DISCUSSION

ARI JANTUNEN and LIISA-MAIJA SAINIO

Sidney G. Winter on strategic management research: Insights from the 2008 Viipuri Prize Winner

On May 14th 2008, The Viipuri Prize, established by the Society for Viipuri School of Economics, was awarded to Professor Sidney G. Winter at the Lappeenranta University of Technology[1]. The Prize, 10 000 euros, is given every second year to a scholar who is among the most prominent in his or her field of study on a global scale, and has

1 The Viipuri Award ceremony was held in association with the Doctoral Promotion at Lappeenranta University of Technology. Professor Winter received an Honorary Doctorate (School of Business) for his outstanding academic achievements. Sidney G. Winter is Deloitte and Touche Professor of Management at the Wharton School of the University of Pennsylvania. After receiving his Master's degree in 1957 and PhD in 1964 at the Yale University, he has worked on public policy issues as a Research Economist at the RAND corporation and later as Chief Economist of the U.S. General Accounting Office. Before Wharton he has held a professorship of economics and management at Yale and has also taught at the University of California, Berkeley and at the University of Michigan.

The publications of Sidney Winter cover research issues and areas especially in the fields of organizational knowledge and co-evolution of firms and industries. His well-known and very widely cited book An Evolutionary Theory of Economic Change, co-authored with Richard Nelson (Nelson & Winter 1982), explains how firms and industries change over time through an evolutionary process. A central theme in the scientific production of Sidney Winter is the nature and use of knowledge in an organization. Where the knowledge resides in an organization, how it is used in production and decision-making and the essence and roles of organizational routines and capabilities – these are some of the focal research issues of Sidney Winter.

Sidney Winter has his background in economics but his research interests have covered a large spectrum of issues from decision-making rules to the dynamics of the techno-economic development of industries. Professor Winter approaches the strategy and management issues from the viewpoint of evolutionary economics taking realism as his starting point in the analysis when opening up the dynamics of technological and economic change and demonstrating the challenges that firms meet when trying to cope with change. His contribution has had an impact especially on strategic management research and organization theory.

ARI JANTUNEN, Professor
Lappeenranta University of Technology, School of Business, Strategy Research • e-mail: ari.jantunen@lut.fi
LIISA-MAIJA SAINIO, Senior Lecturer
Lappeenranta University of Technology, School of Business, International Marketing
• e-mail: liisa-maija.sainio@lut.fi

also had a great impact on the research carried out at LUT School of Business. During the award ceremony Professor Winter gave a lecture on the topic *The Nearsighted Watchmakers: Understanding Evolutionary Progress in Technology and Organization*.

Nearsighted Watchmakers and Rigor and Relevance of Research

In his Viipuri Lecture, Professor Winter explained the background of the half-century development of evolutionary theory in strategic management, and its points of departure from the original Darwinian theory. The evolutionary progress in biology is blind, unconscious and automatic, taking place without any element of intentionality. Therefore, if there is a designer or a creator, namely, the *watchmaker*, he must be blind. However, managers and engineers can intentionally manipulate things in organizations to produce novelty, and seek profit in doing so. To a limited extent, they can test their proposals to see if they work locally. However, they cannot reliably predict the consequences of efforts in large-scale implementation; hence, they are nearsighted watchmakers. Therefore, variety is a crucial factor in evolutionary processes, shaped by intentional activities, experimentation and learning.

In his presentation, Professor Winter used a metaphor of cardboard with pins and thumbtacks to describe the structure of knowledge and the problem of rigor and relevance in research. The cardboard referred to the problems of reality, whereas the pins and thumbtacks represented the extent of knowledge of that domain, the height showing the strength of knowledge. There are clusters of pins and sticks of varying heights – little sporadic towers of reasonably strong knowledge – in the large field of ignorance. Scientific rigor leads to higher towers, but the challenge of relevance is that the problems of the real world do not necessarily conveniently place themselves exactly where the towers of strong knowledge are. Instead they frequently embrace large areas, or even fall into places where there are no towers at all. Therefore, intelligent and effective decision-makers try to detect where the towers are and construct shaky little strands and foot bridges at high levels among the individual towers in order to hook up available knowledge, so that it would be more relevant to the actual problem. It is easy to be in favor of rigor, in the sense of "*fewer weak structures in the towers, please*." However, so much of the real action is not about the height of the tower, but the quality of the little foot bridges that decision-makers have to construct. And that is an art form – the art of management. Professor Winter summarized the issue by saying that "*the hard part of the rigor and relevance problem is, how you advance the art of practical decision-making, given that it so often must draw on something vague called judgment and general intelligence, to hook up the existing knowledge in a useful way?"*

Interview with Sidney Winter

Question: If you look back, what kinds of insights or research findings have been the most important ones?

SW: There are a number of things that actually happened after the publication of the 1982 book. One is the research in psychology, but which was introduced into this discussion, primarily by Michael Cohen, on the basis of skilled habitual behavior. In trying to construct an alternative story about effectiveness in the world, we had come to the view that the choice and deliberation oriented scheme of standard economics was a very incomplete rendering of what made for effectiveness in the world, and that skill was an alternative model of what made for effectiveness in the world, and we built that idea into the book; first at the individual level, and then at the organizational level. But we really did not have any idea at the time that we had a strong physiological basis for our views, and in fact, the human brain is organized in a way that is different for what is called skill memory or procedural memory than declarative memory. So Cohen led the way in bringing that into the discussion in the simple experimental scheme in his 1994 paper with Bacdayan[2]. When the psychological evidence referenced in that article came to our attention, we knew that we had a stronger foundation than we had really imagined.

Another important thing was the industry evolution angle, which we had really seriously missed in our book. We had not really featured the point that the really vivid examples of the evolutionary struggle in the economic realm come in the early years of industries, when you have a lot of variety and when the positions are not yet established. When that point was put forward, primarily by Steven Klepper who pointed out the way these historical developments typically take place, it was a huge insight and a huge contribution to the credibility of the overall program[3].

To add one more, my colleague Daniel Levinthal introduced the so-called NK modeling technique into organization theory[4]. And that, although not empirical, provided us with another thing that was very badly missing, which was an account of how variety originated. So the NK modeling system, especially as revealed in that article, tells about how variety naturally arises as a consequence of path-dependent learning and systems, where correct answers to questions cannot be laid down in advance.

With those three contributions, we acquired one very strong reinforcement of a position we'd already taken, and two vastly important supplements to an existing stand, and these, in my view, just made a tremendous contribution to its credibility.

Question: How do you see the relationship between economics and research on strategic management? What do they have to give to each other?

SW: I think there is a great deal more to strategic management research than economics can ever provide. But, you would hope in principle that the economics that did feed into strategic management research was appropriately directed to the problems of strategic management. And, I think, abstracting from the part of the contribution the evolutionary economics has made there, the story is kind of disappointing: a lot of the economics that was fed into strategic management research was not very appropriate to the domain of application. Economics, in its microeconomic portion, seeks to understand pricing and allocation in the system as a whole. That is its basic task. And the assumptions that it makes in the interest of getting on with that work, are not appropriate assumptions for an inquiry into who prospers and who does not prosper in the process. They, quite reasonably, at one level, abstract from crucial ques-

2 Cohen and Bacdayan (1994)
3 Klepper and Graddy (1990), Klepper (1996)
4 Levinthal (1997)

tions that are relevant to the question of who prospers and who does not prosper. And so, in my view, the ordinary mainstream foundations of strategy are as yet poorly articulated and there is an important contribution still to be made in making that connection. And that is only the start, because then I would say that all the evolutionary qualifications to that story would have to be acknowledged as well. The strategic management subject obviously has a very large economic component, but so far, in terms of the contribution, I would say that the surface has barely been scratched.

Question: If you were now in the beginning of your doctoral studies and trying to choose the topic for your dissertation, what topic would you take? What are the most relevant or most interesting issues in the evolutionary economics framework, or in strategic management, from your perspective?

SW: That's a very difficult question to answer, because I see a big spectrum of possibilities there. So, there is opportunity, for example, if you want to go off in the direction pioneered by Cohen, to go off into the psychological and neurological foundations of the relevant decision-making model. But, I think that one of the problems that is, so to speak, sitting there on the table, and has been sitting there for a while, is to understand the historical dynamics that form industries from a wealth creation point of view. I think it is a little startling that in strategic management the orientation to wealth creation is as feeble as it is. We don't seem to be very interested in the question of who it was that walked away from the episode with the great fortune and why did that happen. I think that is a huge area of opportunity.

Question: Routines are one central construct of your work. In the context of the university as a research organization, what routines would you consider to be the most important ones?

SW: Let me back off a long way from the question. This is a point of difference between myself and former Viipuri Prize winner Jim March. Jim March has famously written on universities, in a couple of different ways, about presidents, and also on the so-called garbage can model. My view of that is that when Jim looks at a university as an organization, if you judge by the kinds of things he's focused on, he is looking at the faculty. The people that I see through my lens are these people who are departmental administrators. They are the non-academic top staff people, who stay in those positions for long periods of time. Chairmen of departments come and go, and those people stay. And those people make everything happen, of the crucial practical kind. They essentially assure the reproduction of the behaviors that were they not reproduced would be a notoriously embarrassing signal that the institution was not doing its job. So, there is that very basic level performance that you get in a university, and those particular people in the structure, are essentially the custodians of the fundamental routines that reproduce those performances. Now, you could argue that everything that is interesting to us is somewhere else than that, except in the cases when you're in an organization where that is not being competently done. That's the usual story with routines. As long as it's competently done, it easily fades into the background.

Next, when getting closer to our domains of interest, it's clearly the promotion system. It's the recruiting, and the promotion, that is fundamentally shaping these institutions, and what they do in the long run, and so that's where you should look. For example the practices of promotion to tenure seem to do inadequate justice, sometimes to the candidate, and more fundamentally to the scientific issues that are raised by the candidate's work: Is that good work, is that not good work? Why do we know? How do we decide? I always feel frustrated by the shortness of the discussions of those questions.

Question: In biology, evolutionary theories are better in explaining what has happened than predicting the future. How do you see it in evolutionary theories in social sciences, do we have possibilities to predict the future? Or will it be possible some day?

SW: The overwhelming lesson, of the entire twentieth century, in science and the philosophy of science, is to tell you that you should not have great confidence in your ability to predict the future. And, this goes even for the most well established theories, as well as for more preliminary attempts, such as we have in economics. So, if you really mean predicting the future in any kind of position, then I think I would say, you need to look at what exactly happened in the twentieth century. To take one example of that history, the understanding of non-linear dynamics, chaos theory and so on, gives us a perspective, where we understand that seemingly reliable laws can have transition phases and go into some different mode entirely, and you can do that with the simplest kinds of dynamic equations. It's a change of mindset to understand that something that looks like so reliable, can fail, and it fails for its own reasons, not for external reasons. That's just one example of the things that undercut any confidence in your ability to predict.

That said… this gives me an opportunity to comment on this point about the role of routines from the evolutionary theory, which is not entirely accurately appreciated. I would still take the stand that if you want to know what the organization is going to do next year, you should examine carefully its existing routines, and assume that these routines will not change. I don't think that it's a uniformly reliable prediction, that the routines won't change, nor would I recommend to an organization that it should not change its routines. But, on the basic prediction question, I think, you can observe the past, as a record, you can ask what explains the way they behaved in the past, and you can extrapolate into the future on the basis of that record. I think that is a pretty strong scientific approach to the prediction task, although I don't expect it to be uniformly successful.

Question: So, do you think that prediction should be the goal that we should try to strive for? It seems that all the companies want the university researchers to examine what the future will be like, and how the companies could be prepared. Basically they are looking for a crystal ball. From their perspective, that would be relevant. Are they asking for the impossible?

SW: Well, I think that is true. From a scientific point of view, I understand the term prediction to mean a statement with testable content with respect to future observation. That means that prediction includes a statement to say "the following kind of constellation of things will never happen." So, that is a very long way from the kind of point prediction that the companies or other decision makers would like. It says: here are the logically possible alternatives; these are off the table, that's the claim of my theory; that will never happen. Now, that is a prediction from the scientific point of view. It has content and it could be wrong. And I think from the scientific point of view, a lot of the valuable predictions that we can make are essentially of that character. So, I do agree at some deep level with essentially a Popperian view of the philosophy of science that says what it's about is making statements that could be wrong, and having them turn out not to be wrong. That's what it's about, statements with content, statements that are checkable. This means that there might be a lot of predictions that are scientifically valuable and strong, but are not of much use to decision makers. That brings in another point, which is quite appropriate from a strictly academic science point of view: attention goes to the areas where science can be practiced in an effective way. So, my example is the fruit fly principle; if you study fruit flies in order to understand genetics, and that's the best way to understand genetics, you are right to study fruit flies. That's where you can practice science. Sooner or later the useful practical understanding may follow, but not right away.

Question: What would be an ideal organization for you at the moment to study?

SW: I have this belief that social science and human intelligence could be applied vastly more effectively to organizational functioning. I'd like to look at organizations which are, in some sense, on the frontier of the practice of deploying intelligence effectively in their decision- making. An example, which comes to mind, related to this credit crunch problem is that Goldman Sachs somehow managed to bail out of this problem, noticeably earlier than others, and in fact, I think they overall profited from the episode. The question is why was that? Was that luck? I doubt it. I think that was something about the organization, something about its culture, something about its management, and I think it would be very interesting to understand why.

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