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Spyros Makridakis: An interview with the International Journal of Forecasting

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Spyros Makridakis is on the right of the photograph, Robert Fildes on the left.

Following the attainment of a place in the Greek Sailing Team in the Olympics of 1960, Spyros Makridakis set sail for New York University where he obtained a PhD in 1969. His first academic appointment was at INSEAD where he is now a research professor. He has twice won the "Best Teacher Award" there. Spyros has held teaching and research positions with several other European and American institutions; as a research fellow with IIM

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Berlin, an ICAME fellow at Stanford University and a visiting scholar at MIT and Harvard. He is also currently a professor at the University of Piraeus. Whilst an academic he has advised numerous international organizations and government agencies and consulted worldwide. In addition to teaching and consulting expertise, he has also authored, or co-authored, twenty books including *Forecasting, Planning and Strategy for the 21st Century*, included in the list of the "One Hundred Most Influential Futurist Books" compiled in *The Encyclopaedia of the Future*, (The Free Press) and *Forecasting Methods for Management* (Wiley) which reached its 5th edition and sold more than 120,000 copies in twelve languages. He has also published more than 120 articles and book chapters. His paper "*The Accuracy of Extrapolative (Time Series) Methods: Results of a Forecasting Competition*", was voted as the most favourite paper published in the field of forecasting during the last 25 years. He was the founding chief editor of the *Journal of Forecasting* and the *International Journal of Forecasting*. His latest books are *Greek Management: Current State and Emerging Trends* (EAΣE), and *Forecasting: Methods and Applications* 3rd Edition (Wiley).

In 1998–1999, he was the Chairman of the Board of TelePassport Hellas (a telecom firm) and from the beginning of 2000, until May, 2004 he was the Chairman of the Board of Lamda Development (a holding company, listed in the Athens Stock Exchange). He has also been on the Board of Directors of five other companies and is currently the Chairman of the Board of PrivatSea, a yachting company, part of the Latsis Group. Spyros' current research interests centre around how future technologies, and in particular the Internet, will affect business firms (and societies in general) and what kind of organizations and strategies will be required in order to anticipate, and exploit, emerging opportunities while steering clear of the dangers associated with such technologies.

The following edited interview took place in Piraeus, October 2004, nearly 25 years after the foundation of the IIF.

Q: Spyros, you found yourself becoming a top international academic. You started life here in Greece in a rather difficult period of Greece's history. How easy was your path to academic success? How difficult?

A: Well, the most difficult change was when I left Greece and went to New York University and I had to take courses. In Greece, they give you one book that you read for the whole course; then you had to memorise it and take an exam on this single book. When I went to New York, the teacher gave us two books, and told us... "Read them and come back next week to discuss them!". So, you understand my state of mind... when my English was non-existent and I had to read two books within a week. So, I panicked!

Q: But, how did you get to New York in the first place? Was that a big decision in itself?

A: At this time [in the early 1960s] I was sailing [one of Spyros' favourite hobbies] ~~at the time~~ with the managing director of a big company in Greece, and he told me, "Look, if you want to do something with yourself – you'd better leave Greece and go study abroad, preferably business!". So that's how I went... and I was lucky because Business schools were just starting in the USA at that time so I found myself in an emerging field with great demand for PhD graduates.

Q: You got a scholarship or...?

A: Well, in the beginning I did not get the scholarship. I went on very little money, but 6 months later I got the scholarship. At this time computers were just starting. I remember... (laughs)... the first computer I had access to was an IBM ~~6020~~ 1620. Input was through punch cards; you could see the little lamps of the computer working on multiplications and divisions. I learned how to do programming very quickly. From that point on it was very easy to get a scholarship. And not only a scholarship, but professors who, doing their consultancy, used me as a research assistant to help in their work. That meant that I was getting an amount of money that for me, at this time, was mythical! And it supported me through my degree without having to work in restaurants, as most Greek students were doing to sustain themselves.

Q: Your doctorate in NYU, what subject was it in and did it involve forecasting?

A: In NYU we had two majors, for me one was statistics and the other was management. So I had to take a lot of courses in psychology and general

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management, subjects which I think became very useful to me later. My involvement with forecasting came later when I went to INSEAD.

Q: So you didn't study any forecasting in your doctorate?

A: No, no, my doctorate was in Management Information Systems, that were very popular at the time, and it was only when I arrived at INSEAD that I got involved in forecasting. I was teaching a course in Stats and no professor wanted to teach Forecasting. I was there at that time with Steven Wheelwright. So they told Steve and me, "Ok, now you teach forecasting…". And that is how we started. We tried to find a book, to find any material to support the course but without success… so we basically said: "Let's write a book!" And that's how our first book came about, "*Forecasting Methods for Management*" book (Makridakis & Wheelwright, 1989, first published in 1973).

Q: The next stage in your career was an impressive job at INSEAD.

A: Well, INSEAD, don't forget, was just starting; it was not very difficult to go to INSEAD. If you were European and had completed a doctorate at a decent US school there were lots of opportunities. What was much more difficult, was to manage to adapt to the environment and, equally important, to make INSEAD more research oriented. The hardest part was to change INSEAD, to make it more academic. There was a period of time, maybe 10–15 years, when I was publishing more than the whole of the rest of the INSEAD faculty put together. So in the beginning it was really tough because there was no support, even to have a paper typed. There were no PCs of course; typing a paper was a major consideration… and you could not type it on your own, you needed a secretary, you needed someone to proofread it, to check the spelling… a lot of work! Things that are now taken for granted… if you wanted to make some changes to the paper… it was cut and paste with the scissors and glue… (laughs)… literally!

But you know, slowly we managed to change the culture, starting with a very, very few people, to make INSEAD more academic, to put increasing emphasis on research. The advantage of this was that since there was practically nobody doing research, there was a lot of money available for those who wanted to research. And one of the benefits was that I got funding to support Michele Hibon to work for me as a personal, full-time research assistant – practically from my arrival there. And Michele helped a lot to do all of the time-consuming computational parts of our research. Because even when we did the first comparative forecasting study in the mid-70s, and had only 111 series to analyze, it was not easy to process them in the computer we had access to at the time. And the memory and the hard disk space available were also serious bottlenecks when carrying out large-scale (for the time) empirical research. I remember we ran programs for the whole weekend to have the results on Monday morning.

Q: So, when did the work on forecasting competitions start?

A: This work started in 1975, submitting it first to the *Journal of the American Statistical Association* (JASA). And basically they said: "We do not publish papers like this!" Because it didn't fit the preconceptions of the time! We finally published the paper in 1979 in the Royal Statistical Society (Makridakis & Hibon, 1979);

Q: As you just said, you wrote the book for an INSEAD course. What made it so popular do you think?

A: Well, at this time, all the existing forecasting books concentrated on just one topic. Let's say there was a book that was dealing with statistical forecasting, and not all of statistical forecasting but just exponential smoothing like the book of Brown (1963). And there was the book by Box and Jenkins (1976) that only focused on their approach to ARIMA models. Other books discussed only qualitative or technological forecasting. And, our idea with Steve Wheelwright was… "Let's integrate all of these areas", they are all approaches to forecasting, and put them in a book. And not make it technical, but make it easy enough for the people in management and students without a strong quantitative background to follow. That was the success of this book. By the way, I do not know the exact figures; maybe it sold more than 120,000 copies… in 12 languages.

Q: Is it about time for a new edition?

A: No, no more editions… (laughs). So, that's how we started and the next logical thing was: "Why we do not also write a book that is more

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academic?" That is how "*Forecasting, Methods and Applications*" came about; the idea basically was the same, but written from a more academic, more statistical standpoint. It is still selling well after all these years. Even now if you go to Amazon and put the keyword "forecasting", that's the book that comes first (Makridakis, Wheelwright, & McGee, 1983).

Q: You've talked about the early books and establishing a research environment at INSEAD. You quickly developed a number of research themes that have stayed with you for most, if not all of your career. How would you describe these?

A: Well, let me tell you what made me start the research on "Competitions". I was teaching a course on forecasting for executives, and sometime later one of the executives who had taken the course came and said: "could you do some consulting for us?" Of course I said: "Yes, I'll do it". And I went to this company and he was not using Box–Jenkins. At this time Box–Jenkins was 'the method!' Remember the mid-70s. "Why don't you use it?" I asked. And he told me: "Look, I cannot explain the method to my boss!... to my executives. So I prefer not to use this method." At that time, we (academics) all thought that the Box–Jenkins was the most accurate [of the extrapolative methods]. Instead, he used one that was much simpler; he was using decomposition, because he could explain it to his boss and executives. So the idea then came to me; let's try to see how much more accurate is Box–Jenkins and then we can help people make the choice. Some methods may be more complicated but let's find out what it costs to a company, in terms of reduced accuracy, if it is not used, how much a company loses by utilizing simpler methods. That was the idea that started off the research. In the beginning, I thought to generate some simulated series. So we generated the series and we were finding you know ... (laughs) that Box–Jenkins was not doing well! Simple Exponential Smoothing was doing better! So I said there is no way we are going to publish these results, because they will tell us the reason is that: "you have generated the particular series". We need to find some real series. I knew several companies through the consulting I was doing at the time, so we manage to collect the 111 series and we started analyzing them. We started with the simple methods like Naïve 1, Naïve 2, Exponential Smoothing and then we went up the [complexity] scale; and as we were going up the scale, we were finding that the accuracy was not getting better, to our big surprise! It is very difficult to realise how much time we spent checking the results. Because my belief at this time was that there was no way that Box–Jenkins and the other advanced methods would do worse than Naïve 2 and Simple Exponential Smoothing.

Q: The previous work [on forecast comparisons] by Newbold and Granger (1974) that the Royal Statistical Society published... did you respond to that in any way?

A: Yes we looked at all the citations that they included. You could say Newbold and Granger were rather the exception [in that Box–Jenkins ARIMA delivered the best performance] while many other studies were finding the same results as us. These studies were not so well known. So once we found all of these studies, we started getting a little more confidence that we are not doing anything wrong. So we wrote up the results and submitted it to the *Journal of the American Statistical Association*. But the reviewers and the editor basically said: "Forget it! It's not worth publishing; these results do not make any sense!" So that was the first (laughs) ... surprise, learning something of the politics and mentality of academia; and, having read the book of Thomas Kuhn (1962), I realised that science does not work the way that science is supposed to work! They [the JASA reviewers and editors] didn't give any reason... that the methodology was not correct or this or that. Essentially they said the results do not support... "our theoretical perceptions" or biases... so they chose not to publish the paper.

Q: That's one key research idea of yours, competitions, empirical comparisons. What are the other major themes that you identify in your research?

A: Well, a major question is when to use a particular forecasting method? In what situation? On what kind of data? If I am given a set of data which method do I select? What happens if the problem is with short-term, medium-term, long-term forecasting or whatever? So a good deal of my

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forecasting work has centred around these questions. However, no matter what was the data, no matter what was the situation, always it was very clear that in terms of forecasting accuracy the simpler methods were doing the same or better than the sophisticated methods! In addition, we were finding that combining various methods, some of them clearly suboptimal, was improving the forecasting accuracy of individual methods while the variance was also being reduced. How do you deal with results like these? ... You are familiar with what happened when I presented the paper to the Royal Statistical Society...(Robert laughs). You, know everybody was attacking me, ... and basically they were saying "the reason why you get these results is because you are not a good statistician." That's the reason this new idea came to my mind, "Let's do now another competition, let's get more series where now the forecasts will be produced by the best statisticians we can find." So that's what led to the M-Competition.

Q: Did that criticism bother you?

A: Well, of course, if you are young... and I was still young, just starting and Priestley and Durbin (well known names, more like "Gods", or close to that) ... tell you that your work is not good because you are not a good statistician, of course it hurts you! But you know, being a good Greek, I get stubborn and said "Ok, I am going to prove that what you are saying is not correct!". So I organized the M-Competition, where we basically found the same results as in Makridakis and Hibon (1979). Even the person [Allan Andersen] who did the Box–Jenkins modelling admitted: "My method is not better than Simple Exponential Smoothing"; but there was still criticism levelled at M-Competition. This time critics shifted the arguments to "the reason why you find these results is because you carry out the forecasting competitions in a way that is *blind*. The forecasters do not know the series, they don't know what's happening and the factors that are affecting the series. That's the reason why you find these results." That's how I came to do the M2 Competition (Makridakis et al., 1993); it constituted 22 case studies, real data, prepared and presented in real time. And the researchers participating could get information from the companies. We ran this study ... and again we found the same thing! The fact is that even these case studies run on a real-time basis corroborated the previous results; then the last competition was the M3 (Makridakis & Hibon, 2000), where we increased the number of series, we increased the number of methods, we try to deal with all the criticisms that have been raised again us, we did our best. We got experts to do the forecasting... and again the results were the same. I don't know if there is more work to be done on this type of competitions. What has pleased me enormously is that in the last IIF conference [San Antonio] the M-Competition paper was voted as the favourite paper published in the field of forecasting during the last 25 years. For me this was the greatest vindication that my work has become accepted by the majority of academics after all these years.

Q: Another research area is your interest in strategy and its link to forecasting.

A: Budget and Strategic forecasting have developed as a consequence of how one can make forecasting more relevant for business. There has been a lot of work done on the level of forecasting for inventory control. But there has not been a lot of work at a higher level, on budget preparation and on strategy! This, in my view, is a logical evolution for someone who does statistical work... to say: "How can I be more useful to businesses?" And at the same time to try to make a little more money by consulting; because budget/strategic consulting is useful for business at a higher level than inventories. So, I started getting involved with how research can become more relevant for budget and long-term forecasting; how one can foresee the consequences, the changes... cyclical and long-term changes as they affect current strategic decisions. I did some work with several companies and in particular for a paper and a whisky company where their concerns were "the impact of cycles on their revenues and profits" or "how much whisky to put in barrels for 12, 15 and/or 20 years later" That is how can one predict demand in 12, 15, 20, or even 29 years from now?" There are some interesting questions there, and some interesting concerns. Although there is a tremendous amount of uncertainty and risk; these questions are real and must be dealt with by identifying long-term trends

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and changes in the environment and competition. Moreover, one must also deal with the huge [unpredictable] cycles! So, the big challenge is how you can provide useful and relevant information to management; how you can persuade them that our approach/methods can help them improve their decision-making process and their profitability. What is very interesting then, if you look at decision-makers from the judgmental bias point of view, is how much people are influenced by recent events. So, if there is a recession, people think the recession is going to last forever; if there is a boom, they think the boom is going to last forever. For instance, the challenge is to persuade them to do the opposite! In periods of recession, to consider the opportunities that would become present during the forthcoming expansion, and during periods of expansion to start considering the consequences of recessions. There are a lot of interesting themes and maybe at some point, when I get more time... that I would like to write a book about them. How much people are influenced [when making strategic decisions] by what has been happening over the last year or two... or even the last 5 years that from a historical point of view is a short period of time that can be influenced a great deal by cyclical factors.

Q: So, these would be the two core themes that you've explored over your research career?

A: Yes these are the major ones. Another has been to try to identify major forthcoming changes and their impact of a firm's strategy? For example questions of the type "How do I act now to achieve some competitive advantages in the future? This is an even more difficult and more interesting question in terms of strategy. For instance, the way technology is it going to influence business firms. One thing is very clear, 'Revolutions' do not come over a night! They come very slowly and only when you look back can we call them revolutions. For me the challenge is how can you first identify fundamental changes in the long-term trends and their likely influence on organisations and societies; and then, how can you persuade management to start doing something about them. In my mind, there is no doubt that the Internet is having and will continue to have profound effects on every aspect of business! The question is how long is it going to take for these changes to provide firms with profitable opportunities? As a second example, there is no doubt in my mind that we are going to see another period of high inflation like the 70s. And the question is when is it going to start? When we are going to see double-digit inflation again? And I don't think this is unlikely, although people now dismiss it, because we have not seen serious inflation for more than 20 years.

Q: Why do you think this is going to happen?

A: It always goes like that! If you look at economies ... (laughs)... they go from periods of low inflation to periods of high inflation, because at some point you get bottlenecks [in the economy]! And right now China and India and the other developing countries start having increasing demand for oil and metals. Recently, there is no doubt that oil prices have been very low, and if prices are very low there is no motivation for expansion, for discovering new supply sources. Eventually you get into a situation where demand exceeds supply and you get high prices and inflation, and that's how we got double digit inflation in the late 70s and early 80s. I don't think this is something we should forget! If you put in a graph what happened from 1950 to 1973 and what is happening now... you will see that we are very close to the point that inflation is going to grow rapidly! Look already what is happening to the price of oil, they went from ~~$~~10 a barrel to more than ~~$65~~ over a period of less than 5 years. The only question in my mind is not whether we will have high oil prices and inflation but **when** it is going to happen?

Q: Do you think we have learned anything?

A: No, I don't think we learned anything! In the end of the 60s, we were saying... "inflation... forget it, we won't get high inflation, or recessions again!" But double-digit inflation and four recessions, including a major one (1973–1975), took place between 1969 and 1982.

Q: Let's return to the earlier stages of your research career. Who do you think were your intellectual influences?

A: Mmm ... well I don't know, because at INSEAD there were no senior researchers at the time I joined. I don't think that there was anybody who influenced me in terms of my research. The

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director of research at the time was Claude Faucheux who was somebody who supported me, not through collaborating on research but rather, by protecting me and providing me the time and money. The protection and funding was very important for my research so I feel grateful towards him.

Q; Later on then…

A: Later on, you know the group [who founded the Institute and forecasting journals] … basically you (Robert Fildes), Carbone (Bob Carbone), Scott … (Scott Armstrong). Another person who was influential was Robert Hogarth who was teaching at INSEAD at the time, and we did some work that had to do, not directly with forecasting, but with evaluating the accuracy of judgmental forecasts. We published a paper in *Management Science* … "Forecasting and Planning" (Hogarth & Makridakis, 1981a, 1981b). And … (laughs) … again we found some very strange results, for example, the average of S&P500 or the Dow–Jones Index (that is a random selection of stocks) was always better than the selection of professional managers that were experts and some were paid six digit salaries to select stocks judgmentally! Which came back to the same idea: very simple models… like the Random Walk (what we call the Naïve 1) that is based on today's price being the best forecast for the future, was beating professional fund managers. Not all of them of course, but the great majority.

Q: Do you have a favourite forecasting book or a science book in general?

A: For me, Thomas Kuhn's *The Structure of Scientific Revolutions*… (laughs) … maybe because I have to a certain extent lived the experience of what he is saying.

Q: You are one of the fathers of the idea that: "A perfect fit does not guarantee an accurate forecast." That seems to be one of the dividing lines separating theoretical statisticians from forecasters. Two parts to the question; where did this idea arise? What does distinguish forecasting from the other more theoretical statistical areas?

A: There has been a tremendous amount of amazing theoretical work as to how you can fit a better a model to time series or cross sectional data, e.g., various types of regression; trying to develop different methods to achieve a better fit to available, historical data. From a statistical point of view, it's clearly interesting work and scientifically correct, and, no doubt, it advances statistical knowledge … But this assumes that history repeats itself! And we know very well from the reality of working with real data, that this is rarely the case! In the future you always get something that is different than in the past. Things that could not be anticipated. This is unavoidable! I am not talking about randomness, which is also a very important component of our data… (Spyros laughs) … the big problem is that there are always new things, unexpected events, structural changes and so on that cannot be predicted from past patterns or relationships. In my view that's where the problem lies, and what is surprising is that many statisticians have taken so long to recognise that! That is what we were saying very strongly in the review paper that we published a decade ago (Fildes & Makridakis, 1995). As you remember we had again problems getting it published because mainstream statisticians were not willing to accept the arguments [despite the empirical evidence] that the future will always be different than the past and that even perfect fitting would not result in more accurate forecast. That is the reason that simple methods do very well, and combining improves on the forecasting accuracy of individual methods. This is because it's averaging the errors. It leads to forecasts that are often better than the 'best' individual method.

*Q: I obviously accept what you say. There always seems to me to be an issue concerned with the fact that, in a way all we forecasters have done is replaced '**fitting**' with '**forecasting**'. Our selection model is: "What did forecast well – it will forecast well!"*

A: Well, our model is: "Let's not fit too well so that we can forecast better. Let's fit in a rough way so that we can forecast better;" Then we can discover the ~~basic~~ direction or the basic pattern, rather than over-fitting the past data. By overdoing the fitting part we do so to the detriment of higher forecasting accuracy. So that's the essence of saying: "I have my simple method". What a simple method does: it stays very close to the basic pattern. What a sophisticated method does? It tries to identify a pattern by separating it from random noise. But

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often the pattern being identified does not remain consistent in the future; it changes, increasing the errors during the forecasting phase. We know that with some data, like those used by Newbold and Granger (1974), sophisticated methods proved more accurate; we have found some of these data too in our competitions. But whatever differences exist are small and not consistent over time, in particular when big cyclical or other changes take place.

Q: There is always this issue as to how close should we stay to the optimal fit?

A: Well, what is an optimal fit? It's a big question mark. Because in addition to underfitting there is also overfitting. Overfitting is as bad as underfitting! So, maybe the direction of theoretical work should be to try find the best fit that is going to maximise the forecasting accuracy, rather than that of the model fit! That could be a substantial theoretical contribution.[1] Let us have criteria to be able to determine the "optimal" fit (not underfitting but also not overfitting available data). What we know is that even if you have a well-behaved series, or it's an aggregate series where there is not much randomness, simple methods can do pretty well in comparison to the method that best fits available data. Simple methods can also identify patterns while not overfitting. Sophisticated methods, on the other hand, do not predict well series that contains a lot of randomness or structural changes.

Q: Of your numerous publications, which is your favourite?

A: My favourite publication is the M-competition (Makridakis et al., 1982). It's obvious that this had the highest impact of all of my publications. While the publications I have done in *Management Science* might be academically more rewarding, but the impact has been less (apart from Forecasting and Planning with Robin Hogarth). Of the books I like a lot "Forecasting, Planning and Strategy " (1990) which has not sold as many copies as my other books but it is intellectually pitched at a higher level; and more rewarding... some of the reviews have been very encouraging.

Q: You started your career in MIS. How much has the IT revolution helped in improving forecasting?

A: Well, I think that IT is going to improve forecasting. I think we haven't yet seen the benefits or improvements in forecasting accuracy [arising from IT developments]. In my view, what we are going to need to do is try to take this old idea of Lewandowski (1982) of including special events and special actions; and trying to remove them from the data. Now we have the possibilities of doing that, now that memory and storage is not a problem. Ten years ago methods were constrained in terms of how much data and information could be stored. Now you can keep data on anything you want, in any way you want, we can then go back and extract useful information to forecast more accurately. We have to start keeping multiple dimensions of information concerning what happens to our series. How specific advertising or promotional campaigns affect demand under the specific circumstances they were done? Or how price increases/decreases influence profitability? Regression models don't always work because they attempt to identify "averages". However; there are so many factors involved that you can not isolate them and pick up their **individual** influence through regression. So you have to find out some other ways to single out their impact. I think what Lewandowski was trying to do was impossible at that time but maybe it's now achievable.

Q: Shifting track, we [Robert and Spyros] met in Greece in the late 70s. What prompted you to found the Institute and Journal of Forecasting?

A: Basically the success of "*Forecasting Methods for Management*". In the middle 70s forecasting did not exist as a field, making it difficult to publish such a book. Academics and editors were saying: "What field is this?" We were lucky to find a publisher [in Wiley] and an editor who was open-minded. So seeing the book's success, and then visiting companies and seeing their great interest in forecasting, came the idea... "Let's try to do something more, let's create a new field!" And I think to a great extent we succeeded; we are getting a good number of people at the annual Symposia. Other journals have developed in addition to *Journal of Forecasting*,... the *International Journal of Forecasting*, the *Journal of Business*

[1] Some approaches to neural networks and data mining combine fit with validation statistics to select an appropriate model.

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Forecasting. There are many books on forecasting being published. So the field has been created. But new developments in the field have slowed down. Some new breakthrough is needed. The direction must be to try to make forecasting more relevant and practical, to get away from model fitting and attempt to optimize post-sample forecasting accuracy.

Q: The Institute, IJF, JoF in many ways have succeeded beyond your expectations. But from my [Robert's] perspective there has been a fragmentation in the field. From Philadelphia attracting 1000 people to Sydney and a very worthy 350. And that seems the number we can reasonably expect from now on. Any thoughts about the decline?

A: As I said before I think it's time for some breakthrough. We need to find some new ideas, become more useful and relevant to practitioners as well as attract new people, like Kostas, by availing ourselves of new opportunities that would raise forecasting from its current stagnation to something new and exciting. What do we offer right now? We offer some methods and we say: "If you want to do well, here are the simple methods and if you have doubts identifying the best method, try combining." Ok, you do not need much expertise in order to follow this advice, the opportunities for consulting are limited, right? However, if we could come up with something that is going to improve on that, then we are going to generate again a lot of interest. This is what we were doing in Philadelphia (1983) and Quebec (1985). At that time we were doing something that was new and useful! We were integrating the field of forecasting and providing some useful, novel advice to practitioners. Then the newness and utility dissipated ... and we've been trying to live on the same intellectual capital for more than 25 years! What have we added in the last 25 years to make the field more useful and relevant for practitioners? In my view, not much.

Q: On the same theme I (Robert) gather from Kostas...there is a Greek saying on choosing research collaborators: "If you have to do something correctly, you would better do it by yourself"; Sparing my blushes and libel suits how did you choose your collaborators?

A: Well ... (laughs), I guess part of it is instinct, ... because at this time I did not know you (Robert) well, I did not know Scott or Carbone. But given the remaining choices it was a good choice, and in retrospective we made an excellent team; although we had different interests and we went in different directions. In the end, we created the field! From my point of view... I had a book for practitioners and a textbook at that time, then we created the Institute, and two journals. That was a complete package. And I think you, (Robert) were the right person at the time, and Scott was the right person at the time and Carbone, also all four of us contributed a lot to the field each in his own way.

Q: (Robert) Obviously we had our agreements and differences over the years about the direction of the field. You, me, Scott, Michele Hibon of course... What makes a successful collaboration for you?

A: Well, the end result ... the ultimate success, right? Now, how do you measure success? You measure success in terms of publications... how many papers you get for the journal, how many people come to ISF. Maybe we could have succeeded more – but given the resources and given the time that we spent... I think we have succeeded! At least that's the way I see it. We have created enough interest in the field, right? You can see new people using our research, and quoting us and asking questions... you cannot expect more than that... Because you cannot change the old guard, it is the new people who come in the field of forecasting without preconceptions who will move the field in the right direction. I think this is happening, the field is moving forward and I do hope that some breakthrough will come sooner rather than later!!!

Q: (Robert laughs) Back to those early years. What was the key event?

A: Well the key event was definitely the conferences and the journal; the two journals. And you know, I regret we had this fight with Wiley and... we should have been more political and not forced the break from the original journal publisher. That was a big mistake. We were young at this time... and opinionated!

Q: (Robert) I think in retrospect having two journals benefits the field, even if we did achieve this by accident (laughs)...

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A: Maybe, but look at the *Strategic Management Journal*, it has become more established because it is a single journal.

Q: You retired from INSEAD about 2000. . . was it?

A: Well, I did not retire, I am actually semi-retired. I have been teaching since 1968 so was I "semi-retired" in 1998; I had 30 years of teaching and research. I think 30 years is a long time to do one job. I wanted a change. So I took this as an opportunity and I said "look I need a big change in my life". And part of the decision was also that I wanted to raise my children in Greece; and it has turned out well.

Q: What do you miss about research?

A: Well, the intellectual stimulation; this is what's great about research, and the ability or rather the luxury to do nothing and just think (and of course get paid for that)! This is a very important luxury that you don't realise until you leave it. In business you end with a lot of things to do and no time to think. Looking back, I am not sure if it is better to be in business or be an academic. . . So, I miss doing research! I would like to go back and do more research; not maybe the technical part, but some reflective research. What I think I would like to do now is write a book about forecasting, but not technical forecasting; the topic would be long-term forecasting and how one can deal with change, combining forecasting with strategy.

Q: What about teaching. Was that an important part of the job?

A: Well, teaching is interesting at the start, but when you have done for the 20th or 30th time; it's not as exciting. And what becomes exciting is delivering special types of teaching: seminars; talks in companies or conferences; trying to be provocative, trying to be original. Trying to get people to change their mentality through your teaching. This is what's interesting for me to teach, rather than a course in statistics or forecasting, where themes do not change.

Q: Let's look at how much your earlier forecasting ideas now influence your business thinking? How often are your forecasting ideas relevant in your business life?

A: Not much, not much! Very often you think that people in business are rational and they look at the future; then you realise that their motivation and the way they make decisions depends on many personal and political factors rather than careful consideration of all factors involved and a emphasis on strategy! (looking disappointed)

Q: So, as a chairman, are you in position to make things happen?

A: You are in position to influence things. But the influence is relative always. The way that decisions are made. . .very fast, with very little time to think of the future consequences.

Q: Yet, you are operating to typical high-tech companies, aren't you?

A: Well, the ideas. . . which direction to go or what to do, what not to do. . . But in a fast moving company, you can see the decisions are made without the luxury of thinking what is going to happen.

*Q: You wrote that: "A major reason for the criticism and discontent facing the field of forecasting has been wrong expectations from the users" (*Makridakis, Wheelwright, & Hyndman, 1998*). Now you are a user, what could you reasonably expect from forecasts?*

A: Well, the users expect that you provide them with something like a crystal ball; you can see the future and help them. What I was trying to say in these articles is to try to talk to the users of forecasting about the uncertainty of the future and what they can do to deal with this uncertainty. However, people are afraid of the uncertainty; they want to reduce it. The misconceived expectation is that forecasters help to reduce uncertainty by providing more accurate or precise forecasts, while a good forecaster should increase the user's uncertainty!

Q: So, now that you are a chairman, how do you deal with uncertainty?

A: Basically trying to have alternative plans when something does not go the way it is supposed to go. Not relying only on one path in case it is not successful. Most of the times companies fail [in achieving their strategic goals]. Thus, what can be done if things go wrong becomes very important? This is not easy, because even in a company where you are a chairman, people think that their understanding of the future is going to be correct. They find themselves threatened if you tell them that: "most of the times plans do not succeed. . ." Often companies fail for reasons that are outside their control! So, it is important to try to be more

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open-minded; to be prepared to do something to minimise the risk, if things do not go as we expect them.

Q: What were the [intellectual] barriers that were the hardest to solve? Things that stopped you moving ahead?

A: People with different background accepting the empirical findings. And I am talking specifically about a group of people... the more theoretical statisticians; who as good scientists you would have expected to give credit to our empirical findings [from the M-competitions]; they simply rejected them. So that's the biggest shock out of this. We are supposed to be a scientific community... Why object to something that is correct beyond reasonable doubt? But also I must admit that there have also been several prominent exceptions like Keith Ord and Chris Chatfield who after a while have come to accept the results of the competitions and our thinking.

Q: Irregular events or shocks; how close are we today to being able to make progress in dealing or even forecasting such events?

A: Well the only thing one can do is to try make analogies. Like now that the price of oil is going up, if you take the last time that the price of oil was going up, and try to see what's the appropriate analogy; how much more can it can go up? If you plot these two series together, you can see that there is another 40% increase is possible compared to the 1973 peak. So it won't be surprising if prices rise to $90 or $100. But if it is the first time... then there is no way to predict it, right? Apart from trying scenarios that also have their own problems!!

Q: You are not optimistic...

A: About predicting the unexpected? You cannot predict the unexpected, right?

Q: If you had to nominate three major advances in business forecasting in the last 25 years what would they be...? Not just you, but everyone. We had our Nobel prize winners!

A: Maybe what I will say comes from my own biases and experiences... First let's put forward the empirical competitions: "Simple methods do as well or better than sophisticated ones". We cannot predict well because the series have structural changes. The second [advance] is the idea of combining. The third is that although methods do not forecast accurately, people do worse than methods! Human judgment is worse than our simple methods or combinations. It is full of biases that result in highly inaccurate forecasts.

Q: Any big ideas out there still to be explored?

A: Using the power of the computer to make a breakthrough in the way we analyse data and use such an analysis to improve forecasting. Maybe we have to forget completely about model fitting, and the idea of using averages, and to concentrate on forecasting specific, individual cases by looking at similar past situations not just their average as we currently do. Another example is how we can get more information from our data; for instance an outlier can provide us with a lot of information that we now ignore or throw away because we have this obsession that outliers reduce the value of R-square.

Q: Anything else?

A: Try to separate forecasting in terms of short, mid and long term. Try to develop different approaches for these three different kinds of forecasting. I think all three are extremely important. On the one hand, we see big structural changes, and on the other hand there are fantastic similarities. If you analyze the behaviour in the big crash of DJIA in 1929 and what happened to the Nasdaq during the dot.com collapse in the early 2000s... and put these two series together, in a standardized form, one next to the other you discover a phenomenal similarity. Maybe there are more analogies like these that we can identify to help us predict the future more accurately...

Q: What is next in your career? You mentioned already about a new book...

A: This book would express my knowledge and experience in forecasting to help people face the future in a more realistic and rational manner. Most books deal with success stories, few people are interested in failures. Thus a book explaining reality as it is (with both its successes and failures) and setting the limits to our predictability would be an interesting and useful one.

Q: It is common for Greeks to use epilogues or epigrams when authoring. You avoid these in your

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best seller. Can you offer us one now to capture the future of forecasting?

A: (laughs)... After having thought a great deal on this question, my epigram is:

"**The Future is not what it used to be**"

May be if we accept this epigram as the foundation of forecasting we could come up with the breakthrough that we need to raise the field to a new level and make it more useful and relevant.

Q: OK, Thanks for this interview, Spyros

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