

James Frisch

LEAGUE OF NATIONS

MR. TINBERGEN'S REPLY TO PROFESSOR FRISCH'S NOTE ON
"STATISTICAL VERSUS THEORETICAL RELATIONS IN
ECONOMIC MACRODYNAMICS."

I. Summary of Professor Frisch's note.

In sections 1 and 2 of his note Prof. Frisch gives some definitions concerning the logical structure of business cycle theories which are so clear that they need no further comment. As regards what follows, I should like to draw special attention to the important distinction he makes between two sorts of disturbances, viz. aberrations and stimuli. As regards the former it is supposed that they do not exert any influence on the further course of events, whereas the latter are assumed to have an influence.

Sections 3 and 4 introduce the notions of reducible and irreducible equations, (with respect to a set of functions) and coflux and superflux relations. This could be most easily represented to the non-mathematical reader of my two reports as a consequence of multicollinearity. It has been remarked in these reports that the determination of regression coefficients becomes impossible if some of the "explanatory" variables are linearly connected (the simplest case being proportionality between two of these variables). Frisch's remarks could be interpreted by saying that there is a systematic tendency to such linear connections as soon as business cycle research is entered upon. These linear connections are the consequence of the other equations of the system. Putting in a familiar but oversimplified form, one could say that "all business cycle curves are more or less sines or waves, and that therefore the danger of multicollinearity is permanently present."

Frisch first defines "reducibility of an equation with respect to a set of functions" irrespective of the business cycle mechanism, and only introduces the latter when speaking of coflux and superflux relations. An equation

$$p = a x + b y + c z \quad (1)$$

a reducible with respect to a certain set of (time) functions p , x , y and z , if there is a further linear relation between p , x , y and z , say

$$A x + B y + C z = 0 \quad (2)$$

by which it is possible to replace (1) by an infinite number of other relations, e.g.,

$$p = (a - 3A) x + (b - 3B) y + (c - 3C) z \quad (3)$$

In these formulae it may e.g. also happen that y is nothing but a lagged value of x , say $y = x_{-1}$. Frisch's definition is more precise in some respects but for our argument this is not so important.

On the other hand, (1) is said to be irreducible with respect to p, x, y and z if a relation of the type (2) (possibly including p) does not exist. Then (1) cannot be replaced by another equation which is independent of (1), as is (3)*

Irreducible equations with respect to such functions p, x, y, z which form the solution of the whole system of business cycle equations are called coflux relations. Reducible ones are called superflux relations. In this case (i.e. when we are speaking of the solutions of the whole system of equations) there are as many equations as there are variables, but they may of course show lags which are different from those occurring in (1).

About the functions now under consideration, Frisch remarks that they form, "of course,..... a much more special class of functions than that which satisfies each equation taken separately, and the reducibility of the equations (is) correspondingly higher."

Prof. Frisch observes further that only irreducible (i.e. coflux) relations can be determined fairly exactly, since the reducible ones are, from the regression analysis viewpoint, indeterminate.

In section 5 the notion of autonomy of a relation is brought in; it is, as far as I can see, the same as what I mean by direct relations (in contradiction to indirect relations which are obtained by one or more elimination steps). This notion is essentially an economic one. In order to "explain", or to study the consequences of policy, one has to know these autonomous relations. Of these relations Frisch already says on page 28 (section 4): autonomous relations are often superflux relations (i.e. reducible ones with respect to the solutions of the system).** As extreme examples of autonomous and non-autonomous relations Prof. Frisch gives the following two cases: the demand function for a consumers' commodity as depending on price and income and perhaps on some secondary variables will, if the coefficients can be determined with any degree of accuracy, come fairly near to being an autonomous relation. It will not be much changed by a change in monetary policy, in the organisation of production, etc. But the time relation between the Harward A, B and C curves is a pure coflux relation, with only a small degree of autonomy.

After some short remarks, in section 6, on aberrations versus stimuli, Prof. Frisch gives his conclusions about the League work in section 7. The following sentences may especially be reproduced:

* An example of a dependent equation would be
 $4p = 4ax + 4by + 4cz.$

** He says it in the form: "...frequently, some of the other equations (i.e., the reducible ones) have a higher degree of autonomy."

"All the way through his work Timbergen uses.... linear forms..... This being so it is clear that it is only coflux relations that are determined....; and the lack of agreement between these equations and those of pure theory cannot be taken as a refutation of the latter.

A case in point (viz. where very much qualification is needed J.T.) is.....the attempt on p. 26, Business Cycles, U.S.A., to get an equation for consumers' outlay. The only result of the various attempts made here is to shift from one to another amongst an infinite number of coflux equations. By a suitable choice of the variates and lag numbers introduced, one can produce practically any coefficient one likes.....The reasons for discarding some of the equations (p.26) are quite unsatisfactory; no other reasons seem to be given than the fact that the coefficients do not work out as the author likes.

In my opinion all these equations are acceptable when interpreted as what they really are: a number of coflux equations. But none of them can, I believe, be taken as an expression of the autonomous structure equation that will characterise demand."

II. Questions to Professor Frisch.

(a) Must not the degree of autonomy of a relation be determined by economic considerations? I made this the basis of my work by starting always from a relation the variables of which were based upon a a priori economic considerations. I consider this as a guarantee that if any result at all is obtained this is an autonomous relation (or a direct one, as I would call it). Would you advise a different method?

(b) Why is it probable that autonomous relations are often reducible?

(c) Is not the procedure used in establishing the equation for consumers' outlay very much the same as that used for establishing demand functions for single commodities?

(d) Which equations in the system would you accept as autonomous ones and which not?

III. Reasons why I feel surer about our relations than Prof. Frisch does.

(a) As has already been expressed to some extent in question II (a) above, I think that most, if not all, of my relations are autonomous, or almost so, because I started each section with a a priori economic considerations. Professor Frisch does not say on what grounds a relation like (1) above should be obtained, but in my opinion it makes all the difference whether a a priori economic reasoning or, rather superficial observation "without theoretical prejudice" as in the Harvard barometer case, is used.

(b) In addition I am less afraid than Prof. Frisch of the consequences of the other linear relations. like (2) above

(1) many contain extraneous variables equivalent to stimuli, (like A_u , I_{pp} , f , h);

(2) some very important relations are non-linear, viz. the n - equation (3.7) and, in principle at least, also the q - equation (3.5) and q_p - relation (3.6), where variable q stands for non-linear expression in other variables;*

(3) the freedom in the choice of lags and coefficients Prof. Frisch speaks of is considerably reduced once economic reasoning is accepted as a basis. Negative lags and lags of more than some definite period, or coefficients of one sign are often prohibited.

Taking the example chosen by Prof. Frisch, viz., the determination of consumers' outlay, I think a correct impression of what is needed is not given when Prof. Frisch says: "no other reasons (for discarding some of the equations) seem to be given than the fact that the coefficients do not become what the author would like to see". What in fact the author likes is to get economically significant relations; and therefore he required that:

(i) the marginal propensity of workers should be larger than the one for non-workers;

(ii) the influence of Pareto's coefficient α if any, should be positive;

(iii) the influence of last year's income, if any, should be positive.

All this seems to me sound discrimination. The doubt which remains is recognised (viz. that the marginal propensity to consume for workers may be anything between zero and 0.10); and (this Prof. Frisch did not know) the influence of this uncertainty on the final equation is calculated and will be shown.

(c) Finally I introduced a number of a priori coefficients in cases where that seemed possible (e.g. the price equations (3.5) and (3.6), and the proportion between the first two coefficients in equation (3.7)).

(d) It may be added that a lack of agreement between our equations and "those of pure theory" is not alleged; merely a lack of agreement with those of some theories.

* As a consequence of points (1) and (2), interest rates and banking variables are all rather clearly "deformed" by A_u , prices by q , profit income and production figures by stock exchange movements. This makes a discrimination between influences of these three groups easier than it would be in theory, since the movements of A_u , q and n are very different.

IV. Proposals for changes in the text.

Since Professor Frisch's note contains very valuable and important remarks of a systematic nature it seems worth while embodying large parts of it in the (enlarged) introduction. Apart from my own terminology Professor Frisch's should be mentioned. The reasons why I am less afraid of the dangers Prof. Frisch mentions than he is should be stated; where uncertainties exist, their influence will be estimated (this has, in fact, already been done since the reports were printed; and the results were shown at the Cambridge meeting).

(Signed) J. TINBERGEN.