Feb. 16. 1938.

Dear Mr. Wilfred Malenbaum

Thanks for your recent letter.

There will be no regular course going on in March in which you would be interested (x) But I think there may be things in the work of the Institute which may be worth your while to look into. At that time we shall primarily be working on time-series morphology. I shall probably be present in Oslo during most of March. You will probably also be interested to meet some of the younger men here.

Of foreigners we have at present Prof. [illegible] from the Univ. of Indiana. Dr. Mendershausen just left after a half year's stay.

I shall be looking forward with much interest to seeing you.

Sincerely yours Ragnar Frisch

(x) unless you understand Norwegian and would be able to communicate with them, to take part in regular seminar work with the students.

R. Frisch til Wilfred Malenbaum.

April 20. 1938

Dear Mr. Malenbaum,

I have read with much interest the paper by Ezekiel & Krishnaswami, your criticism and Ezekiel's Reply in the Feb. issue of The Review of Economic Studies. I jot down my impression.

In point of principle I am in full agreement with your critical remarks on the methodology of the successive residuals: 1) if a discontinuous (or more correctly: irregular) shape is admitted in one of the functions, any shape may be selected for the others and finally a perfect fit obtained. ~~If~~ You have stated this very clearly and precisely on p. 144.

2) Even if the criterion of some regularity is imposed on all the functions, the problem is indeterminate if a fairly strong correlation exists between

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two of the variables (or more generally: if a subset of the variates is strongly connected by an additive relation of the form considered). ~~[illegible]~~ For instance the functions $X(x)$ and $Y(y)$ in

$$E = X(x) + Y(y) + T'(t) \qquad (p.\ 144)$$

must be indeterminate if there is a close correlation between x and y. [illegible] (This applies even if a regularity assumption is made re T) but ~~[illegible]~~ such a correlation exists in the Edelberg data; as he himself explains. (In the period investigated y is correlated with x [illegible]. p. 154). Edelberg however does not seem to realize the importance of this. He seems to think "that even then can the functions X(x) and Y(y) be determined". Successive approximations could be found" (p. 155, 3rd line).

I think you have realized this point [illegible] [illegible] you ~~may~~ ought to have stressed it a little more. The whole

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object of my „confluence methods" is, of course, to handle these situations of multi-dependencies (however only in the case of linear relations). This was also my main point in the pamphlet „Pitfalls ...".

I do not agree to your general considerations p. 150 about „fitting". Those cases where a minimalization of residuals is not warranted are those where we have not yet taken all relevant factors into account (so that the residual minimalization will give a priori instead of a real relationship). Starting from any set of assumptions which we temporarily believe is adequate, — residuals must be minimized. What else should we do? The departures from this principle which

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You mention, are indeed nothing else than the introduction of one or more additional variables. Whenever the last is (temporarily) completed we have nothing to do but to minimize residuals. — If we are going to test the theory statistically at all.

Finally I do not agree in your considerations about „qualitative" and „quantitative" analysis. Here I am in fairly good agreement with Eddington (p. 156) It is impossible by a „qualitative" analysis to arrive at a „quantitative" result such as a constant, the shape of a curve etc. A definite assumption about the shape of a curve is, of course, already a „quantitative" item of logic.

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In no sense I think Ezekiel is right in his defense of taking centroids vertically; i.e. for narrow class intervals of the variable he chooses to consider as "dependent". He is right if and only if he interprets the relation sought as an "expectation function" (p. 153). Then, by definition, the centroids should be taken vertically. But ~~[illegible]~~ his argument here is, I think, very confused, he confounds the viewpoint of "expectation function" with a metaphysical idea of "causation" which is here ~~[illegible]~~ I believe irrelevant.

Regarding the main methodological points at issue, namely 1) and 2), I can't see that he has in his "Reply" met any of your critical remarks.

Of course the present comments only pertain to the methodology. I have not formed any opinion on the data or the reasonability of Ezekiel's results.

Yours sincerely
R.F.

[1938]

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE.
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Professor Ragnar Frisch
Vinderen
Oslo, Norway

Dear Professor Frisch,

It had been my intention, when embarking upon this year as a travelling fellow from Harvard University, to spend a term at the Institute of Economics. My work here will make it difficult to leave before early March, by which time your winter session, I am told, will be well under way.

I would be very happy to be able to come to Oslo to meet you and to attend some meetings of your seminar in statistics. If the necessary University arrangements can be made, would you let me know when in March it might be best to come?

With many thanks, I am

Yours sincerely,

Wilfred Malenbaum

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