

Discussion of Malerba et al., Ozcan, and Reichstein et al.

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The paper by Malerba et al.

Franco Malerba, Richard R. Nelson, Luigi Orsenigo and Sidney G. Winter, Changing boundaries of firms in the evolution of two vertically related industries: towards a history friendly model, version: 31 March 2005.

The paper by Malerba, Nelson, Orsenigo and Winter is part of an important and long-term project on history-friendly modelling. It is not by chance that the first output—some years ago—of this project concerned the computer industry. When industrial economics emerged as a discipline in the 1950s, the automobile industry was the benchmark case, but this role has now been overtaken by the computer industry and related industries. The 1999 paper presented a simulation model developed in relation to the history about with mainframes, minis and PCs—so the basic Nelson–Winter model had to be extended to handle shifting demand and computers had to be characterised both by cheapness and performance. In the present paper, another side of the history of the computer industry is confronted. It is the production of hard components, operation systems, and specialised software. This story could be told in terms of the individual elements—like a model of the evolution of the software industry—but the paper emphasises continuity with the 1999 paper, so we are still dealing with the evolution of the industry producing computers, but now is added the integration or disintegration of the production of the necessary hardware parts and computer programs. Thus we may think in terms of IBM's early integration of everything and the present coexistence of IBM, Intel and Microsoft.

To model and simulate this vertical history, a market for a single component augments the previous model. The demand partly comes from computer firms that have outsourced their production of components, but there is also an exogenous demand from other industries and from the military. This exogenous demand is not historically incorrect, but from a

modelling point of view it largely seems as a trick that helps the producers of components to survive in their instable market. The production of components is undergoing technological progress of the Nelson–Winter type, but the quality of components that are produced in-house is larger than that of purchased components. Thus component firms need to obtain a higher quality through additional R&D efforts. However, they can deliver to several computer producers, and this specialisation gives them advantages. This set-up of the model determines the occasional make-or-buy decisions of the computer producers: If the computer firm is small, it tends to specialise in computers, but as it grows larger, the probability of vertical integration increases.

The paper uses its two-industry model to clarify the discussion of vertical integration and disintegration between the transaction cost approach and the division-of-labour approach—and it supports the latter, but this approach is changed to a division-of-innovative-effort approach that also emphasises complementarities. The study of two industries is the simplest environment for developing this approach, but maybe the authors should emphasise that the model is far from the multiplicity of ‘components’ emerging in relation to the real production of computers. Such an extension would demonstrate the enormous pressure on highly integrated computer forms to specialise since they cannot obtain high component quality in all areas. The extension would also emphasise issues of how to design the simulation model. The present model has upheld the complexities in the demand for computers, but if this part of the model was simplified, it would be easier to handle the additional complexity of introducing several component markets. But even in the present version, it is not easy to analyse the results of the simulation exercises. They could be presented more systematically if the model was somewhat simpler, but the best solution is probably in the paper to include a web link to the program and allow critical and competent readers to check it out. Furthermore, the addition of new descriptive statistics on the simulation runs would probably be helpful. For instance, I would like to know something about the instability of the component market, and here even the extremely simple Hymer–Pashigan

instability index would be helpful. I would also like to see statistics based on George Price's formula, and I guess that the result would be that the innovation effect is much larger than the selection effect. The question is whether this really is a description of the history of the computer industry?

To conclude, it is very important that the authors continue their long-term project. They provide us with a way of confronting some of the many and crucial complexities of evolutionary industrial dynamics, but I miss a development of the measures of quantitative evolutionary economics. Maybe such an emphasis would help to persuade more PhD students to enter the field of history-friendly modelling.

The paper by Ozcan

Serden Ozcan, Examining slack–innovation relationship: longitudinal evidence from US farm equipment industry (1966-2000), version: May 2005.

The paper by Ozcan indirectly related to what might be the very first set of exercises in history-friendly modelling and simulation. Herbert Simon became a pioneer of Artificial Intelligence computing because he wanted to understand actual decision-making in organisations, and in that connection he sharpened his ideas of satisficing behaviour. This was really a theory of search and innovation under pressure in accordance with the old proverb that necessity is the mother of invention. Cyert and March tried to use this theory in their more-or-less history-friendly simulation models of organisational dynamics, but they quickly recognised that it is both necessity and slack that are the mothers of invention and innovation. Later, their results were used in the literature on organisational change, and Ozcan gives us a thorough and thoughtful review. He demonstrates that the issue of the relationship between slack and innovation tended to become isolated, and he does a fine job in demonstrating that a more complicated model is needed to handle the issue in a convincing way. This model specified different kinds of slack, of which only some are available for performing the search and innovation that is triggered by unsatisfactory performance. He also emphasises that this model is not covering the more pedestrian forms of

innovation for which the pressure on the organisation by a low level of slack might be more helpful.

My way of describing the theoretical core of Ozcan's paper is not identical to his own, but maybe he might improve the effectiveness of the argument by dwelling a little on the initial contribution of Cyert and March and its origin in the work of Simon. He might also add arguments on the relevance of choosing patenting in 11 firms of the US farm equipment industry to test the extended slack–innovation model. It seems to be a pretty stable industry and the data covers 35 years during which there has been time for both slack accumulation and slack decumulation, so it fits well into his general story.

The paper by Reichstein et al.

Toke Reichstein, Ammon J. Salter and David M. Gann, Break on through: sources and determinants of product and process innovation among UK construction firms.

My comments on the paper by Reichstein, Salter and Gann will take their starting point in the fact that I started my research career by studying the industrialisation of the Danish construction industry in the 1950s and 1960s. This industrialisation was a huge attempt of a traditional industry to 'break on through to the other side'. Unfortunately, Danish construction is still hardly more innovative than the British one, but it should be remembered that a large number of activities were outsourced to the supplying industries in which they became much more innovative. Furthermore, it was clear that many innovations took place at the systemic level so that they were hardly recognised as innovations by individual firms. Even at the level of individual firms many innovations were hardly recognised because firms were so flexible that they could not distinguish between innovation and trivial adaptation to a new building site. Finally, it was clear that flexible regulators—and in particular the flexibility of the leading civil servant—were crucial. Without the change in the rules many of the innovations would have been impossible—like when the strength of a cement wall could only be measured in terms of bricks!

I mention these issues because I sense that there is a certain conflict between the paper's theory and its empirical evidence and maybe even among the authors. To resolve these conflicts, it is useful to note that it is only the top of the iceberg of innovation in relation to construction that is perceived by individual construction firms. When we add that we only see the parts of this perception that is recorded by the innovation survey that is not adopted to the peculiarities of the construction industry, some of the conflicts should be resolved, but maybe this reconciliation should be stated more explicitly in the paper. In any case, it is very important to use the available data on innovation in the construction industry, and these data seems to be treated professionally and they yield plausible results. So the first task is obviously to finalise the paper. However, my own conclusion of my early studies of the innovative performance of construction industry was to overall study of its overall processes of system-level transformation as well as firm-level innovation and diffusion. To use the concepts that we now possess, I wanted a history-friendly model of the evolution of the construction industry and an empirical analysis of available data on the change in the characteristics of the industry in terms of, for instance, Price's equation.