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Interview with Herman O. Stekler

Herman,

Thanks for sitting down and sharing your experiences with the readers of the *International Journal of Forecasting* and the forecasting profession. Before we start, what are you working on right now?

Besides teaching a couple of course here at GW, I am finishing up serving as guest co-editor for the IJOF special issue on Sports Forecasting. As far as research is concerned, I currently have a number of papers under review and I am working on others with several different coauthors. These involve a variety of topics in macroeconomic and sports forecasting.

Let's begin with a bit of personal background and your undergraduate experiences at Clark University.

My family moved from Vienna, Austria, in 1939 and settled in Worcester, MA. My educational experience was heavily influenced by the impact that this forced emigration had upon my family. My father had been a lawyer in Austria, where canon law was practiced. However, he could not practice in the US because civil law was practiced there, so he became an accountant. My parents insisted that I obtain as much education as possible, but they also insisted that the skills that I obtained had to be transferable to other countries, in case I was forced to emigrate again. I don't know what they thought of my undergraduate major, but they indicated that they would only provide graduate education support if I became a doctor — even though they knew that I could not stand the sight of blood and would prefer to be a lawyer.

I attended Clark University between 1952 and 1955 and received a B.A. in economics with a lot of courses in mathematics. Economics was considered a difficult subject, and at the time students were not permitted to take economics courses until their sophomore year. I really enjoyed my undergraduate years, and I still have a very fond feeling for Clark. Several professors nurtured my interest in learning. Among them, Daniel Gorenstein, who taught in the mathematics department, was the favorite professor of a number of students. While I was strong in that area, he strongly encouraged me to consider applying mathematics to economics.

Other professors also provided encouragement and advice. These included James Maxwell, Chairman of the Economics Department, who was my honors thesis advisor; Morris Cohen and Robert Campbell, who supervised the grant that I received from the Social Science Research Council in the summer between my junior and senior years; Roger Van Tassel, who taught the Senior Seminar and made me put my nose to the grindstone when I wanted to goof off; and James Hurd, who introduced me to the field of industrial organization. This was a relatively new field at the time, but I was fascinated by the application of micro theory to real world situations.

I did not find college that difficult. Between classes and studying I would visit the library to read. I became enthralled with articles in *Econometrica*, which turned out to be a big help later on. In my junior year I was selected for Phi Beta Kappa. Yes, it was a boost to my ego, and others observed a bit of arrogance. In the summer between my junior and senior years I received a research grant from the Social Science Research Council. My project was interdisciplinary, involving economics, political science, and history. I studied the impacts of the Copper Tariff of the 1930s, and the research was later used in my senior thesis and honors

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thesis. These were directed by Roger van Tassel and James Maxwell respectively.

What was it like applying for graduate school? How did you make your decision?

Although I had originally wanted to go to law school, this was not financially feasible. However, I believed that I would be able to get financial aid to study economics in a graduate program.

I applied to graduate schools and for financial aid from the Social Science Research Council, the National Science Foundation, and the Woodrow Wilson Foundation. All three institutions offered me assistance; I accepted the one from the Woodrow Wilson Foundation. In applying for the Woodrow Wilson, I had to list my top three schools in order of preference. I listed them as (1) Harvard, (2) Yale, and (3) MIT. In the Woodrow Wilson interview I was questioned by Paul Samuelson (MIT), Eugene Rostow (Yale), and a third person from McGill whose name I have forgotten. They asked me why I was interested in applying mathematics to economics. My responses about reading *Econometrica* and looking at real world problems surprised them.

Following the interview, I received a telegram of acceptance for admission to McGill, a school to which I had not applied. Given this letter, I made a FORECAST. If I was accepted at a school to which I had not applied, I predicted that I would receive the Woodrow Wilson grant if I revised my preference ordering for the graduate schools. I therefore switched the ordering and moved MIT to the top, because Paul Samuelson had impressed me. Before I received a formal letter of acceptance from MIT, they sent a letter stating that I had funds for my second year. With that letter I knew that I had made a correct forecast: I would get the grant and be admitted to MIT. The General Electric Foundation funded my thesis fellowship in the third and fourth years.

You entered MIT. What was the environment like? Tell us about your experiences and how you became interested in economic forecasting.

The students in the economics program at MIT surprised me by how competitive they were. To me, education was about learning. The students were afraid of Paul Samuelson. In particular, they were scared even to question him. There was no field of macroeconomics at the time, only monetary economics, and econometrics was not taught as an organized course. Bob Solow gave a seminar on the subject. We learned our microeconomic theory from Bob Bishop.

At the end of the second year, students had to take 4 field exams, plus they were required to have taken two other fields, one of which had to be from outside the department. (My outside field was from the Sloan School of Business.) These were taken over a week: Monday, Wednesday, Friday, and Monday. The four written exams were then followed by oral exams in three of the fields.

In the second year, two important events occurred in my training. First, my interest in business and applied economics really came out. I spoke to Paul Samuelson frequently about my frustration and concern that economics was too abstract. There were not enough applications to the real world. Second, I met Sidney Alexander, who would become my thesis advisor. I worked with him as an unpaid RA on his seminal 1958 *Economic Journal* paper, which laid the foundation for my thesis research.

I spent the summer between my third and fourth years at the Rand Corporation in Santa Monica, CA. Bob Solow had recommended me, and I was assigned a project funded by the Defense Department dealing with the Defense Early Warning (DEW) system. This was a radar system from Alaska across northern Canada which was designed to alert our two countries in the event of an attack from the USSR. The research was interesting, and it led to a continuing interest in the defense industry.

My work on my thesis began in earnest during my third year. It built on Alexander's (1958) seminal work. He identified the peaks and troughs, i.e., the turning points, in the FRB Index of Industrial Production. Although his analysis was systematic, it was *ex post*. I wanted to develop a procedure that could be used to forecast those turning points in real time. So I came up with the idea that one would predict a turning point if the indicator was below (above) its previous peak (trough) for n months. Naturally, if every movement against the trend was called a signal, there would be a large number of false turns. This led to an analysis of the tradeoff between the length of the forecasting lead and the frequency of false signals. I suggested that a three months up or down criterion would yield the best results. (The current criterion for predicting a turning point is that the indicator must decline for three consecutive months; see Stekler, 1990; and Vaccara & Zarnowitz, 1977, 1978.) This procedure was applied to the regression that Maher (1957) had developed, and was published as a reply to him (Alexander & Stekler, 1959).

Other topics that were analyzed in the thesis included the relationship between a diffusion index of indicators and/or the orthogonal components of the indicators, and the first difference of the series being predicted. These are issues that others are still examining today.

Although the thesis was written 50 years ago, I still have an abiding interest in these topics. I have written several papers on the forecasting performance of leading indicators, but am now more concerned with the question: Why do forecasters fail to predict recessions? Are they more willing to fail to predict one that occurs than to predict one that does not occur? The evidence is that forecasters prefer to miss turning points that occur rather than to predict recessions that do not occur. In typical fashion, I have always been a contrarian. I personally believe that the cost of a recession is so great that a forecaster should never miss one. (One of my current colleagues argues that she will never predict a recession unless I have already done so, because of my willingness to make that type of error. Incidentally, I predicted the current crisis in October 2007.)

Dissertation well on its way, your thoughts turned to joining the work force? What did you want to do? What were your options? How did the job market process work then?

There was no formal job market back then like there is today. Your advisor, in my case Sidney Alexander, sat you down and asked what you wanted to do and where you wanted to go. They would then contact potential employers.

I did not restrict my search to the academic market: my desire to apply economic theory in the real world was very strong. In 1958 I had worked with Morris Adelman as an RA on a consulting project. It was an industry study that incorporated both law and economics. Once again, law fascinated me. The idea of applying my favorite topics in the business world and the lucrative world of consultancy was attractive. My business sector interviews included Allied Chemical and IBM, and I received job offers from both of them. However, I chose academia, because I did not like some of the culture of the business community. During my visit to IBM, I was struck by the fact that every professional had to have his shoes polished before lunch.

Southern Methodist University in Dallas offered me a very interesting position as an econometrician, but I declined, for two reasons. First, I had enjoyed my time in California the previous year and it was the go–go place in the US. Also, it seemed that they wanted to slot me narrowly into econometrics, which I was not prepared to do.

You accepted a job offer from the Haas School of Business at UC Berkeley. What was Berkeley like?

The job market was different then from what it is now: there were no job market papers, and one did not go and present a paper; instead there were interviews at the AEA convention. My interview occurred during the famous NY Giants–Baltimore Colts championship football game of 1959. I was a Giants fan, and when I left my hotel room they were ahead; I forecasted the final outcome incorrectly. I accepted Berkeley's offer of an Assistant Professorship in the Business School. The teaching load was also different then — there were three courses each semester.

Initially, my research focused on industrial organization topics, as was appropriate for a business school. I wrote theoretical papers on market structure and firm size. In addition, I empirically modeled the relationship between profitability and firm size (Stekler, 1963, 1964), to test whether Alexander's previous results still held. At about this time the Business School received a large research grant from NASA. These funds helped to support my research and led to a book (Stekler, 1965) which examined the structure and performance of the aerospace industry. Here I benefited from my graduate fellowship at Rand; the book was well received, but not until I had switched fields again and returned to doing research on forecasting.

My interest in forecasting resurfaced because I was doing consulting work for the Bank of America and assisting them in preparing their projections. I was also approached by another consultant who was advising a bank as to whether they should finance a large modeling effort by Otto Eckstein. My task was to evaluate every econometric model then in existence and determine whether any of them provided accurate forecasts. They did not. For my efforts I earned $6000 and published two papers, while Otto Eckstein formed

DRI. Those papers had a very important impact on my career.

Before we get to the rest of your career, do you want to say anything about the atmosphere on the Berkeley Campus?

I was at Berkeley during some of the most exciting, and also the most contentious, years that any campus has ever experienced. It was at the time when students began to express their views about the Vietnam War, and the campus authorities tried to curtail some of the protests. Being relatively young and liberal, I naturally supported the students. Of course, this made me the most liberal member of the Business School faculty, and led to some consternation among my colleagues.

How did you go from the radical Berkeley campus to the staid Federal Reserve Board?

A colleague of mine, Sherman Maisel, was named a Governor of the Federal Reserve Board. I wrote to him in the spring of 1966 inquiring about working at the Board, and he arranged for me to have an interview at the Board during the spring break. I also had an interview with the IMF. I started work in the National Income Section of the Research and Statistics Division in July 1966. The National Income Group prepared the Greenbook forecasts for the FOMC meetings. Economic forecasts were included in the Greenbook for the first time in the fourth quarter of 1965. They were basically set of a judgmental forecasts from back-of-the-envelope calculations. The FRB-MIT-PENN macroeconometric model was under construction and being introduced during my time at the Board, but it was not yet being used for making forecasts.

I had two main tasks at the Board. First, I was expected to help prepare the forecasts, and sometimes wrote an explanation of the rationale behind the numbers. Second, I was encouraged to undertake research projects that would improve the Board's forecasting process. The experience at the Board was new to me and led to a set of research questions which I have continued to investigate throughout my career. I tended to focus more on the practical aspects of macroeconomic forecasting than on the technical econometric issues of getting the model to run. I was involved in trying to rationalize and provide the economic intuition for the forecasting procedures.

Two areas were of particular interest to me. The first dealt with data quality. I was concerned about the accuracy (or lack thereof) of the data used in the models. This could be due to the use of preliminary data, which were subject to revisions; data which needed to be forecasted because it arrived with such long lags; and data subject to measurement error.

The second area which continues to interest me has to do with the search for and use of "leading" indicators for signaling turning points. The indicators in this context were individual macroeconomic variables. My thesis and my work with Alexander had looked at indicators for the "business cycle". I now began several research papers with Susan Burch (Burch & Stekler, 1968, 1969), including one on the value of using consumer attitudes in forecasting.

In the spring of 1968, my research findings and forecasting intuition proved useful for monetary policy. The Vietnam War was heating up and Federal Spending programs were being expanded. The preliminary retail sales were reported to have increased by 3% in one month, and, based on these numbers, the Board's economists became concerned that the economy was going to overheat and create inflationary pressures.

I did not believe that the numbers were accurate and told my supervisor so. I had examined inventory data that referred to the same time period and felt that the retail sales data were in error and should be revised downward. A large increase in retail sales would suggest that inventories should have fallen dramatically, but instead they had remained the same. My advice prevailed in this instance. The retail sales data were lowered in the next two revisions, first to a 1% increase and then to a 1% decline, from the original 3% increase.

You met Lois Ernstoff while at the Fed. How did this happen?

A colleague in Research and Statistics, Joan Turek, introduced me to Lois, a classmate of hers from Yale. Joan told Lois that she had invited me to dinner. Lois said, "Alright, I will come to your place for dinner." Joan responded, "No, I invited him to your place so that he could see how well you can cook." That is how we met in July 1966. Lois was working for the Institute for Defense Analysis on international economic issues. Colleagues started to notice that I no longer appeared to live in the office, would actually leave early, and was no longer so intense about a 0.5%

revision in a forecast. I was smitten. We were married in February 1967.

Working at the Fed seemed like such an intellectually stimulating and ideal place — what made you want to leave?

I really enjoyed working at the Board; it was fun being so close to the decision-making process and working with practical forecasting issues and real time data. However, it became frustrating. At that time, FOMC meetings were held every three weeks. The staff spent two weeks preparing for the next meeting and had only one week for thinking and conducting research. I wanted more time for the latter and decided to return to academia. I started looking in late 1967, and had interviews with a number of schools at the ASSA meetings.

I almost cancelled my interview with U. of Chicago at the meetings because Lois was 8 1/2 months pregnant and was experiencing discomfort when I left the apartment. In fact, she was about to deliver our first daughter, Beth. Fortunately Beth arrived after the meetings, on December 30th in time for the tax benefit. Friends and colleagues have accused me of making her run around the block in order to obtain the tax deduction for that year. I admit to being frugal, but I cannot be accused of doing that! In the 1960s employers could discriminate more than they can today. Lois was working for a think tank. At the time, women had to leave their jobs when they reached the 7th month of pregnancy. Lois enjoyed her work and did not want to leave. Her employer kept checking with her as to when her 7th month was going to arrive, and Beth was born nearly two weeks after the “official” 7th month.

Why did you choose Stony Brook? What was it like there? Who did you collaborate with?

In 1968 I had a number of job opportunities, and chose to join the faculty at the State University of New York – Stony Brook as a full professor. The department and university were expanding, and it was indicated that the department was going to become a center for applied economics and that my research would be valued. Instead, for some reason the department went in the opposite direction, towards theory and with a heavy emphasis on mathematical techniques, rather than the understanding and application of economic theory.

Nevertheless, I found colleagues to work with. Sheldon Chang was in the electrical engineering department, and we wrote several papers (Chang & Stekler, 1976a,b, 1977) together on the application of control theory and its use in macroeconomic forecasting.

I also returned to my interest in understanding and predicting “turning points”. This led to an AER note (Stekler, 1972, 1974) asking how people can look at the available contemporary information and miss turning points. Does everyone have to have a prior of zero for missing the turning point? This has been a continuing question throughout my research, and Marjorie Schnader and I looked at this in several papers (Schnader & Stekler, 1990, 1991). We discussed it in terms of (1) prior beliefs and information, and (2) the costs of making Type I and Type II errors.

I also continued to visit the FED until 1971 to continue my research on macroeconomic forecasting. Among the papers written during this period was one with Jared Enzler in the *Journal of Business* (Enzler & Stekler, 1971). We conducted one of the first decompositions of a forecast’s performance in a recession, the 1968–69 one.

The Steklers move to Washington. What was the incentive?

Lois was offered an excellent position in the International Finance Division at the Federal Reserve Board. I was a full professor with tenure and still mobile. There were plenty of opportunities for me in Washington. I worked on President Carter’s Council on Wage and Price Stability (COWPS), before moving to the Industrial College of the Armed Forces (ICAF).

What was it like working in a policy environment?

The work at COWPS was very applied and data oriented, and I enjoyed that part of it. We were working with real time data and trying to understand the impacts of inflation on consumer expenditure and business investment. We were involved in the deregulation of the airline industry. This turned out to be a classic example of forecasting changes or turning points and different expectations on the economic actors. The economists at COWPS accurately predicted that it would lead to lower airfares and greater competition, but they failed to predict some of the structural changes that occurred. I am still trying to figure out why they missed these changes even though business analysts predicted them. Overall, though, the pace at COWPS was overwhelming. There was little time for reflection and real research.

You returned to academe of sorts at ICAF. What was that like?

I first moved to the Institute for Defense Analysis (IDA) after COWPS. There I worked on forecasting for the construction industry and technology for military aircraft. These were topics I had investigated in the 1960s. I loved the research environment but was restless and wanted to teach. Then the military discovered that I was an economist and not averse to teaching in a military environment, and I was offered a position at ICAF. One of the people who recommended me was Mary Holman, a professor of economics at GW. There I taught courses in defense economics, industrial organization, and (believe it or not) military strategy.

I was able to conduct research on various topics in defense economics, including the economic impact of another World War II type confrontation with the Soviet Union. It again became clear that my real love was forecasting.

You have been an associate editor and book review editor for the IJOF and a director of the IIF, and are an Honorary Fellow of the IIF. How did you become involved with the organization?

While I was teaching at ICAF, I read an announcement about the IIF conference in 1982. ICAF agreed to fund my attendance since forecasting was one of my areas of expertise. There I met Robert Fildes, who introduced himself by saying, "It is a pleasure to meet one of the old timers in the field of forecasting." He actively encouraged me to participate in the organization, and we have been friends and collaborators ever since.

While writing my thesis in 1986–87 I was forced to read nearly every one of your papers. Charles Nelson, my advisor, said that anyone interested in forecasting needed to know your work and contributions. Nelson also referred to you as one of the old-timers. We finally met as panelists at the Federal Forecasters Consortium Conference in the spring of 1992. We had corresponded a few times and you had helped me with working papers. After the conference we agreed to have lunch. You came to GW for lunch and never left. How come?

While I was still at ICAF we had lunch together and I met a number of members of the economics faculty. As I was planning to retire in 1994, I wondered what I was going to do next. Lois told me to do whatever I wanted to do. "Go and spend 100% of your time forecasting and teaching as you love to do." So I called Joe Cordes, the department chair, and asked whether there was an opening at GW. There were no regular full-time positions, but he suggested that I come as a research professor for a year and he would give me an office next to you. One year turned into fifteen years, and here I am.

With one exception, my years at GW have wonderful. On the extremely positive side, I have worked with you, Bryan Boulier, Bob Goldfarb, and Tara Sinclair. Working with all of you, I have picked up new techniques and new ideas which have stimulated my research. The one exception was when Lois died in 2004, after almost 38 years of marriage. The loss was especially hard on me because we had always worked as a team. The whole department was very supportive throughout the entire period.

While at GW, I have spent time on teaching, which I love. I have had the opportunity to teach Money and Banking, Intermediate Macroeconomics, The Forecasting Seminar, and the senior Proseminar in Economics. The last is a course where the students write a research paper. Thus, I have been able to get into even more research and a source of free labor.

(*Interviewer's note: Herman has published* 35 *papers with numerous co-authors, including a number of undergraduate students!*)

Let's talk more about your interest in turning points.

Some people argue that turning points are unpredictable. I disagree. I have never had trouble predicting recessions. In fact, I have predicted $n+x$ of the last n recessions. What I have tried to understand is why people have failed to predict recessions and turning points. My work with Schnader we have already talked about. That work suggested several hypotheses, and has led to research with Paul Goodwin and Dilek Önkal which tests several of these in an experiment. I believe that people have asymmetric loss functions when it comes to calling a turning point.

Perhaps we should once again revisit how to use leading indicators and call turning points. My thesis considered this topic, and several papers from the

1970s and onwards were along these lines. Starting with Stekler and Schepsman (1973), I have written a number of papers exploring this topic. Two other good examples are Stekler (1972, 1976b). I think that the emphasis on forecasting with various types of models has resulted in the profession paying less attention to the leading indicator approach.

In addition, we should consider great ratios that can provide information. These include ideas mentioned earlier such as inventory-sales, the savings rate, etc. One example of this research is the question of whether the savings rate could serve as an indicator for macroeconomic activity (Stekler, 1976a). Vaccara and Zarnowitz (1978) provide a useful discussion of forecasting with an index of leading indicators. There is evidence that this type of information was not used efficiently in the lead up to the current crisis.

Is this why you have been so interested in forecast evaluations?

The economics profession seems to view forecast evaluations as nothing more than horse races and beauty pageants. I place a much higher value on them. Forecasting is an ongoing process and one which economists are continually asked to perform. I am interested in what we can learn from both forecast evaluations and the forecasting process. For example, if we could obtain insights about forecasters' loss functions, we might be able to improve our forecasting record at turning points.

Another example is the systematic errors that forecasters make. They overestimate growth during recessions and underestimate it during recoveries. This is supposedly consistent with the Hatanaka (1975) and Samuelson (1976) explanations. They argue that the underestimation of changes was related to the belief that the variance of the actual outturns would be greater than the variance of the predicted outturns. However, at least early on the opposite result held (Stekler, 1975). This should be examined again. At the same time, this would only explain the average tendency to underpredict changes, not the systematic errors that have been observed (Fildes & Stekler, 2002).

Since my interest in forecasting extends beyond macroeconomics, Joe Cordes once said that I am willing to forecast anything that moves and everything that stands still. I therefore also evaluate sports forecasts. Looking at those forecasts has an important advantage: the vast amount of data; for example, Song, Song, and Stekler (2007) examined over 30,000 predictions made by sports analysts and statistical models. Generally, the main results obtained from those studies are consistent with the findings from the economics literature. However, one result overrode a belief held by many in the economics forecasting community: statistical forecasts are not significantly better than judgmental predictions (Stekler, Sendor, & Verlander, 2010).

What are your views on structural macroeconometric models?

The technical arguments over this estimator and that estimator do not intrigue me as much as the way in which the information is used. For example, what are the role and impact of real time data that are preliminary? Are there insights into the dynamics of the economy that could reveal the possibility of turning points? Forecast evaluations are an integral part of the scientific method; the economics profession should not forget that.

In 1968 I wrote about the forecasting record of macroeconometric forecasting models. I concluded that the models were not entirely successful in forecasting economic activity. Nelson (1972) obtained similar results, i.e., that it was possible to forecast better with less well-specified models containing accurate past information than with better-specified models using preliminary data. These findings are in accord with my belief that all evaluations should be based on real time data. Forecasters are forced to make their decisions based on the data that are available to them, not the numbers that history provides.

The critical lesson from this is that the relative accuracy of one forecasting approach over another based on some measure should not be used to exclude the "weaker" forecast. There can still be valuable information content in the latter. This has been demonstrated in the literature on combining forecasts, beginning with Bates and Granger (1969).

Does this interest in real-time data explain why you are looking at forecasts made during the Depression?

Yes, that is the reason. While they are hard to find, there are a lot of explanations for that period that have been published in one journal. Bob Goldfarb and I (Goldfarb & Stekler, 2000) wrote about the forecasts that were made in 1930. What is interesting is that observers at that time were able to accurately document what was happening in real-time. They noted the continual decline in economic activity, but kept saying that the recovery was about to happen. As is shown in work that has not yet been finished, I have found that in 1931 the forecasters changed their perceptions and began to say that there was no end to the decline in sight. I am now trying to determine what made them change their perceptions, which might give some insights into the forecasting process.

Let's turn to the current crisis and your thoughts. Could it have been predicted? Do you think that there are indicators of crises?

As far as the current crisis is concerned, I don't really know whether it was (not) predictable. With hindsight it is always possible to find information that "proves" it one way or the other. However, I do have some interesting questions to pose. For example, why was the financial community better informed about the dangers than the Central Bankers? I read somewhere that they saw the dangers as soon as two of the Bear Stearns hedge funds collapsed, but I believe that this was not recognized as a warning sign by the macroeconomic decision-makers or forecasters. Also, how did the leading indicators behave in the months prior to the beginning of the recession? I personally predicted the recession in October 2007. *(Note: Herman made this prediction numerous times to faculty members and students in the Department of Economics.)* Now I have to go back and look at the information on which I based my prediction. Unfortunately, I did not keep a diary.

What are the prospects for recovery?

This recession has no counterpart in any other post-World War II recession. This makes forecasting even more difficult than usual. Currently (end of August 2009), the standard forecast is for the trough to occur sometime within the next few months, then a very slow recovery is expected to take place continuing into 2010. However, I don't know whether this path should be considered the most likely scenario, as I don't know where the major stimuli for a recovery will originate. Consumption growth is likely to be weak given the likelihood that the savings rate will increase; investment might not grow given the existing excess capacity and slow consumption growth; local government spending is more likely to decline than grow; and the stimulus effect of federal government expenditures might have worn out. Net exports might increase, but that depends upon the growth of other nations and/or a substantial decline in the value of the dollar. Thus, there is a strong probability of a double dip recession, though the second dip would be less serious. But remember, I do predict more recessions than actually occur.

What are you going to do next?

I am not planning to teach any regular courses after December 2009, but I will work informally with students on research projects. The current crisis will certainly be a topic of interest. However, since I retired once before, in 1994, and have previously made a similar statement, I doubt whether anyone can accurately forecast whether or not this will be the peak (end) of my teaching career.

As far as my research is concerned, I would like to finish what I started. I have been working on the evaluation of joint forecasts such as GDP and inflation. Sinclair and Stekler (2009) and Sinclair, Stekler, and Kitzinger (forthcoming) examine the problem of correctly predicting the signs of both variables simultaneously. The problem of evaluating the magnitudes of the errors of each of the variables must be done in the context of a decision rule, and this work has not yet been completed. I am also continuing to look at the Depression forecasts and want to turn to some of the post-WWII periods to determine how perceptions are formed and then change.

What else will I do? Travel with Alice, my significant other, attend conferences, and, since the future is uncertain, who knows?

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