 in Confl. Anal.). And inversely, if $\begin{pmatrix} 1-\lambda'_2 & & \\ & \ddots & \\ & & 1-\lambda'_m \end{pmatrix}$ is positive definite ~~if~~ ~~all its~~ itself and all its minors will constantly increase as the points $\lambda'_2 \ldots \lambda'_m$ move along a beam towards origin. Hence we must have $\bar{\lambda} > \lambda'_s$.

Nor can the size of $\bar{\lambda}$ as compared with λ_s tell us anything which is not essentially revealed by the size of $|1-\lambda'_2 \ldots 1-\lambda'_m|$ and its principal minors. Compare fig. 6 in C.A.

All the fuss with the ~~[illegible]~~ proportional characteristic equation is therefore superfluous

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p. 17a. ~~[illegible]~~ ~~[illegible]~~ The scatterance approach which you favor is *just* as dependent on the *linearity* assumption (= nonskewness) as the bunch technique.

[Can I keep your MS in case I may want it for further consideration?]

You will understand that I do not consider this part of your study as ready for publication. But if something valuable could be said about *methods* of estimating error variances, I would be extremely interested.

With best personal regards
cordially yours R.F.

June 6. 1935

My dear Waugh:

Thank you for your of May 27. Each time we exchange letters I see a little better what you are driving at. I probably did not read carefully enough your distinction between „errors of observation" and „disturbances". My objection now would be that if you only want an „estimate" of the price that will be produced taking the disturbances into consideration by all means just use the classical regression of price on the other variates, and use the classical standard errors. In this case there is no need for the confluence approach. The confluence approach aims at discovering the structural relationship (i.e. the relation between the systematic parts) and it is for the discovery of this structure that scatterance, bunch analysis etc. are useful. I plan to write a paper in Econometrica stressing the difference between a regression coefficient in the sense of an "expectation coefficient" and in the sense of a structural coefficient. This I believe is important from the point of view of *economic* significance.

If you plan to revise your paper in any case it may be a good plan to do so after having seen this. If you do and reduce the paper drastically, leaving out everything that is to be found elsewhere and stressing as much as possible the *economics* of the question, it may after all be suitable for Econometrica.

I am very pleased at the possibility of seeing something more of you in 1939–40; you will of course be very cordially welcome, but do you really think we have anything more to tell you that you do not know already?

With best personal regards to you, Mrs Waugh and the children,

Sincerely yours Ragnar Frisch

1 December 1947

Dear Friend:

It was good to have your letter of 27 October. I am glad that my paper in the American Economic Review appealed to you. I shall certainly do what I can to push this study further.

At the moment I am very busy with work for the United Nations, being a member of the Sub-Commission on Employment and Stability. I am also a member of the Economic and Employment Commission. They elected me Chairman of the Commission, but I have recently handed in my resignation as a member of this Commission as it will take all too much time. I do expect, however, to continue as a member of the Sub-Commission.

I had hoped that I would have an opportunity of going down to Washington and would then certainly not have missed the opportunity of seeing you. As things now look, I shall, however, not be able to do so before I sail on 12 December. One of these days I may give you a telephone call just to have a chat with you.

I am at present staying at the Aberdeen Hotel, 17 West 32nd Street, New York City.

With warm regards to you and the family, I am

Sincerely yours,

Ragnar Frisch

Mr. Frederick V. Waugh,
Economic Adviser to the President,
1006 South 26th Street,
Arlington, Va.

Professor Ragnar Frisch/bq

4 June 1969

Professor Fredrick V. Waugh
1006 South 26th Street
Arlington, Virginia 22202
U.S.A.

My dear Waugh!

It was good to have your letter of 19 May reminding me of the pleasant time we spent together in Norway and other places.

I have a daughter of 31 and a granddaughter of 6. They are both living with us in No. 98, Slemdalsveien at present. I need not tell you how happy we are to have them here.

After the death of my first wife in 1952, I remarried a girlfriend from my boyhood days and we live very happily together.

I became emeritus in 1965 but am writing on research problems more furiously than ever.

I am glad to know that all is well with you.

Warm regards

Ragnar Frisch

Dictated by professor Frisch

Bente Qvigstad

P.S. Enclosed is a copy of my letter of 4 June to professor Stiglitz.

Ad brev fra R. Frisch til F. V. Waugh 4/6 - 1969.

Professor Ragnar Frisch/bq

4 June 1969

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

Dear professor Stiglitz,

This is only to let you know that I have received from professor Fredrick V. Waugh his corrections to the Frisch-Waugh article, and that I accept gladly all the corrections suggested by Waugh.

Sincerely yours

Ragnar Frisch

Dictated by professor Frisch

Bente Qvigstad