Ragnar Frisch and the Probability Approach

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Ragnar Frisch was a dominating figure among the econometricians in the interwar period. The method he devised to analyze simultaneous systems of structural equations, confluence analysis, exerted a strong influence. Frisch was familiar with probability theory and used it in other parts of his work, but it played little role in confluence analysis. His student and assistant, Trygve Haavelmo, set himself the task of finding out how economic laws could be tested in a confrontation between theory and data. The quest took him to the United States in 1939 and resulted in "The Probability Approach in Econometrics" (1944).

The development of Frisch's conceptual apparatus for studying economic data took place against a backdrop of empirical studies based on economic statistics in the United States since the 1920s. While Frisch at the outset may have been attracted to the wave of empiricism in economics, he gave it short shrift after further study. His view was reinforced after he participated from the floor in a roundtable at the joint meeting in 1927 of the American Economic Association and the American Statistical Association in Washington, D.C., on "quantitative economics" (see Bjerkholt and Dupont 2010, 31–32).

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Frisch used the opportunity as appointed opponent at Johan Åkerman's doctoral defense in 1928 at Lund University to vent his view. He told the audience that the participants in the roundtable he had attended took "quantitative economics" simply to mean economic statistics, but "quantitative economics is something else and more than empirical manipulation of numerical data about economic phenomena. The busy compiler of statistical data and calculator of correlations will only give a meagre contribution to the analysis of economic phenomena, as long as he works without the design and understanding provided by a theoretical economic structure" (Frisch 1931a, 281; our translation). He reiterated his point almost epigrammatically: "*The observation material is and remains a dead mass until it is animated by a constructive theoretical speculation*" (281; our emphasis).

About fifteen years later, shortly after the publication of the "Probability Approach," Haavelmo (1950, 265) expressed his own position equally epigrammatically, like an echo of Frisch's statement but with a twist: "*A sample of observations is just a set of cold, uninteresting numbers unless we have a theory concerning the stochastic mechanism that has produced them*" (our emphasis).[1]

Frisch never embraced Haavelmo's statement, while Haavelmo would have taken Frisch's statement as an embedded part of his own assertion. The present essay aims to shed some light on Frisch's econometric view and interactions in the interval between the two statements.

Frisch's sharp and often-stated criticism of insufficient theoretical guidance in empirical work was reinforced by his penetrating analysis of how data sets depicted as points in an *N*-dimensional space "flattened" when one or more linear relations held between the variables. Frisch called it "collinearity."[2] Multiple collinearity, that is, simultaneous relationships within the same set of data, would invalidate correlation and regression analysis. Most empirical studies did not even consider the possibility of simultaneous relationships holding between the variables under study, and Frisch did not find much merit in those studies.

The approach of modeling in terms of a set of structural equations became Frisch's most important contribution to econometric analysis. He set it out in lectures at Yale University, 1930 (see Bjerkholt and Qin 2010).

1. Presented at the Cowles Commission conference in December 1945.

2. Frisch 1929 was completed during the visit to the United States in 1927–28 and was never distributed widely. Frisch consequently misstated its year of publication as 1928. Frisch 1929 provided the theoretical underpinning for the confluence analysis (Frisch 1934a). For a recent evaluation of Frisch 1929, see Leznik 2006.

The adoption of the modeling methodology may be viewed, however, not as a new idea but as the outcome of Frisch's scientific agenda, which since the mid-1920s had been driven by the search for a "new contact point between economic theory and economic life" (Frisch 1926, 302; our translation). We have argued elsewhere that Frischian epistemology is driven by the investigation of "correspondence rules" as defined in Campbell 1920 and Bridgeman 1927. The modeling approach is then thought of as the experimentation and the tool connecting "abstract measurement" and "empirical measurement" in the sense of Ellis 1968 (see Dupont-Kieffer 2003, chap. 5).[3]

At the founding of the Econometric Society, Frisch was the key figure, not only an organizational talent but fully equipped with recent advances in mathematics, statistics, and economics and full of promising ideas for developing econometrics. Frisch's approach to analyzing economic data generated by the interplay of simultaneous relationships left little role for probability. Statisticians such as Harold Hotelling and Jerzy Neyman saw little merit in Frisch's firm belief that simultaneity invalidated the applicability of probability measures to economic data. Frisch's view was not founded upon a general distrust in probability, but he was—with reference to the epigrams above—more concerned with "constructive theoretical speculations" than "stochastic mechanisms."

In section 1 we comment briefly on Frisch's archive. Section 2 sets out a sample of Frisch's exchanges with some leading members of the Econometric Society in the 1930s about the relevance and usefulness of a probability approach. Section 3 deals with how Frisch and Neyman interacted and swapped ideas in 1936, when Neyman paid a visit to the econometricians. Section 4 conveys an impression of how the research agenda at Frisch's institute—or rather, laboratory—in the 1930s was concerned with probability issues. Section 5 concludes.

1. Frisch's Archive

The prime sources on the history of econometrics have with regard to Frisch been handicapped by limitations, not in the existence of sources but in access to them. Frisch failed in some ways to get his major ideas

3. Frisch argued for "quantitative definitions of concepts" by letting "the theory get its concepts from the observational technique" as a basis for establishing "a connection between the abstract concepts of economic theory and the economic life as mirrored in the numerical data of the economic statistics." This would be a starting point for improved and more effective observations, "showing the way to pursue for the statistical approximation technique" (Frisch 1926, 302–3; our translation).

properly presented internationally, and there is no one to blame for this other than Frisch himself. One opportunity he missed was to publish a lecture series he gave in 1933 on his main econometric ideas (see Bjerkholt and Dupont-Kieffer 2009). Frisch's scientific ideas in 1930 can also be assessed from a lecture series at Yale University in 1930, finally published in 2010 (see Bjerkholt and Qin 2010; Bjerkholt and Dupont 2010).

Frisch accumulated a comprehensive archive of letters, notes, and documents of different kinds from the mid-1920s until his death. One might be tempted to say that he never threw anything away and had the secretarial help needed to file everything. There are indications that from early on he had thought that some documents, say about the founding of the Econometric Society, would be of substantial historical interest. Apart from that, he seems to have kept the archive for his own needs to retrieve documents, something he seldom may have done. He did not write retrospective surveys of his work, memoirs, or historical accounts, except on rare occasions, and then often hastily as part of a preface, or for the Nobel Prize in 1969, for which he retrieved letters from 1926. The archive is thus huge in terms of personally written documents and not particularly easy to survey.

Frisch's will, written in 1951, left all his scientific documents to the University of Oslo and instructed his trusted assistants and coworkers from the interwar period, Haavelmo and Olav Reiersøl, to publish whatever was publishable in his papers. When Frisch died in 1973, Haavelmo reluctantly took charge of the archive and after some years had the bulk of the correspondence sent to the University Library, which later sent the collection to the National Library of Norway, where it still resides.[4] The collection comprises correspondence with roughly 1,200–1,300 persons, among them the bulk of persons of distinction among econometricians and statisticians of the 1920s, 1930s, and 1940s. The correspondence includes the *Econometrica* editorial correspondence for the years Frisch was editor.[5]

Two other subcollections of the archive are particularly important as sources for the development of Frisch's ideas. These are the project files of the University Institute of Economics and Frisch's personal notes.

The University Institute of Economics, established in 1932, was run as a laboratory, with Frisch as the supreme master over a large number of

4. The University Library of the University of Oslo may be one of the very few libraries of major universities with no interest in keeping scientific archives, not even for Nobel laureates.

5. The remaining part of the Frisch archive is still residing at the Department of Economics, University of Oslo (the successor of Frisch's institute). The archive is now being cataloged and organized, but it is still undecided when and how it will be made accessible.

assistants and several mechanical and electric calculating machines (Bjerkholt 2005). The institute was set up to connect "abstract measurement" with "empirical measurement" (Dupont-Kieffer 2003). Computations entered into practically all projects, not only the empirical ones. Methodological projects were undertaken with much emphasis on numerical simulations and solutions; purely theoretical projects drew on numerical calculations to try out ideas.

At any one time a number of projects would run in parallel, with assistants shuffled from one to another. Frisch used a simple and useful way of documenting the execution of the projects. All project documents were dated and placed in folders. A project document could be a theoretical note, a computational instruction, a filled-in calculation sheet, or a graph, among other things, even external documents such as a letter received from other econometricians or an article torn out of a journal. Other documents could be minutes taken from meetings, interviews with outside experts, and so on. The underlying principle was that all pertinent information, whether inputs or outputs, should be filed.

Each folder had an inventory list, listing all project documents. This procedure maintained reasonable order in the files, even to the extent of making the projects reproducible. Making them reproducible was not without problems, however, as implicit contextual and laboratory knowledge may have been lost, just as specially designed computing equipment had been dismantled. Projects often started out as a brief note, like a sketch of an idea, by Frisch, almost like a whim. But there was usually much more to it than that: even if the initial note was short, it could reflect deep concerns. (In section 4 we look closer at one particular project.) The project documentation was mostly in Norwegian, but in some projects involving visiting scholars other languages were used.

The personal notes (for lack of a better term) were notes taken by Frisch on scientific issues but not incorporated into project files. Such notes were typically written at Frisch's home, on travels, and, not least, during the annual recess Frisch organized for himself in the mountains, in whichever country he happened to be. Frisch's activities would then be mountaineering in the day, scientific work at night. Notes would most often be written in Norwegian and seldom typewritten afterward unless they were drafts of new papers.

Frisch was an incessant note taker and would often scribble down notes from events he attended, such as one-on-one meetings, lectures, discussion sessions, doctoral dissertations, and so forth.

2. Interactions with Schumpeter, Hotelling, Waugh, and Schultz

Many of the members in the Econometric Society took an interest in Frisch's methods of confluence analysis. Among them were Joseph A. Schumpeter, Harold Hotelling, Frederick V. Waugh, and Henry Schultz. The last two visited Oslo to meet with Frisch.

Just two weeks before the organization meeting of the Econometric Society in 1930, Frisch (1930) sent Schumpeter a six-page letter in which he proposed that an economic system could be conceived as a set of structural relations, equal in number to the number of variables (x_i) and the relations specified through a set of constant parameters (a_{ij}):

$$F_1(x_1, \ldots x_n; a_{11}, a_{12} \ldots) = 0$$

$$\ldots\ldots$$

$$F_n(x_1, \ldots x_n; a_{n1}, a_{n2} \ldots) = 0.$$

Frisch noted that here "we have the curious situation that if the material at hand fulfils our assumptions it is impossible to determine these constants a_{ij} that express the nature of our assumptions, because in this case we would only have a single observation, namely, the one corresponding to the solution of the system." Frisch then posited that if the functions $F_1, F_2 \ldots$ contained another set of variables, which we may call disturbances, $\xi_1, \xi_2 \ldots \xi_m$, the set of structural relations would become

$$F_1(x_1, \ldots x_n; a_{11}, a_{12} \ldots; \xi_1, \ldots \xi_m) = 0$$

$$\ldots\ldots\ldots\ldots$$

$$F_n(x_1, \ldots x_n; a_{n1}, a_{n2} \ldots; \xi_1, \ldots \xi_m) = 0$$

with $\Omega(\xi_1, \xi_2 \ldots \xi_m)$ being the "frequency distribution" of the set $(\xi_1, \ldots \xi_m)$, and to this distribution would correspond a distribution of the x's, say $\Pi(x_1, \ldots x_n)$ which was contingent on the values of the parameters $\{a_{ij}\}$. The actual distribution of the x's could, however, be determined from empirical observations. By confronting $\Pi(x_1, \ldots x_n)$ with the actual distribution one might pose the problem of determining the $\{a_{ij}\}$ to make these two distributions as similar as possible. Frisch thus noted that in point of principle, the constants $\{a_{ij}\}$ could be determined if we knew Ω, which Frisch tongue in cheek admitted that we do not, "but we may make some more or less plausible assumptions about it." In the letter Frisch did not use the term *probability*. But one can hardly avoid the conclusion that the reasoning is strongly conducive toward an interpretation as a starting point that could have led toward a probability approach in the sense of Haavelmo.

It would have been more in line with the later picture of Frisch as someone who saw no role for probability in econometrics, if the argument presented to Schumpeter had concluded that the occurrence of disturbance terms within the equation system would provide a scatter in the observations that could allow the determination of the individual equations with appropriate methods (provided by the confluence analysis). But Frisch went one step further and suggested the idea of a probabilistic approach.

The idea he described to Schumpeter stands out from the exchanges he had later and in which he dismissed probability theory "as we know it today" as not very helpful for resolving the problem of simultaneity.

In *Confluence Analysis* Frisch (1934a) explained the problem geometrically as observations of three variables fulfilling the conditions (apart from random errors) of lying in two planes and therefore necessarily would be clustered along the common straight line of the two planes. Any attempt to determine the two planes from the given data would be futile, and any result coming out of such an attempt would be "*fictitious determinateness created by random errors*" (6). In sweeping statements he characterized a substantial part of regression and analyses in recent years as "nonsense."

Frisch's convictions about the structure of economic reality paired with "passive observations" left him in no doubt that normality or other reasonable distributions were unlikely to be fulfilled, as required by standard methods of statistical analysis. He was more concerned about the possibilities of acquiring extraneous information that could help identify the autonomous structural relations (see Bjerkholt and Dupont-Kieffer 2009, lecture 6).

It is of interest in this context to note Haavelmo's sophisticated and retrospective formulation of what *Confluence Analysis* was about:

> *Confluence Analysis* was written in part as a protest against a mechanical and uncritical use of the classical least-squares method to estimate demand functions and other economic relations. Frisch pointed out that economic data, in general, do not satisfy the conditions required to justify the use of the classical method of least-squares. It is of course true that the strict conditions of a theoretical model perhaps never are exactly fulfilled in any observational material. *And good theoretical models*

> *should be able to absorb moderate discrepancies between model and facts without the inference drawn becoming valueless or nonsensical*. (Haavelmo 1950, 258; our emphasis)

Harold Hotelling was exactly the same age as Frisch, could follow Frisch's reasoning, and had also read Frisch 1929. Frisch held Hotelling in high regard. They saw eye to eye on most issues related to promoting econometrics and had few disagreements. Hotelling was well aware of his uncontested position as the leading statistician in the United States. In 1931 Frisch expressed to Hotelling his surprise and disappointment that American statisticians did not know Frisch 1929. "I do not believe that you need to worry about most American statisticians not knowing it," Hotelling (1931) assured his colleague; "most of these gentlemen know nothing whatever of a theoretical nature." Frisch and Hotelling had an exchange at the Syracuse meeting of the Econometric Society in June 1932 (see Mayer 1933, 94–96), pinpointing their respective positions. Frisch's sweeping criticism of the use of standard errors may in Hotelling's view have left him without firm ground to stand on. This came out in a brief exchange in 1933. Frisch had read an article by Hotelling and wrote to him:

> I quite agree with you that a gap is to be filled in the analysis of significance and accuracy, but I feel that this gap cannot be filled only by the use of standard errors. . . . the use of such parameters are sometimes dangerous in giving an air of exactness to the results which may not be quite warranted. . . . Assuming normal distribution of the universe . . . is . . . a very narrow hypothesis. If we attack this more fundamental problem of the sample pattern, then we may frequently be guided more by intuition than by mechanical formulae of standard errors. Often this intuition may find a better help in graphical or other short cut methods than in mechanical formulae. (Frisch 1933)

Hotelling (1933) responded by expressing agreement in general, including reservations on standard errors as measures of accuracy, but not any further: "Free-hand methods and graphs are valuable for preliminary exploration, and for exhibition, but any valid conclusions to be drawn from them can also be obtained in a more objective fashion, with a proper use of the canons of statistical inference, which are also capable of yielding conclusions not to be deduced from graphs by visual examination."

Frederick V. Waugh of the Bureau of Agricultural Analysis may have been the one who dug himself deepest into Frisch's way of thinking.

Waugh was three years younger than Frisch and had earned a PhD from Columbia in 1929. Unlike many others, he had studied Frisch 1929 and had been fascinated by it. When he studied Frisch's notions of "scatterances" and "sub-scatterances," which were measures to express the degrees of freedom in a mass of observations, he found them so interesting that when he got the chance to go to Europe he decided to spend half the time with Frisch in Oslo.[6] Frisch was delighted, of course; the institute was in its first year, and Waugh was the first transatlantic visitor. The confluence analysis book was still almost two years away and might not have been written if it had not been for Waugh, who brought with him U.S. data, the analysis of which figures prominently in the book (and at the institute was nicknamed "Waugh's potato data").

As a practitioner of Frisch's econometric approach, Waugh became as proficient as Haavelmo and Reiersøl, but unlike them he was driven by his own empirical research needs. His interest in Frisch's work was apparent from Waugh 1935, which he followed up in a paper titled "The Complete Analysis of Regression Systems in Several Variables," which offered constructive criticism of Frisch's method and proposed improvements on computational issues. In return Frisch adopted the name "Doolittle-Waugh method" for the improvement Waugh had achieved in the Doolittle-Gauss method for calculating symmetric determinants.

In a paper presented to the Econometric Society meeting in Chicago, in December 1936, "On the Determinateness of Regression Coefficients," Waugh (1936, 1) again dealt with Frisch's contributions, which he noted as having attracted well-deserved attention among mathematicians, while "these studies have not been adopted very generally by statisticians. One possible—but, I think minor—reason for this is the extremely mathematical character of most of Frisch's publications which makes them difficult for many statistical research workers. I believe, however, there is a more basic reason for the failure of many statisticians to follow Frisch's interesting leads." Then he elaborated on the "reason for the failure," which was a failure highly intrinsic to Frisch's frame of analysis. Frisch had not developed any kinds of criteria for testing the significance of scatterances, particularly for testing whether scatterances differed from zero, which implied a linear dependence within the set of variables.

6. Frisch credited Maurice Belz for having suggested the term "scatterance" (called "scatter coefficient" in Frisch 1929), and defined as the square root of the determinant value of the matrix of correlation coefficients (Frisch 1934a, 7). The term was in frequent use in the 1930s but is long gone from lists of statistical terms.

In fact, Frisch commented on this point in 1929: "For a rigorous analysis it would be highly desirable to have an exact criterion for the significance of the observed magnitude of scatter parameters in the form of formulae for the mathematical expectation and standard deviations of these quantities, or better still, in the form of complete theoretical distributions" (96).

As Waugh (1936, 10) observed, Frisch "proceeded to develop other approaches to the problem of correlation and regression, practically abandoning the notion of scatterances. This, I believe, was unfortunate because the scatterances seem to me to be very useful, and, in fact, more useful for most practical problems than any of the methods developed in 'Confluence Analysis.'" It was a similar kind of criticism that Hotelling had raised, namely, that Frisch did not adhere sufficiently to formal and theoretically satisfying solutions of the problems he encountered, but this time coming from the practical research worker.

Henry Schultz was two years older than Frisch and had been present at the organization meeting of the Econometric Society. He was, with Hotelling, Frisch, Schumpeter, and Irving Fisher, among the few at that meeting who came to play a major role in the activities of the Econometric Society. His career was cut short by his untimely death in a car accident in 1938.

Frisch and Schultz met during Frisch's visit in 1927, and their subsequent correspondence was comprehensive. They had a lot of shared interests and also disagreements, not least on computational methods. So while Waugh could be regarded as a devoted follower of Frisch, Schultz was more of a skeptic who also criticized other parts of Frisch's work and vice versa.

Some offhand remarks are amusing. In 1930 Schultz struggled with probability: "During this quarter, I have been spending a considerable portion of my time on probability and sampling. *Probability, as you well know, is a puzzling field, one in which we don't know what we are talking about and in which we nevertheless get correct results*" (Schultz 1931; our emphasis).

Schultz was incidentally the first to submit a paper to *Econometrica*, already in March 1932.[7] Frisch refereed it, and Schultz (1932) was not

7. The paper titled "A Comparison of the Elasticities of Demand for Selected Commodities Obtained by Different Methods"—submitted even before the name of journal had been decided—was published in *Econometrica* 1.3:274–308.

equally happy about all of Frisch's suggestions, yet was able to appreciate Frisch's view:

> The suggestion which, after due deliberation, I find impossible to accept, is that I omit all coefficients of correlation and all references to them. You will please recall that I used these correlations simply as measures of the goodness of fit of the various formulas. From this point of view, I see no objection to them. In fact, I find them quite instructive. But that is not the main reason for my seeming obstinacy. Some of my coefficients of elasticity have been criticized as being of little value for the reason that they have been derived from equations which, as judged by the coefficient of correlation (simple or multiple) do not give a very good fit to the data. The implication was that had I taken sufficient pains to develop a formula which describes the data with the highest degree of probability—whatever that may mean—my elasticities of demand would have been entirely different. This paper is in part a reply to the criticism.

In 1931 Frisch received from Schultz work sheets for a study of wheat demand. Frisch responded by setting out his pet ideas but carefully avoided criticizing Schultz directly. This was somewhat unlike his usual style and may have reflected his high regard for Schultz:

> I think that your methods in this particular case have led to results that are perfectly sound, but I do not believe that this shows that the orthodox correlation methods, computation of regression coefficients and their standard errors, are safe in general. As a matter of fact if you have a situation where the set is multiply collinear not only the regression coefficients but also their standard errors will become of the indeterminate form 0/0. And in the statistical cases that approach to this situation both the regression coefficients and their standard errors become meaningless. I should think it would not be difficult to construct cases which would give small standard errors on the regression coefficients although these coefficients have no sense. It therefore seems to me that the only safe procedure is to fall back on the computation of the scatter coefficients as a preliminary orientation regarding the cluster type, and then proceed to the computation of the other correlation parameters (if they are wanted at all) only for those subsets for which the scatter coefficient analysis has shown that the classical correlation parameters will have a meaning. (Frisch 1931b)

In 1935 Frisch sent Schultz the *Confluence Analysis* book. Schultz (1935) got annoyed over being treated with kid gloves in the accompanying letter:

> To judge from the first paragraph, in which you inform me that you are not "particularly criticizing" my work and that I have "in most cases safeguarded myself against erroneous conclusions," you are evidently under the impression that I have gone to all the trouble of studying and applying your confluence analysis only for the purpose of avoiding criticism by you. . . . No, the point at issue transcends personal pride. It is whether your method is any improvement over the standard error-graphic approach which I have been using. . . . It is, of course, true that some statisticians have drawn erroneous conclusions from correlation analysis in which the independent variables were too highly correlated with one another, and that your approach would have exhibited these high inter-correlations. But so would the least square—standard error approach. I regret that you have never deemed it advisable to make really significant, practical comparisons of the advantages and limitations of the two procedures.

Frisch (1935) proposed a computation competition:

> If you have time to spare on an experiment would it not be an idea for each of us to construct an example, say in six variables according to certain rules which we put down at the beginning but not letting the other fellow know what the unfolding capacity of the set is? Then you could analyse my data by your method and I could analyse your data by my method, and afterwards we could compare results.

But Schultz (1935) just got even more annoyed:

> You suggest that each of us construct an example according to certain rules and then analyze the data by both methods and compare results. What was my analysis of the example which you and Mudgett used if not such an experiment! True, the number of variables was only four, but in all other respects it meets the specifications. I remember how much I was surprised when I discovered that your method failed me just where I needed it most; namely, in those cases in which the standard errors were relatively large. When you have given evidence that you are willing to take honest experiments more seriously I shall be glad to cooperate with you in constructing new test cases and in getting to the bottom of this issue.

3. Frisch and Neyman

Jerzy Neyman figures in the history of econometrics mainly through the Neyman-Pearson theory of testing. This theory was not developed as part of an econometric program but was given prominence in econometrics after being embraced in Haavelmo 1944.

Neyman did not play an important role within the econometrics community, as elaborated in Aldrich 2010. He became a member of the Econometric Society in 1934 and participated in the Sixth European Econometric Society meeting at Oxford, on 25–29 September 1936 (see Qin 1993, 125–28). But that was about it. His influence within econometrics became limited.

At the Econometric Society meeting Neyman presented a paper titled “Survey of Recent Work on Correlation and Co-variation.”[8] The meeting also had an impromptu presentation by Frisch of his “ideal programme for macrodynamic studies” (see Aldrich 1989, 21, and Qin 1993, 48). In a verbal exchange between Frisch and Jakob (later, Jacob) Marschak during the discussion of Haavelmo’s presentation, Frisch elucidated the distinction between “structural” and “confluent” relations, concepts used by Haavelmo in his paper (see Aldrich 1989, 22).

Observant readers of the report from the Oxford meeting can hardly have avoided the oddity of finding that the summary of the “Neyman-Pearson theory of testing” was “prepared by professor Frisch.” The Neyman-Pearson theory of testing was certainly a most pertinent message to bring to the attention of the mostly young econometricians attending the meeting, but there was nothing about the Neyman-Pearson theory of testing paper that Neyman had brought to the meeting.[9] Neyman’s paper briefly surveyed various issues and dealt with Frisch throughout. Neyman (1936, 4–5) had clearly studied Frisch 1934a, praised it as a “remarkable

8. The complete title was “Points from the Survey of Recent Work on Correlation and Co-variation” (Neyman 1936). Neyman did not participate in any other Econometric Society meeting, apart from the huge joint statistical meeting in Washington 1947, at which he was invited by Frisch to submit his paper, published as Neyman and Scott 1948, and the only Neyman paper in *Econometrica*.

9. Volume 1 of the *Statistical Research Memoirs* had just come out. Frisch had received a copy from E. S. Pearson and acknowledged that in a letter to Pearson of 19 July 1936 and congratulated at the same time Neyman and Pearson with “the eminent contributions you are making to the important theory of testing hypotheses.” Neyman and Pearson had written about testing since 1928; in Neyman and Pearson 1936 they introduced “power function” and “unbiased critical regions” (see David 1995), which makes it likely that this was the source for Frisch’s contribution to the report.

work," and spoke of Frisch's concept of "true" regressions as "an attempt to treat empirically the machinery of observed variations." The knowledge of the "true" regressions was strictly speaking unattainable, but in certain cases, as exemplified by Frisch, "the range of indeterminateness may be negligible" (Neyman 1936, 5).

But Neyman, who also touched on the role of random causes in business cycles with reference to Frisch 1934b, marked his disagreement with Frisch's claims that standard errors of regression coefficients could not be relied on as true measures of the uncertainty of estimates or for eliminating irrelevant variables. This was a matter of importance to Frisch who, as we have shown, had disagreements over this also with Harold Hotelling and Henry Schultz. Frisch's argument, that simultaneity would cause the residuals to "cluster," invalidating the assumptions underpinning the standard errors, did not cut any ice with Neyman:

> I disagree with [Frisch's] opinion as to the inadequacy of the theory of testing hypotheses based on sampling for the elimination of variables which are irrelevant. In particular I cannot agree that even the mere comparison of regression coefficients with their standard errors is inadequate in such cases as are exhibited in his tables. . . . It is certainly a most surprising fact that, as Frisch describes it, drawing at random one number from one hat and dividing it by another similarly drawn from another hat, we may get something reasonable and useful. But the theory, supported by frequently repeated sampling experiments, shows that the results of such divisions follow a definite and known law and that they may be used so as to have a prescribed frequency of correct judgments. This is, of course, the only thing we may hope for. (Neyman 1936, 5)

After the meeting Frisch gave two lectures on problems of distribution and problems of structure at the Department of Statistics, University College London, 1 and 2 October 1936. During the meeting and up to the end of Frisch's stay, he and Neyman had a series of one-on-one sessions on statistical problems. Frisch raised the issues, and Neyman responded and explained. An unusual feature about this exchange is that the notes seem to have been passed back and forth across the table, such that they took turns at writing (see appendix 1).[10]

10. The notes comprise about twenty pages. The appendix is a transcription, lightly edited, of selected passages, as indicated by page numbers.

It started out elementary and got more intricate as they went along. From a direct reading of the appendix, it seems that Neyman stays within the classical regression model. Frisch is probing the limitations of this framework, arguing as if the classical model is merely a special case of a much larger class. Frisch keeps coming back to how the regression estimates can be determined to give good predictions. For Neyman this is not much of a problem, as he apparently took for granted that the residuals from the regression equations were independent and identically distributed. It may seem surprising that Neyman does not seem to admit that the usual variance formulas for the estimators must be modified when the residuals are not identically distributed. As Frisch posed the problem of evaluating the uncertainty of the parameter estimates, a clarification on this point ought to have been close at hand. Underlying the differences manifest in this exchange may have been deeper differences with regard to the very nature of the residuals and the appropriate statistical tools to analyze them.

It is near at hand to read into Frisch's argument that he has the simultaneity problems clearly in mind. With hindsight we know that the OLS formulas are incorrect under such assumptions. After Frisch had returned to Oslo, the exchange with Neyman continued a little longer by letter.

There is a slightly curious aftermath to this story. Neyman must also have left with some notes from the discussion. He passed to one of his students, Miss H. V. Allen, a problem raised by Frisch during the conversation and written down in the joint note. The problem was stated by Allen (1938, 60) as follows:

> Suppose it is known that the two random variables x and y have the following structure:
>
> $$x = a\xi + \alpha$$
>
> $$y = b\xi + \beta,$$
>
> where ξ, α, and β are some mutually independent random variables and a and b are certain constant coefficients, the values of which are unknown. What are the conditions under which the regression of y on x is linear?

The problem that Neyman passed on was in fact simplified a little, as the original problem had three equations and left-hand-side variables; see page 138. After Allen had worked on it, Neyman passed the problem to another student, Evelyn Fix (1949), who reformulated it slightly and worked out a more comprehensive and complete answer.[11]

4. Presumptive Analysis

One of Frisch's institute projects, Presumptive Analysis, is related to Frisch's skepticisms about standard methods, normality assumptions, and so forth. Space does not allow a more comprehensive presentation. The project, which was not particularly successful, was, briefly stated, about applying the "excess" method, as derived from the 1905 Charlier formula, which as Gram-Charlier expansions is in active use in financial market modeling and other areas along somewhat other lines than the one pursued by Frisch.

The project took place over about six months in May–November 1935. As explained above, all project documents were entered in inventory lists, called *BEREGNINGS-LISTE*, the first of which is reproduced here in facsimile (see appendix 2). As can be seen from initials in the second column, most documents are by Frisch (RF) and Haavelmo (TH). The facsimile displays 10 project documents, but on additional lists there were 131 more documents in this project. At the time the institute hosted two visitors, both statisticians of future renown, Tjalling Koopmans, working on his doctoral dissertation, and from Denmark Georg Rasch, who later became a famous name in psychometrics.

The Charlier formula aimed to show how a given distribution could be written as the normal distribution plus additional "excess" terms that are weighted products of higher moments of the given distribution and derivatives of the normal distribution. Frisch's initial note, item 1 on the list, stated the idea of the project in somewhat airy and Frischian terms. It also gave the initial instructions:

11. In 1947 C. R. Rao, having read Allen 1938, submitted a solution of Frisch's problem to *Econometrica*, drawing on suggestions by Hotelling (Rao 1947), only to be informed by a letter from Neyman that the solution was incorrect and that the problem had been solved by Fix (1949) (see Rao 1949). A few years later the problem popped up again in Laha 1956, 1957. Allen, Fix, Rao, and Laha all stated that the problem had been posed by Frisch at the Oxford Conference of the Econometric Society in September 1936, although the problem is nowhere to be found in the report from the meeting but arose as told above in the postmeeting exchange between Frisch and Neyman and, indeed, after the meeting had ended. It may well have spread wider; Kenneth Arrow reviewed, for example, Rao 1947 for *Mathematical Reviews*.

Principles for choice of optimal values for smoothing. The "excess" determined regression.

Given two variables y and x. The desired formula is $y = k \cdot x$

Criterion: $(y - k \cdot x)$ should in the future *as often as possible be as small as possible*. Do not set as a criterion that $\Sigma(y - k \cdot x)^2$ should be as small as possible, neither that $(y - k \cdot x)$ should be normally distributed.

One has to choose risk type. Some people will risk once in a while to be very wrong while on the other hand to be guaranteed to be almost always guessing close to the mark. . . . "The greatest possible luck for the greatest number of observations."

The "normality" of the difference $(y - k \cdot x)$ depends of course upon k. We thus cannot say a priori whether the distribution of $(y - k \cdot x)$ is normal or not. We produce the normality ourselves just as we produce cycles ourselves. We must choose risk type, i.e. we must choose the distribution of residuals that we fancy the most—and thereafter determine k such that the *future* distribution concurs as much as possible with the ideal. . . .

Then comes the practical task of how—on the basis of available data—we can presume the future distribution to be, if we choose a specific statistical method. Here we must distinguish between the intrinsic properties of the future distribution and those we create ourselves. If we choose the diagonal [regression], then "certainly" the future distribution will usually be skewed. I would prefer a future distribution which is more *hochgipflich*—having positive excess.

We could simply maximize the excess. This is something completely different from the least squares method. *That* method leads of course to require that the future distribution should have the smallest possible square deviation (which in all likelihood is tantamount with the greatest possible normality, i.e. excess and skewness as close to zero as possible). We shall maximize the excess instead of making it close to zero. We could formulate this as requiring the smallest possible distance between 25 and 75 percent fractiles.

Practical way of proceeding: The excess of $(y - k \cdot x)$ can be expressed by the moments of $(y - k \cdot x)$, which again can be expressed by the moments of y, the moments of x and the cross moments. The excess E can thus be determined as a function of k and the empirically determined moments.

Experimental calculations: Determine $\mathrm{x} = \mathrm{a}\zeta + \alpha\xi$ and $\mathrm{y} = \mathrm{b}\zeta + \beta\eta$; where ξ, η, ζ are three erratic independent variables, normally

> distributed . . . a = 1; b = 2; $\alpha = 0.2$ and $\beta = 0.3$. The observation series have a certain number N of observations. Calculate moments from every 10th observation. Run through 500 observations. Cumulate moments [xx], [xy], [yy], [x^2y] etc. up to 4th order.
>
> Determine within each observation set: 1) The elementary regressions; 2) the diagonal [regression]; and 3) the excess determined [regression]. (our translation and abbreviation)

Frisch was here, and in other projects where he hunted for alternative approaches, very skeptical about falling back on the least-squares method and perhaps even more about making unwarranted assumptions about normality. It can only barely be seen in Frisch's note above but runs like a streak through several projects.

Georg Rasch was put to work by Frisch and wrote a long handwritten note (item 2) deriving formulas for determining regression coefficients such that the excess is maximized.

This was how this project got initiated, and then it went on via calculations, assessment of results, revised ideas and instructions, new calculations, and so forth. As Koopmans visited in the autumn of 1935 he would naturally be briefed about ongoing activities. Frisch arranged a meeting between himself and Koopmans, asking Haavelmo to take notes, which Haavelmo did in his taciturn way, item 6 as rendered below:

> *Notes from meeting between Professor Frisch, T. Koopmans and TH, September 20, 1935*
>
> The excess method is just one special method among all the various methods that can be considered applied instead of the highly special least squares method.
>
> Koopmans means that the assumptions about distribution laws etc. must lead back to the assumption about a universe.
>
> Frisch means that we should build on the observations as they actually are and do our assumptions on the basis of "the sample as it is." We consider the variable as consisting of two parts, a systematic part and an error element. The assumptions about the error element are based on the actual example. "But I do not object to the idea of the universe, in many cases it may help. But elsewhere not, particularly in economic data of historical character, data which cannot be repeated."
>
> If we pose the problem of whether the same result will be repeated one must make the assumption about a universe.

K: Does this framework have any meaning when the sample is small? For what shall we apply our empirically determined coefficients if we do not want to consider the concept of a universe?

RF: When we from a certain set of observations have determined certain coefficients, we will naturally at the next opportunity try to determine the same coefficients again and compare the results. The true character of the universe cannot be observed.

We can determine whether our observed sample is normally distributed without employing the concept of a universe. All characteristics are applied to the sample. (our translation)

The notes said practically nothing directly about the project under consideration, but dealt with fundamental underlying principles. The positions taken by Koopmans and Frisch here are consistent with how they are depicted in the history of econometrics literature.

The project went on with Haavelmo in charge of calculations, and several assistants got involved. Frisch monitored results and gave further instructions. The project was closed in November 1935, and Haavelmo wrote the concluding report, the last document in the project folder. After filing the document, Haavelmo penciled a final note unwarrantedly on the last inventory list, perhaps reflecting his feelings after hours of wasted time:

> To be filed, as it seems to be the case that we don't get anywhere. To put it bluntly the situation is roughly as follows: When we find a method which seems logical and adequate, it does not produce better results than the usual least squares methods. When we find a method producing surprisingly good results in a given case, it is most likely to be wrong. 28 November 1935. (our translation)

We leave no judgment on the project. Although filed as a failure, at least with regard to whatever hopes Frisch had had for it, we cannot rule out that the failure to a higher or lower degree also could be due to overambition with regard to computational demands, as the formulas that Rasch had derived bore out.

5. Conclusion

Frisch was the founder of econometrics as a separate discipline and contributed an impressive conceptual apparatus for studying relationships among economic data. He held firm convictions that analysis of data had to be led and guided by theoretical assumptions. If these were not well-established theoretical laws, they at least had to be "constructive theoretical speculations." He was concerned that empirical analysis without a proper methodological foundation would lead to meaningless and fictitious results, and was highly critical of the "nearly irresistible temptation" of determining the parameters by some chosen procedure without proper consideration of whether the procedure was adequate.

In the glimpses given above of Frisch in exchanges and interactions with other scholars, his skepticism of current procedures comes through clearly. He in many cases doubted that the data used fulfilled the requirements for probability-based measures in regression analysis. This could often be due to simultaneity, as he argued in several of the exchanges. He also held the view that there was a need for much greater coherence between theoretical concepts and economic data. His effort to establish national accounts to support macroeconomic analysis can be viewed in that perspective.

In his laboratory work he inventively searched for alternative methods, as we have shown in the—admittedly—special example of the Presumptive Analysis. In the history of econometric analyses Frisch is a forerunner of the more mature stage and particularly in formalizing identification theory. Frisch exerted influence by his explorative confluence analysis and in the direct influence he had on the next generation of econometricians, particularly Haavelmo.

His hesitancy in using probability reasoning, based on "more or less plausible assumptions," as he wrote to Schumpeter, has been asserted to reflect a deterministic worldview. But this is at variance with the impression Frisch conveyed in his Poincaré lectures (see Bjerkholt and Dupont-Kieffer 2009, lecture 8, and Bjerkholt and Dupont 2010).

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Appendix 1
Discussion of Statistical Regression Problems between Dr. Jerzy Neyman and Ragnar Frisch, Oxford and London, 28 September 1936 and Subsequent Days

[Note by the authors: This exchange between Jerzy Neyman and Ragnar Frisch, during which they literally shuffled note paper across the table and wrote down, in turn, points they wanted to make, followed up the comments Neyman had made about Frisch's confluence analysis in his paper for the Econometric Society meeting. A further letter exchange took place after Frisch's return to Oslo. The document is written in a compressed and abbreviated style as part of a verbal exchange. The authors have tried to enhance the legibility with annotated information, including page numbers, placed in square brackets, and some light editing of the original text.]

[page 1]
[Frisch began the discussion by setting down the notation to be used, in a way that also suggested the issue, "true" or structural relations versus regression results:]

$$x, y, z$$
$$z = ax + by$$
$$z = b_{zx.y} \cdot x + b_{zy.x} \cdot y.$$

[Neyman took over and structured the discussion by putting down key assumptions as three points:]

I. Elementary probability law of z depends on x and y so that we have $\phi(z \mid x, y)$

[Insertion by Frisch: "z *must* be and x and y *may* be random variables!"]

II. The expectation of z is:

$$Ez = \int_{-\infty}^{\infty} z\phi(z \mid x, y)dz = Ax + By = \bar{z}(x, y). \quad (1)$$

[Insertion by Frisch: "$z = Ax + By$ would be the true regression in Neyman's sense."]

III. a_0 and b_0 are calculated by minimizing the sum of squares

$$S_0 = \sum_{i=1}^{n} (z_i - ax_i - b_i)^2. \quad (2)$$

Then $z(x, y) = a_0 x + b_0 y$ is the estimate of $\bar{z}(x, y)$ and if we consider

$$z - a_0 x + b_0 y = \varsigma, \quad (3)$$

where z is the random variable z and not any of its already observed values. The variation of ς will be smaller than that of the deviation of z from any other linear function of x and y.

[Insertion by Frisch: "What about the deviation of z from $Ax + By$?"]

The question is a very good one. It shows the inaccuracies in my statement. I should have stated the problem more carefully, like this:

Consider the case when we first obtain a system of n triples of values of x, y, and z:

$$\begin{array}{l} x_1, \ldots, x_n \\ y_1, \ldots, y_n \\ z_1, \ldots, z_n \end{array} \tag{4}$$

and then take one more triple x_{n+1}, y_{n+1}, and z_{n+1}. We should consider x_i, y_i, and z_i as random variables for $i = 1, 2, \ldots, n + 1$, such that the probability law of x_i, y_i, and z_i is independent of i and as described in I and II.

[Insertion by Frisch: "Yes!"]

[page 2]
Now we use (4) for calculating (2) and then for obtaining a_0 and b_0. Those will appear as linear functions of $z_1, z_2, \ldots, z_n$.

[Insertion by Frisch: "Yes."]

My statement should be understood in the sense that

$$\mathrm{E}(z_{n+1} - a_0 x_{n+1} + b_0 y_{n+1})^2 \leq \mathrm{E}(z_{n+1} - a' x_{n+1} + b' y_{n+1})^2, \tag{5}$$

where a' and b' are any linear functions of (4), that is to say, any other linear estimates of A and B in (1).

[After these preliminaries Frisch introduced one of his pet ideas:]

What I am interested in is to have two coefficients α and β such that—to express it provisionally and vaguely—the distribution of $[z - (\alpha x + \beta y)]$ in future observations is as "good" as possible for my purpose. I may fix the ideas by saying that I want

$$\sum [z - (\alpha x + \beta y)]^2 \tag{6}$$

in future observations as small as possible. For this purpose I believe that coefficients other than the a_0 and b_0 may under certain circumstances be better.

[Neyman took over attempting to clarify Frisch's problem:]

I have the impression that the problem may be treated directly.

$$\alpha = \sum_{i=1}^{n} \lambda_i z_i, \qquad \beta = \sum_{i=1}^{n} \mu_i z_i, \tag{7}$$

where λ_i and μ_i are coefficients independent of the z_i but depending on the x_i and y_i ($i = 1, 2, \ldots, n$) and to be determined so as to have the expectation

$$E(z - \alpha x - \beta y)^2 = \text{minimum.} \tag{8}$$

(I take it that x, y, and z in the formula you wrote correspond to my x_{n+1}, y_{n+1}, and z_{n+1}.) Consider

$$z - \alpha x - \beta y = z - x\sum_{i=1}^{n}\lambda_i z_i, - y\sum_{i=1}^{n}\mu_i z_i = \varsigma \tag{9}$$

and denote by

$$\sigma^2 = E(z - Ax - By)^2. \tag{10}$$

Then

$$\varsigma^2 = z^2 + x^2\Big(\sum_{i+1}^{n}\lambda_i z_i\Big)^2 + y^2\Big(\sum_{i+1}^{n}\mu_i z_i\Big)^2 - 2xz\sum_{i+1}^{n}\lambda_i z_i - 2yz\sum_{i+1}^{n}\mu_i z_i. \tag{11}$$

Now using (1) and (10) we may find the λ's and μ's minimizing the expectation of (11). I believe there is a means of making the calculations simpler.

[page 3]
However, to write those it would require a somewhat quieter atmosphere!

[Frisch responded:]

I have the same impression as you that if the problem is formulated in the above terms, you will get $\alpha = a_0$ and $\beta = b_0$. But your setting does not correspond to my problem. In the first place I am *not* imposing any such condition on α and β that they shall be of the form (7). I am even prepared to accept outside information in addition to the values x_i, y_i, and z_i ($i = 1, 2, \ldots, n$). I may add that I am thinking of Σ in (6) as being extended to a certain future sample x_j, y_j, and z_j ($j = n + 1, n + 2, \ldots, n + m$), but I should think that this latter point does not introduce anything fundamentally different from your setting.

[Neyman clarified his position:]

What I know is limited only to the class of coefficients α and β which are linear functions of the z's in the sample that is being used for estimating A and B. If we abolish the assumption of the linearity, I am not aware of any result concerning the relative value of $E(z - a_0 x - b_0 y)^2$, so that it may not be a minimum.

My x_{n+1}, y_{n+1}, and z_{n+1} do refer to a "future" case; often the previous observations $x_1, \ldots, x_n$, $y_1, \ldots, y_n$, and $z_1, \ldots, z_n$ have been used for estimating A and B. As we take the expectation $E(z - \alpha x - \beta y)^2$ it is perhaps better to omit the subscript $n + 1$, as you have done.

[Frisch continued:]

Do you mean that if we assume that (x, y, z) follow a probability function . . .

[The exchange was, however, adjourned here and Frisch wrote down a remark to himself: “Even assuming (1) Neyman is not prepared to defend that a linear function is the only possible thing.” The exchange was resumed in London on 30 September 1936.]

[page 4]
[Frisch reopened by setting out the issues to be discussed:]

Three kinds of problems:

I. Problems of evaluation (problems of prediction)

II. Problems of elementary regression coefficient

III. Problems of structure (problems of control)

Re I. Formulation of the problem.

x and y are known; what is wanted is to compute z. A statistical material (x, y, z) in n points is given. (1) is assumed. Then Neyman would compute the elementary linear regression of z on x and y. Let it be $z = a_0x + b_0y$. We will also try $z = c_0x$ and $z = d_0y$. Neyman must know *something* about the expectation of z, knowing x and y. If Ez is assumed as a higher degree polynomial in x and y the empirical approach would be based on such a higher polynomial.

[page 5]
We would select one of these three equations that gives the smallest estimated standard error of evaluation. It turns out that for whatever [illegible] the estimated standard error of evaluation is not larger for $z = a_0x + b_0y$ than for $z = c_0x$ and $z = d_0y$, so consequently, one is by this criterion *always* led to take $z = a_0x + b_0y$.

This is the Markoff-Neyman rule. In other words, in problems of evaluation the Markoff-Neyman rule leads to accepting the elementary regression, taking in as many variables as the computation machine can bear—without computing any standard errors—if it is reasonable to assume that the probability law of the complex of variables is as in (1).

[Neyman took over at this point. The further exchange about “Problem I” comprised a graph drawn by Neyman, discussion of the components of the standard error of evaluation (i.e., standard error of deviation plus standard of estimates), and a suggestion of evaluation “in some future experiment.” It was, however, found impossible to render in a coherent and readable form.]

[page 6]
[Frisch continued with problem II:]

II. Problems of elementary regression coefficients

Here the possibility of fixing x and y comes in. Suppose we have dice—with displacements of center of gravity—then if I have a large number of dice—with exact figures for x and y, and if I know that the probability law is of the form (1), I know that I may *change* the average value of z's in the experiments by taking another die. If neither x nor y is a random variable and if we *still* can suppose that we have the same probability law, the same A and B, then we could change the average value of z as we like.

[page 7]
Suppose x is controllable, y and z random—will the control of x—for instance the fact we keep x constant—tend to diminish the standard error of evaluation of z?

[Insertion by Neyman: "It will do so if we choose a particular value of x from which the standard error of evaluation has a minimum!"]

If x is changed and account is taken of the change that this produces in the expectation of y, some influence on the expectation of z may be produced.

[page 8]
The government makes propaganda of making artificial manure, giving credits for this purpose. Some farms use it, others don't. The government wants to know the effects of this manure. Does it *on the average* affect the output of the farm (measured in money) in some way? In this case one would compute the elementary regression of output on a number of factors, and in particular one would be interested in the partial coefficient of output on manure. This coefficient would now be tested by its standard error.

$$\text{Output} = 2.15 \cdot \text{manure} + \text{etc.}$$

2.15 is a figure in which we are interested. It is not just the same to us if this figure is instead, say, 0.5. The testing of the 2.15 coefficient would be done by computing its standard error.

[Insertion by Neyman: "This example is not bad."]

[page 9]
Neyman and Frisch agree that in this case it is essential to have a non-confluent situation. Neyman thinks that the standard errors of the standard errors would not help.

[Frisch noted in the margin, "useful," "superfluous," "detrimental," which in the terminology of the Confluence Analysis denotes the three kinds of effects on the fit of including an additional variable in the analysis.]

[page 10]
[Neyman took over:]

z = output

x = artificial manure

y = farm yard manure

Let

$$\bar{z}(x, y, u) = Ax + By + Cu + D = E(z \mid x, y, u)$$

and $z(x, y, u) = ax + by + cu + d$ being the estimate of $\bar{z}(x, y, u)$.

The standard errors are able to detect cases when we may be practically certain that, e.g., $A > 0$ and even may be in the position to state that, say, $3.5 < A < 4.5$.

But this does not help us much. It may happen that y is highly correlated with x, so that, for example,

$$x + qy = R = \text{practically constant} \quad (R = \text{ totally manure}).$$

Then a unit change in x will be almost invariably accompanied by a change of $(R - 1)/q$ in y in the opposite direction, and the total effect of x increasing by a unit on the volume of $z(x, y)$ will be something approaching $A - (R - 1)/q$.

[page 11]
[The discussion was resumed on 1 October 1936 and Neyman started out:]

Consider the following situation.

Suppose

(i) that one additional unit of artificial manure, x, gives, ceteris paribus, an average increase in output, z, equal to A;

(ii) that one additional unit of farmyard manure of standard quality gives, ceteris paribus, an average increase in output equal to B;

(iii) that farmers try to estimate the quality of the farmyard manure and to reduce the available amount to that of the standard. This estimate y is liable to error, so that

$$y = \eta + \beta, \tag{12}$$

η being the true equivalent of standard quality farmyard manure and β a random error independent of η;

(iv) that by buying the artificial manure the farmers aim at total R of manure per acre, but owing to certain circumstances this total is not exactly reached, owing perhaps to the fact that the area actually manured is not always the area that they intended to manure, the original plans being altered at the last moment, etc.

Consequently the amount of artificial manure used will be

$$x = R - y + \alpha, \tag{13}$$

where α is a disturbance, independent of y. The problem is to determine the values of the population regression coefficients of the output, z, on x and y, and to see whether and what is their connection with A and B, in which we are interested.

The relation between any particular z and x and y is given by the equation

$$z = Ax + B\eta + \gamma, \tag{14}$$

where λ is a disturbance that we shall assume to be independent of the remaining variables. Further, owing to (12) and (13), we have

$$\begin{aligned} z &= A(R - \eta - \beta + \alpha) + \beta\eta + \lambda \\ &= (B - A)\eta + A\alpha - A\beta + \lambda + AR \end{aligned}$$

[page 12]

I have assumed here tacitly that η, α, β, & γ are measured from their population means and that they are independently distributed.

Writing down all the variables x, y, & z in their final form,

$$\begin{aligned} x &= -\eta + \alpha - \beta \\ y &= \eta + \beta \\ z &= (B - A)\eta + A\alpha - A\beta + \gamma \end{aligned}$$

We see that there is only one disturbance in the strict sense of the word. (It appears that the conception of a disturbance is relative to the system, not to single variables.) Moreover, there are three basic variables causing connections of x, y, z. Those are η, α, and β. What is the “structural” or “true” regression equation?

As far as the elementary regression equation of z on x and y is concerned, its population form is given by

$$z(x, y) = Ax + \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_\beta^2} By + \text{constant}.$$

Having a sufficiently large sample we shall get both coefficients A and $(\sigma_\eta^2/(\sigma_\eta^2 + \sigma_\beta^2))B$ with any desired accuracy, and this may be useful.

[Neyman then wrote down the following system:]

$$x = [a_1\xi + b_1\eta + c_1\varsigma] + \alpha$$

$$y = [a_2\xi + b_2\eta \qquad + c_1\varsigma] + \beta$$

$$z = [a_3\xi \qquad + c_3\varsigma \ + d_3\varepsilon] + \gamma$$

[From Neyman's note at the Econometric Society meeting it is clear that the model was meant to raise the issue of a common factor in x, y, z. The model reappeared restated and simplified by Frisch on page 17. The intermediate pages are suppressed here.]

[page 17]

[Frisch took over and set out the following system of equations:]

$$x = a\xi + \alpha$$

$$y = b\xi + \beta$$

$$z = c\xi + \gamma$$

Making variances of α, β, γ very small, as compared to the variance of ξ.

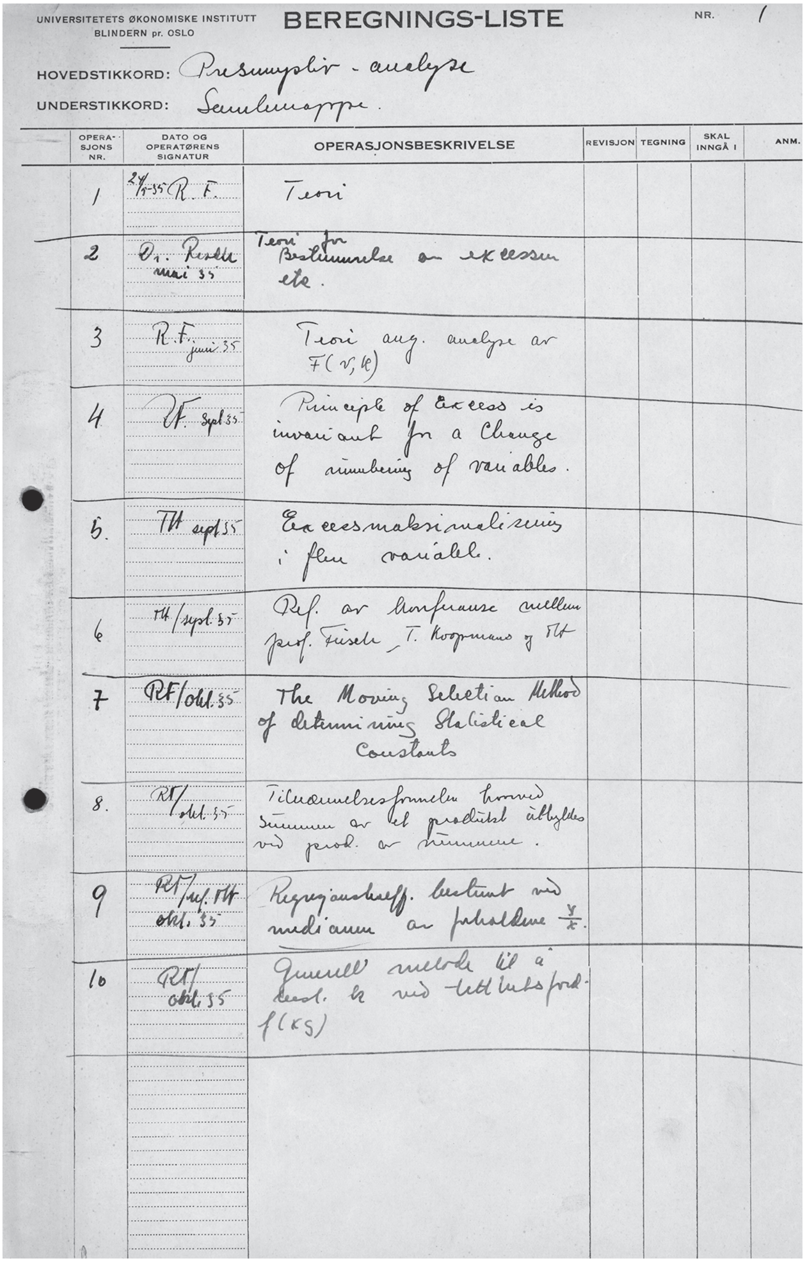
Two problems.

I. To test deviation $\sum(z - (a_0x + b_0y))^2$ in the same sample as served to determine a_0 and b_0.

II. Construct some method of determining a_0 and b_0, using these a_0 and b_0 and determine $\sum(z - (a_0x + b_0y))^2$ in the next sample.

[The scribbled exchange continued over yet more pages. The streak running through it is Frisch's hunt for true structural relations and his skepticism of conventional regression assumptions. He seemed to counterpoise "structurality" and "normality." Neyman made at one point the following remark: "We cannot determine structure with an increasing degree of accuracy by increasing the sample but we can determine with an increasing degree of accuracy *limits* for the indeterminateness. Even if we know the whole population the structural indeterminateness will be present. This is indeterminateness with regard to the disturbances. (Then there is the other more troublesome question of how to define the structural equation when there is *some* correlation between the parts x' and x'', etc.)." Another repeated concern of Frisch was prediction, in particular the "possibility of constructing a prediction equation better than the regression."]

Appendix 2
Inventory of the First Ten Project Documents of the Presumptive Analysis Project



UNIVERSITETETS ØKONOMISKE INSTITUTT
BLINDERN pr. OSLO

BEREGNINGS-LISTE

NR. 1

HOVEDSTIKKORD: Presumptiv-analyse
UNDERSTIKKORD: Samlemappe.

OPERASJONS NR.	DATO OG OPERATØRENS SIGNATUR	OPERASJONSBESKRIVELSE	REVISJON	TEGNING	SKAL INNGÅ I	ANM.
1	24/5-35 R.F.	Teori				
2	O. Reiersøl mai 35	Teori for Bestemmelse av excessen etc.				
3	R.F. juni 35	Teori aug. analyse av F(v, k)				
4	RF. sept 35	Principle of excess is invariant for a change of numbering of variables.				
5.	TH sept 35	Excessmaksimalisering i flere variable.				
6	TH/sept 35	Ref. av konferanse mellem prof. Frisch, T. Koopmans og TH				
7	RF/okt. 35	The Moving Selection Method of determining Statistical Constants				
8.	RF/okt. 35	Tilnærmelsesformelen hvorved summen av et produkt uttrykkes ved prod. av summene.				
9	RF/ref. TH okt. 35	Regresjonskoeff. bestemt ved medianen av forholdene y/x.				
10	RF/okt. 35	Generell metode til å bestem. k ved tetthetsford. f(KS)				

Note: This is a facsimile of the original, which is at the University of Oslo.