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Knowledge about knowledge since Nelson & Winter: a mixed record*

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Summary

Progress in our understanding of the role of knowledge in the economy, based on Nelson and Winter's book published in 1982, has been mixed. It has been greatest when their concepts have been enriched by empirical evidence, often coming from outside evolutionary economics. It has been least when discussions have been mainly theoretical, and constrained within evolutionary economics.

The development and application of advances in knowledge have always been of central concern to scholars of economic development and change (Loasby, 1998; Smith, 1776; Toqueville, 1840). The purpose of this paper is to assess the progress (and lack of it), since the path-breaking book by Nelson and Winter (1982), in our understanding of the nature, the sources and the consequences of the processes that generate and apply the knowledge that underlies technical change. Reflecting its author's professional deformation, it will be only incidentally concerned with the implications of the advances in such understanding for orthodox economics. Instead, I shall assess progress in terms of improvements in empirical understanding (both description and explanation), and of usefulness to policymakers (public and private).

I shall argue that progress has been uneven: greatest, when the concepts developed by Nelson and Winter have been confronted by a rich body of empirical material, often emerging from traditions outside evolutionary economics; least, when they have been constrained to mainly theoretical debates and developments. This would come as no surprise to the authors themselves. The richness of their own analysis emerged from their development of the concepts of Schumpeter and Simon, in order to understand an observed corporate world of change, complexity and uncertainty, largely at variance with the prevailing assumptions of economic orthodoxy.

1. Two precursors: knowledge in production and in trade

Significant empirical challenges to orthodox assumptions began to emerge well before the appearance of Nelson and Winter's book. Beginning in the 1950s, growth accounting exercises using the aggregate production function were left with a large residual unexplained by the growth of labour and capital. This was often attributed to "technical advance" (Solow, 1957). However, many of the related assumptions were implausible, such as the prior existence of a commonly available stock of knowledge, bits of which are drawn upon when relative factor prices are right (Rosenberg, 1976). And how this stock of knowledge is created is left exogenous, in spite of overwhelming evidence that much inventive and innovative activity is endogenous (Freeman, 1974; Schmookler, 1966)

More convincing - but now increasingly neglected – was the so-called neo-technology explanation of patterns of international trade[1]. This emerged in the 1950s from the "Leontieff Paradox" where the prevailing theory – that the capital-rich and labour-scarce USA should be relatively strong in international trade in capital intensive industries – was contradicted by the facts. Consequently, some writers predicted that the US should be relatively strong in R & D intensive industries rather than capital-intensive ones, and this turned out to be the case. Subsequent analysis confirmed that international differences in technological activities were more powerful predictors of OECD trade performance over a wide range of manufacturing sectors than the relative abundance of labour and capital. Crucial to such explanations is the rejection of the orthodox assumption that all countries have equal access to knowledge. Indeed, it is the international differences in the knowledge of new products and processes that creates the trade in the first place. Such a Schumpeterian assumption is of course readily acceptable to observers of the innovation processes.

In a world where knowledge is equated with information (i.e. costly to produce, but virtually costless to transmit and reproduce), such trade is in theory sustainable, only if the knowledge (information) is either kept secret or protected as intellectual property. But how could it emerge in the first place? Vernon (1966) and some earlier writers argued that, while the scientific principles underlying technology are widely understood in the industrialised world, knowledge of markets

[1] For a more thorough account, see Dosi et al., 1990. For recent developments, see Fagerberg, 2002.

still tended to be parochial and national. His explanation of international differences in innovation was therefore essentially demand determined.

Many other of Vernon's insights have stood up well to the test of time and experience: for instance, his explanations of the importance of geographic agglomeration in innovative activities, and of the dynamics of international technological diffusion – and in particular of mass production - to developing countries. But it is difficult to sustain – in terms of national demand – that the technological strengths of (for example) Switzerland in marine engines and pharmaceutical products emerge from the major demands of the Swiss Navy, and the tendencies of Swiss citizens towards hypochondria. More plausible explanations emerge in terms of the extension of Swiss firms' mastery of knowledge of machinery and of synthetic dyestuffs in textiles into other potential fields of application. Similar arguments can be made about patterns of technological specialisation of many other countries.

And it is here that Nelson and Winter's emphasis on technological regimes - the cumulative and path-dependent nature of useful knowledge - has been a major step forward. In addition to information, it comprises tacit knowledge; in addition to technological knowledge it includes organisational knowledge; and in addition to knowledge from understanding, it includes knowledge from practice. We shall now explore where and why these concepts have advanced our understanding of the role of knowledge in the economy.

2. The benefits of publicly funded basic research: knowledge in papers or people?

The orthodox justification for publicly funded basic research activities used to be, and to some extent still is, the "public good" nature of the information in the papers resulting from the research. It was developed in the late 1950s to support the emerging practice in the USA after the

Second World War of large-scale public support of basic – and mainly university-based – research (Arrow, 1962; Nelson, 1959). In achieving this policy aim, it was – and perhaps still is – very successful. However, it ran increasingly into difficulties when applied to an interdependent world with countries of different sizes and at different levels of development. If knowledge is costless to transmit and re-use, why can't foreigners – who have not paid for the research – benefit from it (the free rider problem)? If the costs of obtaining foreign-produced knowledge are negligible, why do many small countries in North-western Europe perform relatively more basic research than the USA itself? Why have successful developing countries like S. Korea and Taiwan greatly increased their output of published papers?

Other anomalies emerged from empirical investigations. Why do firms in science-based industries extensively publish the results of their research when, according to the information-based view of knowledge, they should be appropriating them by keeping them secret or protecting them through patents? If the output of basic research is useful, why do patents cite published papers less frequently than its share of total R & D? If knowledge travels so freely, why is there a national bias in the pattern of citations in papers, patents and between the two?

Answers to only a few of these questions have come from fully paid-up members of the evolutionary economics community, and many more from policy makers, sociologists, bibliometricians, and eclectic and rather a-theoretical applied economists (Brooks, 1994; Callon, 1994; Hicks, 1995; Jaffe, 1989; Narin, 1992). However, the key explanations emerge from the wider view of knowledge expounded by Nelson and Winter. In particular, if knowledge is assumed to be mainly tacit and person embodied, the apparent anomalies melt away. Knowledge then flows mainly through person contacts and mobility, so the degree of international "leakage" is limited by both language and by the limited degree of international mobility. Companies publish papers in order to signal the fields and problems where they want to establish linkages to the tacit knowledge of those performing related (and largely publicly funded) basic research. Effective absorption (i.e. replication) of research results from elsewhere requires a minimum threshold of investment in research skills, equipment and professional networks. And the case for public support shifts from producing information to the training of skilled problem-solvers.

However, there is now perhaps the danger that the pendulum has swung too far (Breschi and Lissoni, 2001). The emphasis on tacit knowledge, and the example of either the Italian regions or Silicon Valley has led to an excessive emphasis on regional clusters of knowledge creation and exploitation as the basis for analysis and public policy. Whilst these are clearly important, they are only part of the story. Evidence suggests that the output of basic research provides for more than the local region: in other words, Stanford University provides for more than Silicon Valley, MIT for more than Route 128, and Cambridge University for more than its Science Park. Reciprocally, successful clusters do not simply emerge from locating activities close together, and they have many important knowledge linkages outside them.

Thus, we still need more systematic knowledge about how far and how quickly different types of knowledge can travel. Recent work by Arundel and Geuna (2001) suggests – paradoxically in the light of orthodox theory – that firms find that the foreign knowledge they find most difficult to acquire is so-called “public knowledge” (i.e. mainly university-based knowledge). This is probably because the cost and time required to join foreign, non-market knowledge networks are greater than to acquire foreign commercial applied knowledge. But even here, things are changing, as multinational firms learn the managerial art of joining foreign networks of public knowledge in fields that are competitively important (Niosi, 1999).

3. International diffusion of technology: replication is not easy

A similar story – and similar progress – has emerged in our understanding of the nature and determinants of the successful international transfer of technology. The traditional assumption used to be that such transfers require simply the transfer of embodied knowledge in machines, and of disembodied knowledge codified in blueprints and operating instructions. However, this has proved untenable in the light of the very different levels of output and productivity achieved with apparently very similar inputs. As in the case of basic research, useful productive knowledge cannot be fully codified, but involves tacit elements – both technological and organisational – that can be learned only through emulation and practice. Firms and countries apparently have different capacities to do this, and “learning” is not a simple – and often unintended – by-product of “doing”, but a consequence of deliberate investments in activities designed to improve performance.

In the advanced countries, the central locus of technological learning is corporate R & D activities. The most effective innovators turned out to be the quickest imitators, and Cohen and Levinthal (1987) have shown that corporate R & D includes both innovative and imitative activities. Large scale corporate R & D activities have also grown up quickly in the most successful technological imitator countries: Japan, Korea and Taiwan. However, painstaking case studies of technological acquisition in industrialising countries show that R & D activities are not their major learning activities. Instead, they are precisely those elements of specialised learning that underpinned earlier periods of industrialisation in the now industrialised countries: investment and production planning, quality control, and incremental improvements in products and processes (Bell, 1984; Lall, 1992). Progress still needs to be made in developing methods for measuring these activities (see Costa and Queiroz, 2002). And we are still far from understanding the incentives, institutions and practices that allow some countries to "learn" and develop much more rapidly than others.

4. The dynamic capabilities of the firm: what are they?

Together with *Research Policy* (innovation studies), the book by Nelson and Winter is the most highly cited in the *Strategic Management Journal*, reflecting its impact on the theory of the firm and on the academic field of strategic management. In particular, their notion of organisational capabilities has been developed into the so-called "dynamic capabilities" theory of the firm, the essence of which is that successful firms have organisational knowledge that enables them to sustain their competitive advantage in a fast-changing world. They do this by developing, combining and sustaining difficult-to-imitate capabilities, built on their market positions, technological paths, and organisational processes (Teece and Pisano, 1994).

Some critics argue that the theory is flawed, since it does not allow prediction: dynamic capabilities can be identified only as a consequence of success. In this writer's view, this criticism in itself is not valid. There are many theories where prediction is not possible beginning with that of Darwin himself. In a complex and fast changing world, accurate predictions of future successes, particularly when dealing with major technical changes, have on the whole proved impossible (Schnaars and Berenson, 1986). The identification of dynamic competencies is inevitably itself part of a learning process, and is neither an elegant theory enabling scholars to predict outcomes, nor a simple recipe enabling managers to achieve corporate success.

A more telling criticism of the strategic management literature in dealing with the development and diffusion of knowledge is its excessive pre-occupation with how firms create a *sustainable* advantage in knowledge and its application (Barney, 1991; Porter, 1996). Whilst this helps us understand the emergence of firms exploiting fields of major technological breakthroughs (e.g. Dupont in synthetic chemicals, Siemens in electrical and electronics, Cisco in IT), it does not help us understand the main effects of revolutionary new technologies, which are located mainly in the sectors *using* the products based on these technologies. Here, potential applications are pervasive because they radically reduce costs, and are disruptive because they often lower barriers to entry. But they are also unlikely sources of sustainable advantage since they are readily available to all competitors from suppliers. Historical examples include the effects of electricity as a power source on the location and operation of factories, and a major contemporary example the effects of ICT on banking, retailing and corporate services.

5. What are innovating routines?

In spite of the apparent success of the dynamic capabilities view of the firm, the notion developed by Nelson and Winter of "routines" (i.e. regular and predictable behavioural patterns within firms) has not been translated into operational categories that can be useful to practitioners and to analysts trying to deal with innovation processes in firms, or more generally with organisational knowledge. As I have argued elsewhere (Pavitt, 2002), this shortcoming could be remedied by closer interaction with the rich and varied empirical literature on the management of innovation. From this literature, it emerges that routines can best be operationalised as activities developed to undertake essential tasks. For example, an essential task in the large innovating firm is the integration of specialised functional knowledge (e.g. between R & D, production and marketing). This can be achieved by a variety of routines, ranging from inter-functional flows of information and people to the establishment of "heavyweight" product development teams. The main tasks which innovating routines must fulfil emerge from three fundamental features of innovative activities since the industrial revolution.

- First, there is the increasing specialisation in the production of knowledge, whether by field, function or institution. Hence the importance of "routines" related to knowledge networks, knowledge co-ordination and knowledge integration, as firms progressively integrate an increasing range of useful knowledge.
- Second, there is the increasing complexity of artefacts, reflecting increasing scientific understanding, but with the continuing tendency for technological practice to run ahead – but not too far ahead – of scientific theory and experimental techniques. If it does, the costs of experimentation can become prohibitively high. Hence the continuing prevalence of routines to deal with uncertainty, and to encourage and exploit both fundamental scientific breakthroughs (e.g. molecular biology), and the formation of the engineering disciplines (software engineering). Hence also the growing importance of ICT and simulation techniques in reducing the costs of experimentation.
- Third, there is the continuing importance of matching specific organisational practices to the specific characteristics of changing technologies, products and markets. These practices include routines for allocating resources, monitoring and control, skill and network formation, and divisional organisation. In established firms, failure to adapt these organisational practices to the requirements of radically new technologies is now more likely than failure of technological mastery itself.

6. Industry structure and dynamics: the uneven development of knowledge

The evolutionary framework of search and selection, developed by Nelson and Winter, makes the nature of knowledge and firms' investment in it a central factor in explaining the size, structure and dynamics of industries. Studies using empirical data from the pioneering Yale Survey have confirmed that intersectoral differences in the richness of opportunities emerging from technological knowledge help explain intersectoral differences in the size and R & D intensity of firms (Levin et al., 1985). And within industries, differential rates of investment in knowledge (i.e. R & D) between firms determine the likelihood of firms' survival and growth (Klepper and Simons, 1997).

Industry dynamics has also been associated with the use of dynamic search-and-select models and simulations, often using biological techniques and metaphors. For reasons of mathematical convenience, simplifying assumptions are often made that are at variance with the empirical evidence. For example, clear distinctions are not made between firms, products and technologies (see also section 7 below), although the evidence shows that large firms contain many products (but fewer over time), and many technologies (but increasing over time). Or it is assumed that selection between products is made entirely through market competition, thereby neglecting search and selection processes within firms. In any event, three sets of questions deserve greater attention.

First, why do producers' goods sectors (e.g. machinery, instruments, applications software, and biotechnology) have low levels of concentration, when most of the industrial dynamics literature associates high technological opportunity with high levels of concentration? The standard answer is that that they have low levels of appropriability (i.e. innovators cannot capture enough of the benefits) and of cumulativeness (i.e. their technologies do not build on previous experience). Why the latter should particularly be the case with knowledge in small firms and in production processes is not clear, and empirical observation bears this out. An alternative explanation is that they are technologies with pervasive applications (Freeman et al., 1982) and low costs of entry (Marsili, 2001)[2].

This relates to the second question, namely what is the influence of vertical linkages between industries? Providers of capital goods (and of knowledge) often have complementary relations with their user-industries in the dynamics of technical change. Given the complex nature of much useful knowledge, specialisation is not complete: both producers and users often retain technological competencies in similar fields, in order to be able to co-ordinate interdependent processes of technical change. This can also increase the possibilities of entry from either users or suppliers. Under what conditions does this happen? The question is all the more important, given the growth of vertical alliances between firms with the purpose of promoting technical change.

Third, how do new product dynamics begin? Historical studies show that new products do not appear simply because of a flash of genius from a scientist, an engineer or an entrepreneur. The accumulation of prior knowledge over a wide spectrum is also a necessary input. Barras (1990) has argued, in the case of applications of information technology in finance and banking, that the radical product innovations that conventionally are said to start a new industry cycle are in fact preceded by the accumulation of process innovation (i.e. a “reverse product cycle”). Similar processes may have been at work in electronics and machinery. Alternatively, Geels (2002) has used the example of the development of the steamship to propose that major technological transitions involve (in some sort of sequence) variety and experimentation, applications in niche

[2] These have since been “re-discovered” by the economic orthodoxy, and given the name “General Purpose Technologies”.

markets, and major reconfigurations in both product architecture, supporting infrastructure and social beliefs. Johnson and Jacobsson (2001) use similar concepts to analyse the recent emergence of the wind turbine industry.

Finally, how and why do major technological opportunities emerge? Uneven patterns of technological development across fields and time not only help explain sectoral differences in concentration and change. They also explain why demand determined models of technical change – and policies based upon them - are inadequate. Unfortunately, in most writing in evolutionary economics, the notion of technological opportunity has become nothing more than a conceptually useful, but exogenously determined, variable. Fortunately, the path-breaking work of Rosenberg (1974) shows that insights into its determinants can be found in the histories of scientific and technological developments. They can also be found increasingly in the cognitive sciences, where two factors influencing the speed of technological advance have been identified: first, advances in underlying explanatory theory (e.g. modern molecular biology); second, the speed with which prototypes can be tested (e.g. computer-based simulations) (Mahdi, 2002; Perkins, 2000)

7. Diversity in what?

Nelson and Winter recognised that evolution in an uncertain and complex environment depends on diversity (pluralism). This has been widely acknowledged since, both in general discourses on the evolutionary economy, and in specific models of evolutionary processes. However, there has often been a fair degree of empirical imprecision in specifying what sort of diversity we are talking about. In our studies of the technological activities of large firms, Pari Patel and I (Patel and Pavitt, 1997) have identified two types of diversity.

- Diversity *between sectors* in large firms' *mix* of technological fields in which they have shown competence to make progress in the state of the art. This shows that different product groups have different and distinctive sources of knowledge on which they draw.
- Diversity *within firms* in the *range* of technological fields in which they have competence. This shows that firms, and the products that they make, incorporate knowledge from a number of technological fields, and that products must be carefully distinguished (analytically and operationally) from technologies.

However, our studies also show the following.

- *Lack* of diversity *within sectors* of the *mix* of technological fields in which competing firms have a demonstrated competence for improvement. This shows that firms do *not* compete on the basis of *technological* diversity: specific bodies of knowledge are rigidly associated with (say) designing an aircraft engine or a drug, and they are different.

This is because there is in fact very little uncertainty about the broad sources and directions of major technological change. Communities of practising scientists know (or soon get to know) what is happening and what is technically feasible in, say, information storage and retrieval, gene splicing, new materials, mechanical engineering, etc. However, numerous case studies show a major lack of consensus – and related uncertainties – about how technologies can be transformed into working artefacts that are useful, and about the appropriate organisational practices to develop, produce and sell them (Christensen, 1997; Utterback, 1993). It is therefore along these dimensions – products and organisational practices, rather than technological fields – where diversity and experimentation are likely to be the basis for competition in the contemporary innovating firm. In this context, the distinction made by Nelson (2000) between knowledge as technological understanding (strong and reliable), and knowledge as organisational practice (weak and unreliable), is particularly relevant.

8. Conclusions

This paper reflects mixed achievements in developing and using Nelson and Winter's original insights into the nature, sources and impact of knowledge. The importance of tacit and organisational knowledge has greatly increased understanding and improved action in three areas: the public support of basic research; the nature of technological backwardness in a country or region; and the sources of corporate competitiveness. In addition, the notions of search-and-selection, and of path-dependent and cumulative technological regimes, have been a powerful heuristics in understanding the dynamics of industrial development and industrial structures. In all these cases, original concepts have been enriched and developed through strong participation in policy debates, and through empirical studies often originating from traditions outside evolutionary economics.

At the other extreme, the concepts of organisational routines, and of diversity, have both been influential conceptually, but unoperational (even misleading) practically. Research on organisational routines has on the whole avoided systematic engagement with the rich and varied findings of research emerging from empirical studies of innovations, organisations and working practices. Discussions about diversity have been mesmerised by biological metaphors and models, and have failed to come to grips with the particular characteristics of the search and selection environments in invention and innovation.

More recent work by Nelson (1993), Lundvall (1992) and others has developed the concept of national systems of innovation, namely, the institutions, incentives and competencies that influence the generation, diffusion and application of knowledge in a country. This concept implicitly recognises the importance of tacit and person-embodied knowledge, the diffusion of which is strongly influenced by distance and language. It also recognises the importance of specialisation in the production of knowledge, since the core of national systems of innovation is composed of specialised institutions combining and interacting in the production, diffusion and application of specialised knowledge.

I have argued elsewhere (Pavitt, 1998) that we have more to learn from Adam Smith than from Schumpeter about the importance of specialisation in knowledge production. Amongst other things, it helps us understand some of the enduring problems of corporate knowledge management: such as integrating specialised knowledge and functions; dealing with "tribal" loyalties to disciplines and professions that can transform competencies into rigidities; and –as already mentioned – distinguishing technologies from products. And there is now plenty of evidence from so-called strategic alliances to show that – as a consequence of increasing specialisation – even large firms are now finding it difficult to internalise all the technological competencies that they need. As a consequence, the appropriate unit of analysis may no longer be the business firm, but the knowledge-related networks in which it is embedded (Brusoni et al., 2001; Richardson, 1972)

As for other areas for future research, the paper has tried to identify a variety of useful studies that could be undertaken. More generally, some of the greatest opportunities appear in neglected but important fields, namely, in understanding the emergence (or pre-histories) of major new industries and major new scientific breakthroughs, where greater use could be made of a relatively old discipline (History of Science, Technology and Enterprise) and of a relatively new one (Cognitive Science).

Finally, we should note that the evolutionary concepts developed by Nelson and Winter have proved most valuable to our understanding of the nature and policy implications of what has come to be called the knowledge economy, when they have been open to problems and empirical results emerging from a variety of disciplines. A more closed system might have the virtues of greater coherence and (formal) analytical rigour. But its relevance to the rest of the world could become open to doubt.

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