Econometric Theory, 6, 1990, 385-402. Printed in the United States of America.

THE ET INTERVIEW: PROFESSOR MICHIO HATANAKA

Interviewed by Koichi Maekawa and Katsuto Tanaka

This interview with Michio Hatanaka is the first in this series given in the East, of which we are very proud. Hatanaka is a pioneer of econometrics in Japan. In the early 1950s he traveled to the United States to study as a graduate student at Vanderbilt University. That step was really unusual in the Japanese profession at that time. His stay in the States was extended to 1966, during which time he taught at Princeton and Rochester. His pathfinding behavior influenced and encouraged many Japanese scholars.

Hatanaka started his academic career as a mathematical economist. This may partly explain why his research papers suggest deep economic thought as well as deep understanding of mathematics and statistics. His early contributions to econometrics are concerned with applying the spectral method to various economic problems. These contributions include the associate authorship of *Spectral Analysis of Economic Time Series* by C. W. J. Granger. His other contributions cover theoretical work on identification and estimation problems for dynamic econometric models, among which Hatanaka's efficient two-step estimator is well known. His research also includes empirical work on the Japanese economy and economic policy, which is relatively less known to people outside of Japan. His attitude toward research has set a standard for Japanese econometricians. His research papers are always a product of deep and full thought, and imbued with his own originality.

His achievements in administration are also impressive. During 1969–1971 and 1979–1980, he was director of the Institute of Social and Economic Research of Osaka University. And during 1983–1985 he was the dean of the faculty of economics at the same university. In addition, he served on the boards of several committees for the Japanese government and the Council of Science of Japan. Among his achievements in administration is the reform of the university entrance examination system, which was notoriously competitive and selective. This was really pathbreaking and led other universities to reform their systems.

This interview, which was given in Japanese, took place on November 20, 1989 in the Economics Department of Osaka University. What follows is a translated version of an edited transcript of that interview. We hope that the readers will share with us the privilege of hearing about the evolution of econometrics as encountered by him since just after the second world war.

Would you like to start by telling us about your schooling at the University of Tokyo as an undergraduate student? Would you comment on the level and quality of courses of economics, statistics, and econometrics and the academic tradition in the University of Tokyo just after the war? How was the students' daily life at that time?

Immediately after the war, while many of the staff of the department of economics voluntarily left the university, the department invited back all the professors who had been purged by the Japanese military regime. Many courses were Marxian, and those which were not were old-fashioned or institutional. No courses of statistical inference or economic theory were offered. The daily life was mostly a search for food. One young professor managed to stand on the platform of a lecture room only to announce that he was unable to talk because he had not eaten since the day before. Later he had to resign because he was not a Marxist. It must have been an exciting time to those students who believed that a communist revolution was just around the corner. The latest developments in Eastern Europe make me feel that I have lived long enough to observe both the rise and fall of communism. The whole of Japanese society was in chaos, and this was reflected in students' lives as well. For instance, some students made fortunes by selling timber for the reconstruction, and other students were active leaders in the Japanese communist party.

How then did you learn economic theory, mathematics, and so on?

Let me add a few words to my previous reply first. Japan did have a tradition of British and European, non-Marxian types of economics dating from the 1920s and 1930s. It was strongest at Hitotsubashi University, which had not only good economic theorists but also applied econometricians estimating the demand and supply functions for agricultural products. In the University of Tokyo, however, it was only the youngest faculty member, Hiroshi Furuya, who was interested in British and American-type economics. He ran private meetings in which Sir John Hicks' *Value and Capital* was read. I joined them notwithstanding my lack of mathematics. Since my education was suspended in 1944 and 1945, I did not even know derivatives. My mathematics was self-taught in my attempt to catch up with the rest of the participants. I did not study statistics. It is fair to say that Japan did not have much of a tradition in statistics or statistical inference.

So, it was mathematical economics that you were most interested in at that time?

Yes.

How about econometrics? Didn't you have any opportunity to learn econometrics at that time?

I learned a little from professors of Hitotsubashi University, but it is fair to reply, no.

Then you went off to study at Tohoku University in Sendai as a graduate student. Was it during this time that you wrote your first paper that appeared in *Econometrica*, concerned with a Leontief system?

Yes.

There were few who published in *Econometrica* at that time in Japan. In those days Japan was not exporting but importing papers on modern economics, wasn't she?

Yes that's true. The library of the American Center in Tokyo had a large collection of new books and periodicals in the field of mathematical economics and econometrics. We hand-copied many articles, and someone even copied a whole book. However, none of us was content with just importing, and every one was trying to find what problems were left unconsidered in Hicks, Samuelson, and the work of others.

Would you tell us about the research environment in economics in the decade after the war in Japan? Please comment on Keynesian economics, Marx's economics, the work of the Cowles Foundation, et cetera in this period.

Mathematical economics was making quite an advance in Japan, as was revealed later in the work of Professor Michio Morishima. Keynesian economics was introduced into Japan before the war, and lively discussions were going on regarding saving and investment, for example. My own professor, Hiroshi Furuya was interested in econometric methods as well, and he mentioned to me the Cowles Commission Monograph no. 10. The only knowledge that I had about statistics was an introductory textbook by Wilks, and I was bold enough to try to study the Monograph. However, how could one possibly understand that volume without knowledge of at least the existence of the problems that surrounded econometricians earlier in the 1930s, and without knowing the general properties of the maximum likelihood estimator? Information was flowing in just enough to know what books to study, but we had no guidance whatsoever. As far as econometric methods are concerned, those days were not much different from mainland China today.

What do you think is the reason for that? Even today in Japan it seems that there are more mathematical economists than econometricians, doesn't it?

I am not ready to explain why Japan did not have a tradition in statistical inference. Today the situation is gradually changing. I don't think your statement is quite correct about people in their twenties and thirties, even though it depends upon the definitions of mathematical economists and econometricians.

In 1953, you went to the United States and entered the graduate course at Vanderbilt University. What made you go there?

For an explanation I have to go back to the paper published in *Econometrica*. After I submitted my draft to the editor, his response was to send me back a galley proof of a paper with my name as the author. Someone wrote it up, taking the essence from my draft, evidently because both my English and the structure of the writing were so bad that writing it afresh was easier than asking me to revise it. By asking the editor, I was informed that it was Nicholas Georgescu-Roegen, whose name I had known as the author of a number of path breaking papers in mathematical economics. Responding to my wish to study in the United States he kindly arranged a fellowship at Vanderbilt University, where he was teaching.

What was your impression about American life styles after experiencing hardship after the war in Japan?

American life styles were known to me through movies, and moreover I was not much interested in material aspects of the American life. Actually, American graduate students were not enjoying the American way of life. What was more surprising than American life was that my study of the English language in Japan contributed virtually nothing to my campus life.

When and how did you leave economic theory?

Vanderbilt changed the subsequent course of my career. Knowing Professor Georgescu-Roegen's research work, I realized that I lacked the talent needed to be a mathematical economist. Vanderbilt gave me a chance to open my eyes to economics other than mathematical economics and econometrics, such as agricultural economics, industrial organization, et cetera, in which they had good professors. So it turned out that I had to disappoint Nicholas.

After that, you worked for many years at Princeton. I expect you have lots of memories of the research environment there during those years. Would you tell us about the people who passed through Princeton during the period you were there and the genesis of your joint work with Granger, the famous book *Spectral Analysis of Economic Time Series*?

I was there for six years. Oskar Morgenstern had the Econometric Research Program affiliated with the department of economics of Princeton University. I was a member throughout my stay at Princeton off and on between 1955 and 1963. Initially I worked on the empirical aspects of the Input Output model. For the first time in my life I handled the data, applied the statistical inference techniques, and, with a program I wrote, operated an electronic computer having only 2^{10} words in its memory. I heard that the computer was designed by von Neumann. For a year I was in a hospital recuperating from tuberculosis, during which time Oskar guarded me from visa and other problems.

Did you meet von Neumann?

No, I did not have that privilege. I think Oskar told me when I arrived at Princeton that von Neumann was suffering from a brain tumor.

How did you join Morgenstern's research program and what position did you take at Princeton?

Oskar had a project on Input Output analysis, and I went there as a research assistant, but later became a research associate. Between 1960 and 1963 I was an assistant professor of economics. Let me return to the previous question about spectral analysis. In 1959 Oskar embarked on the harmonic analysis of economic time series, which John von Neumann suggested to Oskar. John Tukey, who was a professor in the mathematics department, argued that spectral analysis was better suited for economic time series. Incidentally many years later Milton Friedman told me that he was also approached by John Tukey on spectral analysis but he did not accept it. Clive Granger arrived from England just after completing his Ph.D. thesis on spectral analysis. John offered to hold a series of meetings among the three of us (John, Clive, and myself), which I think continued for several months at the pace of once every one or two weeks. Beginning with some time series charts John suggested what to compute; then after gazing at the graphs of the results John suggested what to compute next. Though I did not understand his reasoning well, I was impressed by his pragmatic approach. The computations were done on an IBM 650, which was perhaps the last model run on vacuum tubes. Clive and I, both bachelors then, were computing on the evening of December 24(!) leaving all the windows wide open to release the tremendous heat from the tubes when the air-conditioner went out of order. Clive went back to England to extend his study of the theory, and I stayed to apply the method to a number of economic problems. The outcome was the book.

I was also fortunate to get acquainted with the brilliant Dave Brillinger through spectral analysis. Ted Hannan and Jim Durbin also stopped in Princeton. As an assistant professor of economics I had the honor of assisting Harold Kuhn in his undergraduate teaching of economic theory. I also sat in on the graduate courses of money and banking and public finance to extend my knowledge in fields other than econometrics.

You wrote two joint papers with Brillinger. Do you have any special memories of those works?

Not particularly about the works, but about the speed at which Dave can think.

Would you tell us how your book and spectral analysis in general were received by the econometrics profession when the book was published?

Even though the book was well received by practitioners of statistics, I do not think it was appreciated by econometricians. Milton Friedman as a session chairman of the 1965 Congress of Econometric Society did adopt the paper by Dave Brillinger and myself, but when the paper was submitted to *Econometrica* it was finally turned down after a series of requests for the revisions. Perhaps the paper did not consider the kind of problems that monetary economists were interested in.

After publishing this book, you continued to develop methods of spectral analysis in economic time series till the early 1970s. You seem, however, to have left this field as a research area after that. Could you tell us why?

In 1970 and 1971 three important books appeared in the field of time series, (1) Box and Jenkins', (2) Hannan's, and (3) T. W. Anderson's. I studied each seriously. As far as the covariance stationary process is concerned, the choice between the time and the frequency domains is by and large one of convenience. However (a) the time domain representation can be easily extended to include the stochastic trend, as emphasized in Box and Jenkins. Incidentally this was earlier mentioned in Quenouille's book. (b) The prediction theory is much simpler in the time domain than in the frequency domain, which was shown first by Kalman and later exploited by Box and Jenkins. (c) The parameters in the time domain representation are more amenable to economic interpretation, in part, due to (b) above. The greatest failure in my career is that I did not see this sooner.

Greatest failure!? Do you mean that you wouldn't have chosen the frequency domain as a research area? Is the frequency domain analysis useless in economics?

No, I would not go that far. The consideration, if not calculations, in the frequency domain helps to clarify the nature of problems related to economic fluctuations; for example, how long the data should be, how short the time unit of data should be, and whether or not a seasonal adjustment method is satisfactory. The first two must be related to whether there are adequate degrees of freedom in the estimation of the spectral density, which, in turn, suggests how finely $[-\pi, \pi]$ might be subdivided. A number of econometricians such as Watson and Cochrane are considering the long run relationships over the frequency as well as the time domain. I hear that polyspectra are being used for testing non-Gaussian properties and nonlinearity though I have not yet investigated to see which domain is better for the purpose.

In 1963, you moved to Rochester. Could you describe your work there?

Teaching the graduate courses of macroeconomics and econometric methods and undergraduate courses of microeconomics and introductory economics. A few years ago I happened to meet a student from that time, who flattered me, saying that my lectures developed his understanding of macroeconomics. He is now a professor in the field of money at a well-known university.

After spending 13 years in the United States, you returned to Japan and took up an appointment as a professor at the Institute of Social and Economic Research, Osaka University. What made you decide to return to Japan? Did you find it difficult to continue your ongoing research, after the lively research activity in the United States?

The reason why I returned is that, given my health conditions, I preferred a more quiet but steady career for my research. I did not find much difficulty in my research, except for the computer facility, but this may very well be because the Institute had been managed just like an American institution as much as it could be within the rigid regulatory framework of Japanese national universities. Even the computer facility was upgraded within several years.

A recent tendency is that Japanese scholars trained in the U.S. do not wish to come back, and when invited back to Japan, do not stay here for a long period. This tendency may be explained in part by the difficulties of private life in Japan. I was lucky. The U.S. dollar was strong, and the price of land in Japan was not yet astronomical so that a little saving enabled me to buy a fair sized block of land near the campus. Many years later Lionel McKenzie, who got me out of Princeton to Rochester, said that in deciding to return to Japan in 1966 I must have foreseen the future of the Japanese economy. Needless to say that I did not.

Did the research environment in Japan in economics seem very different to you, from that in the United States?

Yes, the most noticeable difference was the heavier concentration in a few areas of research. It is not an exaggeration to say that every theorist was working on growth models and every econometrician was building macroeconomic models along the lines of Keynesian economics. Reactions can be catastrophic when fields of concentration stop growing or lose their raison d'être.

Perhaps you would like to tell us about the intellectual tradition and atmosphere of the Institute in the early 1970s.

When I arrived, the Institute was just exploding due to tense personal relations. I do not know about the active days of the Institute, which were before I arrived.

You became the head of the Institute twice during the seventies. Did you have any particular policy in administrating the Institute?

The policies that I adhere to are (i) talented people should be hired, (ii) once they are hired everyone of them should be treated fairly even if some are later found disappointing, and (iii) let everyone do whatever research he or she likes to. From the four years experience of full time administrative work I have a great deal to say about the rigid regulation imposed upon national universities by the Ministry of Education of Japan, but I would not elaborate on it here.

Was there any positive externality to you from administrative responsibilities?

Not at all for my research. However, it is a good way to learn how the human organization works, and my personality has been greatly influenced by the administrative work.

Let us return to your academic work. You wrote two famous papers on the efficient estimation for dynamic econometric models in *Journal of Econometrics* in 1974 and 1976. We would like to hear about how that work came about.

My memory is vague, but let me try hard to recall. I was studying Phoebus Dhrymes' book, *Distributed Lags*, in which I think he mentioned the case where lagged dependent variables and serially correlated disturbances coexist. I was aware of the fact that this case arises in empirical econometrics frequently. Earlier I had studied Rothenberg's article on the asymptotic equivalence between the three stage and FIML, in which the method of scoring was given without a name. I wondered if the method was applicable to the problem, and found in 10 or 20 minutes that it was. It took some time for me to explain the derivation accurately because I was not then accustomed to handling asymptotic theory. I am grateful to Chris Sims, who offered comments on a draft at an initial stage. I think the method is more useful in simultaneous equations, but some people do not know my 1976 article in spite of the fact that they know my 1974 article.

In 1980, you left the Institute and moved to the department of economics at the same university. What attracted you to the department?

For many years empirical studies had not been appreciated in Japan in connection with economic theories, and therefore the status of econometrics had been low in the curriculum of graduate schools. The situation began to change when the Bank of Japan and the Ministry of Finance initiated empirical studies on money and finance in late 1970s. A group of relatively young staff of the Faculty of Economics of Osaka University became aware of the new trend, and they wanted me to teach econometrics at the graduate level. Despite strong oppositions from elder staff the group made econometrics one of the compulsory courses for first year graduate students along with micro and macroeconomics. This is now done in some other Japanese universities as well, for example, the University of Tokyo, but Osaka University really led the way as early as 1980.

Yes. There are a number of economists at Osaka University doing good empirical studies.

In Osaka University a group of young faculty staff, all trained in the United States, influenced graduate students to turn their interests toward empirical studies, and the students in turn realized that they had better learn econometric methods. It is necessary to give an econometrics course to the first year graduate students, but I think it is more important to have the faculty's leadership to turn the students' interests toward "how reality is" as well as "what the economic theory tells us."

Is the undergraduate econometric course also compulsory at Osaka University?

It is not, but it is given a status close to compulsory. The only compulsory course is freshman economics.

What was your philosophy in teaching the econometrics course at Osaka University?

I like to begin with my philosophy about econometrics. Different inference procedures are derived from different models. Even though there exist model search procedures, each of such procedures also assumes explicitly or implicitly a model at a higher level, and also the loss function of the person who performs the inference. In choosing a model and a loss function subjective judgments play an important role. A person engaged in empirical studies has to make a number of decisions on the basis of his own judgments in the particular circumstances of the given tasks and the available data. We should teach econometric methods so that the students are able to make their own judgments. It requires reasonably good if not full understanding of the concepts and logic of econometric methodology. In the graduate school of Osaka University the master thesis is compulsory. In it the students survey economic theories and previous empirical studies, but lack time for any serious empirical studies of their own. In the light of this system I gave elementary, basic parts of econometrics to the first year students, and more advanced parts to the third year (i.e., first year doctoral course) students, letting them select the fields of study from wide areas in econometrics. The course for the first year students is compulsory whereas the one for the third year students is not.

Do you feel that the level of econometrics courses in Japanese universities is comparable to that of other courses overseas?

I think those at Osaka University are.

The younger generation in econometrics in Japan are becoming more active internationally. For example, they contributed more than 10 papers in this journal in the past five years. However, most of them were trained overseas. What do you think about this?

I do not care where they are trained. In fact they should be trained at the best places in the world. I do wish that they eventually come back to make econometrics more lively in Japan.

Didn't you have any particular disadvantages in your academic career as a person coming from a small and isolated country like Japan?

I guess I did, even though I do not wish to make it an excuse for my poor research achievement. You say Japan is small. Japan is not small in terms of all academic professions, nor in terms of the economics profession, but it is worth noting that the number of students with interest in economics has been declining in the past 10 or 20 years. Within economics the field of econometrics is now beginning to do well. For example, graduate students in Japan are aware of the recent tendency for knowledge of econometrics to help them to get relatively better academic jobs. Again I am more optimistic about the future of econometrics in Japan than you seem to be.

An exciting panel discussion on "Econometric Analysis vs. Time Series Analysis" was organized by The Bank of Japan in 1981. You took part in it as one of the panelists and mentioned that both approaches would be unified toward dynamic econometrics. Do you think that econometrics is being developed as you predicted or diverging in an unexpected direction? What impression did you gain from the discussion?

I do not recall much of the discussions except that I was not prepared well. At that time some people, unaware of the general trends of econometrics, contrasted "econometrics" and "time series analysis." If the two were mutually exclusive and contradictory, one would not have seen the book by A. C. Harvey entitled *The Econometric Analysis of Time Series*. The proper contrast is between (1) modeling least dependent upon information other than the data, and (2) modeling utilizing heavily the assumptions and the constraints on the parameters that cannot be verified by the data alone. Time series analysis is a group of statistical techniques, and it can be used in either of the two modeling approaches. Harvey's book surveyed the development in (2) until 1980 or so, and the literature on rational expectations represents another development along the lines of (2). On the other hand, the VAR model advocated by Chris Sims belongs to (1). Except for a tautology there is no a priori information that can be considered absolutely true; different investigators have different degrees of confidence about the validity of information other than the data. Therefore, it is not surprising that for two investigators facing the same problem, one uses the modeling strategy (1) and another uses (2). In (2) the effective number of parameters is small, and therefore the statistical inference is made with a large number of "degrees of freedom" relative to the number of variables involved. However, this advantage is "bought" with unreliable constraints and assumptions. I fully agree with Chris Sims on this point. Some of the assumptions can be tested by the data, but there is a limit. For example, the Wu-Hausman test examines only a portion of the conditions for variables to be predetermined. Moreover one cannot test a large number of hypotheses using identical data.

It seems that many people were very reluctant to accept the rational expectation theory when it appeared. What do you think is the main reason for this happening? How did you react to this?

Let me begin with the history of rational expectations as I see it. In the 1950s Cagan, Nerlove, and many others used the adaptive expectations model to describe the formation of expectations. With the finding of persistent biases of expectation implied by the model, Muth proposed rational expectations in 1960. For years it remained quiet, as nothing more than an interesting piece of an econometric model. In the 1970s it was discovered that rational expectations in a specific macro model implied the impotency of some policies. The people who wanted to argue against such policies vigorously publicized RE. So the people who wanted to justify such policies counteracted also violently. But now it seems to be an almost unanimously agreed opinion that the policy implications of the RE are much dependent upon the details of the specifications of models. To me the RE is a working hypothesis regarding the formation of expectation. We should compare the empirical performance of the adaptive and rational expectations models, but it has not been done to the best of my knowledge. The adaptive expectations model has a serious drawback in that economic agents in nonstationary circumstances are not supposed to correct biases in the light of past experiences. The RE assumes that the bias never exists at any moment, and as such it could be applicable only to the fields where the learning process is very rapid, for example, financial and foreign exchange markets and hyperinflations. Empirical studies of foreign exchange markets have found a standard form of the rational expectation model inadequate because it does not incorporate proper consideration about risk premiums. The defect can be remedied by modeling the RE hypothesis using the Euler equation.

Nowadays VAR models can be easily estimated by any standard package and are often used to test the Granger causality. You, however, warned against the use of VAR models in your paper in *Economic Studies Quarterly* in 1983. Could you describe the point for us?

First of all I don't think that inference in a VAR model is simple. The asymptotic properties of the OLS estimator depends upon (1) whether the variables are cointegrated or not, and (2) whether the variables have nonstochastic trends in addition to stochastic trends. The article by Sims, Stock, and Watson shows a systematic way to deal with different cases. My 1983 paper you mentioned, was not written to criticize the VAR modeling, but in any case it would be more appropriate to describe what I think of it now. Important economic theories such as the neutrality of money and the rational expectations have been investigated on the VAR model. However, I accept the results only as a first approximation. (1) Even though the VAR assumes nothing in the way of economic theoretical content it does assume a particular statistical model, especially linearity. (2) The model must include all the relevant variables, whereas the current availability of macroeconomic time series data does not allow us to include all the relevant variables. This is serious especially in Japan because the entire period since the recovery from war destructions has to be split into two: the period of vigorous growth from 1955 to 1973 and the period of mature growth after 1974. (3) I am most critical of policy analysis that is based upon VARs. (a) Many scholars mention Lucas' point on the effect of expected policy changes upon the behavior equations, a point which applies to the VAR as well. (b) Seldom mentioned is the arbitrary decomposition of the innovation covariance matrix, upon which policy simulations are carried out on the MA representation. (c) I like to emphasize that, while changes in policy variables are contemplated, it is assumed that the observed dependence of current policy variables upon past economic variables, that is, what is sometimes called the reaction function of the government, is kept invariant. It denies the intention of policy changes. I do not think that the simultaneous equations model is of much use either. A highly respected Japanese economist found his simultaneous equations model unidentified. Policy analyses are very difficult in either the VAR or the simultaneous equations model.

You have expressed a pessimistic view towards the use of VAR models and simultaneous equations models for policy analyses. It is quite discouraging to applied econometricians, isn't it?

First of all, the policies should be classified into (1) macroeconomic policies and (2) policies concerning a specific sector of the economy. In my previous reply I meant primarily the macroeconomic policies. In evaluating the effects of policies concerning a specific sector it seems often sufficient to know only the reactions of economic agents to policy changes, which are considered exogenous. The traditional approach of econometrics should be adequate to cope with such problems. Secondly the important task of econometrics is to test the validity of economic theories, and econometrics has been more successful in this purpose. For example foreign exchange markets provide rich sources for exciting empirical studies using time series analysis. It is granted that the framework of simultaneous equations is rarely used, but it nevertheless underlies Hansen's GMM.

It seems that there must be some superiority in the use of VAR models over simultaneous equations models since VAR models are immune from identification problems. Do you mean that VAR and simultaneous equations models are equally difficult to use for policy analyses?

No. The VAR and the simultaneous equations model have different kinds of difficulties, and one cannot make a general comparative statement on the "degrees" of difficulties. I would say that the point (c) in my previous reply is a serious drawback of the VAR. As for the policy analysis with the simultaneous equations model the most crucial defect would be the possibly unfounded classification between the endogenous and the exogenous or predetermined variables rather than the identification, The policy analysis would be made in terms of the reduced form anyway, and with lack of the identifiability one might directly estimate the reduced form if one is confident about the classification of the variables. It may be worth noting that the rational expectations hypothesis results in a nonlinear simultaneous equations model with nonlinear constraints across equations, as emphasized by Sargent, and it would be by a great deal of simplification that the RE model be reduced to a VAR form, even if it ever can be.

You always emphasize that econometricians should pay keen attention to the particular nature of the data generating mechanism in economic time series which is very different from those in other fields of science. Could you describe more about it for us?

Economic time series data are not the outcome of designed experiments but observations of a human society. The length of data is often very short when one sample period must cover the times over which the institutional framework and other social circumstances are fairly homogeneous. It requires a number of research techniques and strategies which would not be needed in other fields. First of all the standard asymptotic theory may not be reliable; secondly, to compensate for the inadequate information supplied by the data, any vague, not very reliable information must be exploited in carrying out empirical studies; thirdly, there is a problem of nonstationarity in the sense that models with constant values for parameters gradually lose goodness of fit. The first point must have been recognized by the readers of this journal, where so many articles deal with higher order asymptotic expansions. The second point is difficult to deal with in the maximum likelihood approach, and I think the Bayesian method is better suited. Econometric theorists do not seem to be aware of the third point. For the purpose of economic forecasting one can use a Kalman filter to update the estimates of time-varying parameters, which is done in Chris Sims' forecasting. In evaluating different models those with stable parameters are preferred to those with unstable parameters. Many years ago Ted Hannan said to me that there must be some substitutability between linear, time-varying parameter models and nonlinear, constant parameter models. Recent experiences in some fields seem to indicate this, but I wonder how long a nonlinear constant parameter model maintains its applicability. My philosophy about human society is that there must be laws in a qualitative form, but there cannot be rigorous quantitative laws.

Another important aspect of economic time series seems to be nonstationarity of the random walk type. Box and Jenkins suggested differencing to achieve stationarity, but their approach might be questioned when any economic theory related to the original data themselves is to be tested. I mean that there may be cases where use of levels rather than differences is appropriate. How do you treat nonstationarity?

I would say that we are concerned with levels rather than differences in economic theories, the only exception being a study of business fluctuations. I highly appreciate the recent, major contributions of Dickey and Fuller, Phillips, Nelson and Plosser, Engle and Granger. It is unfortunate that the empirical studies of the rational expectations were made before their results became available. In the textbook which I am writing now I am warning the readers not to trust the empirical results of the RE because trends were treated inappropriately.

Would you tell us about your textbook you just mentioned?

In 1970s we had excellent textbooks, Johnston and Theil. Even though Johnston's was extended in its 1984 edition, it is now necessary to have a textbook covering the topics that have developed in the past 10 or 15 years. Fortunately the level of mathematics of Japanese economics students is higher than the Americans, because the Japanese are trained for mathematical economics. I start at Johnston's level, but I quickly raise the standard. I have tried to give an elementary but full explanation of asymptotic theory. The book covers cross-section, time series, panel, and micro-unit data. It surveys inference procedures on the RE models and even covers unit roots and cointegration.

We do hope that it will be translated into English. How are you going to treat the Bayesian approach in your textbook? One of your former students told us that you seemed to be very close to Bayesian in your lectures at Osaka University.

Yes, I have a chapter on the Bayesian approach as well as one on the maximum likelihood approach. You must have noticed that my replies to a number of previous questions suggest Bayesian methods, and they form a main theme of the book. However, I cannot explain many topics in this textbook. To show how vague information other than the data can be incorporated in the analyses I explain the stochastic constraint on the parameters of regression models, contrasting it to the non-Bayesian pretesting.

Is your favor for the Bayesian approach related to the fact that the sample size available in economic data is usually small?

Yes partly, but only partly. Different scholars would have different degrees of confidence about a specification of a model and a constraint used to identify a model. We have this kind of problem even in the analysis of 50,000 micro unit data. With a large number of data we would be more efficient in rejecting wrong over-identifying constraints, but the validity of the observationally unverifiable part of the specification cannot be tested no matter how large the data set.

What research area would you think important in econometrics in the course of the next ten years?

I do not know. I have committed so many errors of prediction in my career, and I am not qualified to answer your question.

What research topics are you interested in now? Would you describe the abstract of your ongoing research?

In 1986, a number of persons, Masanao Aoki and you, Koichi Maekawa among others, brought Granger's cointegration to my attention as I had lost contact with Clive. Even though the Granger-Weiss article starts out with economic equilibrium, the Engle-Granger article seems to be more concerned with the multicollinearity among trends of different variables. The two are not identical problems. For many years I had felt that most of the disagreement among economists was primarily due to different opinions regarding the adjustment speed to attain the equilibrium. If I had not known the Granger-Weiss and the Engle-Granger articles I would perhaps be happily finishing my career with textbook writing, but cointegration changed that. I developed a statistical method to check the stability of equilibrium in the sense in which stability is used in mathematical economics. Two of my former students are helping me to apply the method to the equilibrium of international capital markets.

In recent years, econometric theory seems to be becoming more and more dependent upon the high powered mathematics and mathematical statistics. What do you think about this trend?

Perhaps you have in mind the invariance principle.

Yes.

Well, the mathematics behind the spectral analysis is also deep. Once the relevance of the invariance principle is recognized, which has been done by Peter Phillips, we just have to try to give an elementary, intuitive explanation to graduate students. It should not be too difficult for those who know roughly the central limit theorem and the theorem about a continuous function of variables that converge in law.

What advice or encouragement would you offer the next generation in econometrics?

Keep up with the trends of economics so that you can think what contributions the econometrics can make toward the advance of economics.

THE PUBLICATIONS OF MICHIO HATANAKA

BOOKS

1. *The Workability of Input-Output Analysis*, Ludwigschafen am Rhein: Fachverlag für Wirtschaftstheorie und Oekonometrie, 1960.
2. *Spectral Analysis of Economic Time Series* (as the associate author of C. W. J. Granger), Princeton, N.J.: Princeton University Press, 1964.
3. *Statistics* (with A. Suzuki), Tokyo: Toyo Keizai Shinpo Sha, 1970 (in Japanese).
4. *Econometrics*, Tokyo: Sobunsha, in progress (in Japanese).

ARTICLES

1952

1. Note on consolidation within a Leontief System, *Econometrica* 20 (1952): 301-303.
2. The structural analysis and a collapse of boom, *Economic Studies Quarterly* 3 (1952): 211-219 (in Japanese).

1967

3. A theory of the pseudospectrum and its application to nonstationary dynamic econometric models (with M. Suzuki). In M. Shubik (ed.), *Essays in Mathematical Economics in Honor of Oskar Morgenstern*, pp. 443-466, Princeton, N.J.: Princeton University Press, 1967.

1969

4. Harmonic analysis of nonstationary multivariate economic processes (with D. R. Brillinger), *Econometrica* 37 (1969): 131-141.
5. Low frequency variation in economic time series (with E. P. Howrey), *Kyklos* 22 (1969): 752-766.

1970

6. A permanent income hypothesis relating to the aggregate demand for money (with D. R. Brillinger), *Economic Studies Quarterly* 21 (1970): 44-71.

1972

7. The estimation of spectra and cross spectra on short time series data, *International Economic Review* 13 (1972): 679-704.

1973

8. On the existence and the approximation formulae for the moments of the k-class estimators, *Economic Studies Quarterly* 24 (1973): 1-15.

1974

9. The performance of short-term predictions on the macro econometric models of Japan (with S. Saito). *Economic Studies Quarterly* 25 (1974): 15-28 (in Japanese).
10. An efficient two-step estimator for the dynamic adjustment model with autoregressive errors, *Journal of Econometrics* 2 (1974): 199-220.
11. A simple suggestion to improve the Mincer-Zarnowitz criterion for the evaluation of forecasts, *Annals of Economic and Social Measurement* 3 (1974): 521-524.

1975

12. The underestimation of variations in the forecast series, *International Economic Review* 16 (1975): 151-160.
13. On the global identification of the dynamic simultaneous equations model with stationary disturbances, *International Economic Review* 16 (1975): 545-554.

1976

14. Several efficient two-step estimators for the dynamic simultaneous equations model with autoregressive disturbances, *Journal of Econometrics* 4 (1976): 189-204.

1977

15. Hypothesis testing in the large macro economic models, *International Economic Review* 18 (1977): 607-627.

1978

16. On the efficient estimation methods for the macro economic models nonlinear in variables. *Journal of Econometrics* 8 (1978): 323-356.

1980

17. Multicollinearity and the estimation of low order moments in lag distributions (with T. D. Wallace), in J. Kmenta and J. Ramsey (eds.), *Evaluation of Econometric Models*, pp. 323-338. New York: Academic Press, 1980.
18. A note on the application of Kalman-filter to regression model with some parameters time-varying and others invarying. *Australian Journal of Statistics* 23 (1980): 298-306.

1982

19. On the stability of the demand functions for money in the U.S. and Japan (with Y. Tsutsui). *Kikan Gendai Keizai* No. 50 (1982): 124-135 (in Japanese).

1983

20. Confidence judgment of the extrapolation from a dynamic money demand function. *Journal of Economic Dynamics and Control* 6 (1983): 55-78.
21. Policy analysis with and without a priori conditions (with M. Odaki). *Economic Studies Quarterly* 34 (1983): 190-210.

1985

22. The identification problem in regression models with time-varying parameters in random walk (with K. Tanaka). *Economic Studies Quarterly* 36 (1985): 133-147.

1989

23. Estimation of a regression model on two or more sets of differently grouped data (with M. Fukushige), to appear in *Journal of Econometrics*.
24. Tests on the attainability of equilibrium conditions (with M. Fukushige), Discussion Paper, Faculty of Economics, Osaka University, 1989.