

IF NELSON AND WINTER ARE ONLY HALF RIGHT ABOUT TACIT KNOWLEDGE, WHICH HALF? A REPLY TO DAVID, FORAY AND COWAN

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For Submission to Theme B: Production and Use of Knowledge

‘The search for certainty reveals itself as a fear of the truth’ Hegel

Abstract

This paper argues that there is an underlying theoretical tension in Nelson and Winter’s (1982) theory, and specifically in its attempt to link Polanyi’s and Simon’s treatments of knowledge. Moreover, it argues that this underlying theoretical tension contributes to a number of cleavages in the science and technology policy and economics literatures. These cleavages produce a sense of dynamism as each side tries to claim Nelson and Winter (1982) for their own, but also and paradoxically, an unhealthy tendency towards intellectual stability.

This underlying tension has surfaced in debates about whether the idea that IT allows the codification of tacit knowledge is insightful or not. Recently, Foray, David and Cowan (2000) threw down the gauntlet with a polemic attack on the tacit knowledge tradition as ‘of no use for social science’ – this paper picks up the gauntlet and argues that they, rather than the tacit knowledge tradition, are theoretically flawed and empirically mistaken.

From ‘The Moon and the Ghetto’ to ‘Cancer and Communism’

The underlying tension in the economics of science and technology since Nelson and Winter’s book reveals itself in a number of ways. For example, why are the papers in the *Journal of Evolutionary Economics* largely (but not exclusively) abstract, mathematical and non-empirical, while the papers in *Industrial and Corporate Change* are largely (but again not exclusively) dealing with concrete reality, in an empirical and largely non-mathematical way? Both streams of research are derived, in part at least, from Nelson and Winter’s work, yet citations analysis reveals a very limited overlap. More significantly, the tension can be found in the overlap between how academics think about knowledge and how they think about firms.¹ Some take an abstract information processing approach, in which Nelson and Winter’s book forms part of a tradition that links back to Simon and Arrow. In this approach both firms and minds can be understood as information processing problem solvers. Alternatively, some take a more concrete approach and look at the specific empirical nature of problem solving, highlighting its tacit (Reber 1989, Vincenti 1991), embodied and embedded nature (Nelson 1996), and the importance of divisions of scientific labour, routines and co-ordination mechanisms. In this approach Nelson and Winter’s book forms part of an appreciative theory

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¹ Just because Arrow, Simon, Polanyi, Hayek, and even Charles Babbage have all treated human cognition and firms with similar frameworks it doesn’t follow that they should. Indeed, one might point out that the differences between a human brain – 3lbs of living, intentional, conscious, porridge like substance, that also happens to be the most complex object in the known universe – and firms with their technologies, organisational charts, links to governments, and employees, are slightly more than substantial.

tradition that goes back to Polanyi and Schumpeter.² Following Foray, David, and Cowan (2000), these differences are conceptualised as differences between objectivist³ and tacit knowledge traditions. In general, this distinction is repeated in people's treatments of knowledge and of firms.

The objectivist tradition, of which David, Cowan and Foray are a part, might argue that both firms and brains are in reality just problem solving processes that are essentially algorithmic. And the biological features of problem solving in brains, and the organisational and technical features of problem solving in firms that at first glance might appear important to a naïve and unsophisticated novice, can be ignored, as illusory, and not part of the real essential features that are analysed by level-headed, sceptical, scientific economists. As Mary Midgley suggests when one looks at this suggestion, one needs to pay close attention to the language – words and phrases like *in reality*, *illusory*, *appear*, and *essential features* invoke not physics but ontological metaphysics! The codification debate, after all, has never been about data.

The paper will argue that these differences in what is regarded as *real* express deep attitudes that underpin the practices in which each tradition emerged. This, in turn, is reflected in the varying funding sources for research on knowledge. In the US the abstract, Artificial Intelligence, problem-solving literature has tended to be funded by the DoD, while the concrete neurological and psychological work has been funded by the NIH, reflecting another cleavage in post-war research funding between American 'fear of communism and fear of cancer' (Pavitt 2000).⁴ The paper will argue that Nelson and Winter attempted to provide a theory of knowledge that brought these two sides together, and furthermore that the tensions that have followed their work are consequences of the fact that Polanyi and Simon are incompatible. They are incompatible because what they regard as 'real' is fundamentally different. Indeed Polanyi is a key player in an ongoing attack on the objectivist's position.⁵

Concentrating on the way that knowledge is understood, the paper will argue that up until the 1990s our understanding of cognition was extremely limited and could not be used to decide between the tacit and objectivist positions. This however has changed in the 1990s ('the decade of the brain') with the introduction of CAT scans and new genetic 'knock-out' animal models.⁶ Today we have a conclusive scientific means of deciding which side of the Simon-Polanyi cleavage is closer to reality – the paper will follow Damasio and Edelman and show that Polanyi was largely (though not totally) correct and that the objectivists are basically wrong. And what appears to be a metaphysical disagreement is based on a lack of understanding.

² A similar cleavage can be found in the justifications used for the public subsidy of science, one side takes an information-processing, public-good approach (going back to Nelson's 1959 paper), while another stresses the role of training and providing researchers with the ability to solve complex technical problems.

³ Objectivism according to Edelman '...stems from the notion that objects in the world come in fixed categories, that things have essential descriptions, that concepts and language rely on rules that acquire meaning by formal assignment to fixed world categories, and that the mind operates through what are called mental representations [that can be described algorithmically].' (Edelman 1996:228).

⁴ This in turn reflects another cleavage in thinking about science funding, that goes back past Nelson's 'The Moon and the Ghetto' to Francis Bacon. This is the distinction between science that 'alleviates man's estate' mainly through the eradication of disease, and science that can be used for controlling nature and men for higher goals.

⁵ Foray et al (2000) for example, regard Polanyi as someone who pointed out the rather trivial point that attention is only possible against a backdrop of less conscious perceptions. This perception is significantly different from Charles Taylor's account of Polanyi, who he describes (with the late-Wittgenstein), on page 514 of his *Sources of the Self*, as one of the authors of '*much of the most insightful philosophy of the twentieth century*' (1989). Similarly, Freeman and Soete (2000) cite Searle and not Polanyi when discussing tacit knowledge, and as a philosopher Searle is most famous for his attacks on objectivist artificial intelligence.

⁶ There were of course excellent psychological studies and attacks by philosophers on the objectivist position (Nelson 1996, Reber 1989, Lakoff 1989, Searle 1995, Taylor 1989).

The paper then goes on to explore developments in the tacit knowledge tradition since Nelson and Winter's seminal work – principally in the work of Damasio, Endelman, Lakoff, Turro, John Searle and Charles Taylor. Particular attention is paid to Searle's developments of Austin and Grice's *Speech Act Theory* which is used to mark out a distinction between *assertive* and *directive* Speech Acts, (which can be related to Walter Vincenti and John Barrow's distinction between scientific and technical theories). And secondly to his analysis of causation in Darwinian and pre-Darwinian theories (which has implications for evolutionary theories of the firm). The paper relates these features to scientific and technical knowledge production, before analysing the criticisms of David, Foray and Cowan.

The paper accepts that their attack on glib use of tacit knowledge is welcome and that more rigour is needed in how it is used.⁷ However, the problem with their polemic attack is that they do not name names and distinguish the good from the bad. Consequently, they throw the baby out with the bath water, and fail to appreciate the strength of some of the positions they attack.

The paper argues that because of the flaws in their objectivist position their views on tacit knowledge and codification commit a category mistake that confuses 'knowledge as a capacity, with information as a state'. Furthermore that they adopt a pre-Darwinian form of explanation that inverts causation, and a pre-Newtonian standard of proof (i.e., 'formal consistency' rather than 'growth of knowledge'). The paper then problematises their conceptions of knowledge, science, technology, innovation, firms, impact of IT on knowledge and innovation, science policy, codification, and tacit knowledge, arguing that much of what they say leads to a unhelpful policy perspective. These theoretical critiques are supported by empirical evidence drawn from empirical case studies of the impact of IT on knowledge production and use in the pharmaceutical, software, aerospace, banking and chemicals sectors.

However, the paper does not argue that David, Cowan and Foray's position is without merit. That would be to fall into the objectivist mistake of seeing knowledge production as something that occurs for its own sake, rather than as part of what Hillary Putnam calls an ongoing practice. When their work is seen not as a scientific theory⁸ but as part of a technical theory⁹ that attempts to provide boundary conditions for policy thinking, then their work is far superior to, and far more useful than, the tacit knowledge tradition. Problems only arise when one confuses the two, and the tacit knowledge tradition provides a way of avoiding those problems. In summary; advances in the tacit knowledge tradition since Nelson and Winter (1982), particularly the work of John Searle, allow the theoretical tension in Nelson and Winter's book to be resolved – their use of Polanyi allows us to understand 'how the world is', while their use of Simon allows us to understand 'how we want the world to be'. As Nelson eloquently pointed out in *The Moon and the Ghetto* 'how the world is' and 'how we want it to be' are rarely the same. The paper concludes that there is no need to choose which half of Nelson and Winter's theory was right about tacit knowledge. The implications for evolutionary economics, technology policy and the theory of the firm are discussed.

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⁷ Indeed they could have gone a lot further, as much of the knowledge management literature's treatment of tacit knowledge is extremely weak.

⁸ That is in the terms of speech act theory an *assertive* speech act with a word-to-world direction of fit and 'true or false' conditions of satisfaction (Searle 1997).

⁹ That is in terms of speech act theory a *directive* speech act with a world-to-word direction of fit and 'good or bad' as conditions of satisfaction (Searle 1997).

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