

Review Essay

Why Is Management Not an Evolutionary Science? Evolutionary Theory in Strategy and Organization

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Books reviewed in this essay:

Howard E. Aldrich and Martin Ruef, *Organisations Evoking*, 2nd edition (London: Sage, 2006), 330 pages

Rodolphe Durand, *Organizational Evolution and Strategic Management* (London: Sage, 2006), 190 pages

Geoffrey M. Hodgson, *The Substitution of Institutional Economics: Agency, Structure and Darwinism in American Institutionalism* (London: Routledge, 2004), 534 pages

John A. Mathews, *Strategizing, Disequilibrium, and Profit* (Stanford, CA: Stanford University Press, 2006), 265 pages

Richard R. Nelson, *Technology, Institutions, and Economic Growth* (Cambridge, MA: Harvard University Press, 2005), 306 pages

Douglass C. North, *Understanding the Process of Economic Change* (Princeton, NJ: Princeton University Press, 2005), 187 pages

INTRODUCTION

In 1898, Thorstein Veblen asked: 'Why is economics not an evolutionary science?'. Veblen's paper was a scathing attack on the state of economic theorizing in his time and outlined an ambitious research agenda to turn economics into an evolutionary- social science (Veblen, 1898). He envisioned economics as a social science, taking inspiration

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Evolutionary Theory in Strategy and Organization

from developments in biology following the seminal work of Darwin (1859) and closely linked to psychology, anthropology' and sociology. Over a century later, we can safely say that this is not how economics turned out. The 'dismal science' has been portrayed as an autistic child that spends its time pulling off ingenious mathematical tricks that have little, if anything, to do with the real world. Critics of mainstream economics in 'the post-autistic economics network' and elsewhere see such behaviour as the result of a hardening of the discipline around a neoclassical core that puts the elegance of its models above a realistic view of its object of study: the behaviours of real people in an institutionalized world.

It could be argued that if there is a science today that comes close to resembling the vision of economics as an evolutionary and multidisciplinary social science that Veblen entertained, it is management studies. Its multidisciplinary nature, at least, is an important characteristic. Psychology, anthropology, sociology, and economics meet in the study of organizations, and in many ways management studies is a microcosm of all social sciences. On the other hand, few would label management studies as an evolutionary science. The *Leitmotiv* for this review essay is the question why this is so. Are there lessons for management studies in the way in which economic theory developed, and do we have anything to gain from mirroring our discipline on the vision for economics that Veblen held out a century ago?

These questions will serve to draw together the reviews of the six books that form the core of this essay. My original assignment by the book review editor was to review evolutionary theory in strategy and organization on the basis of recent books by Richard Nelson (2003), Douglass North (2005), Howard Aldrich and Martin Ruef (2006), and Rodolphe Durand (2006). The former two books are by leading economists of an evolutionary and institutional bent who are critical of the neoclassical paradigm in economics. The latter two books are by evolutionary organization theorists with a sociological orientation. To balance out this list, I included a third book from an economist, Geoffrey Hodgson (2004), and a book from a strategy' scholar with a background in economics, John Mathews (2006). Together, these six books give a good idea of where evolutionary theorizing in both economics and management studies stands today.

My message will be that all six books deal with issues that are crucial to moving social science theory and, by extension, management studies forward. Together, they hold out the promise of an evolutionary social science that in many ways resembles the vision for an evolutionary institutional economics that Veblen promulgated over a century ago. This is both a humbling and an exciting thought. It is humbling because it makes one wonder how much progress social science has actually made in the twentieth century. The three books by the economists reviewed here, at least, give the reader the impression that much of the intellectual effort that has been applied to advancing their mother discipline has been misdirected. At the same time, a revival of Veblen's research agenda is an exciting prospect because some progress has, of course, been made. This progress allows a much more convincing attempt to understand how organizations and their institutional contexts change over time than the one that Veblen and his contemporaries were able to muster.

The purpose of this review is to put evolutionary theorizing in the social sciences in historical and philosophical perspective. As will become clear below, evolutionary theo-

ri/drigr is first and foremost concerned with how social structures transform themselves from within. As such it can be contrasted with a-historical social science perspectives such as neoclassical economics and functionalist sociology. I will first review the three economics books in terms of the general issues that they highlight with respect to theorizing in the social sciences and in terms of the building blocks for an evolutionary approach that they propose. The three management books will subsequently be reviewed in terms of their contributions to evolutionary theorizing in management studies, but I will also consider them as part of the broader movement towards an evolutionary social science. I will conclude with a reflection on the title of this essay: why is management not an evolutionary science?

EVOLUTIONARY THEORIZING IN ECONOMICS

The three economists whose books are reviewed here are united in their themes. They all present their work as criticism of the neoclassical view of economics, take an evolutionary view of economic change, and emphasize the importance of institutions in understanding economic outcomes. Yet there are also key differences in how they position their work. Nobel laureate Douglas North labels his work 'the new institutional economics', Richard Nelson is widely seen as one of the founding fathers of modern 'evolutionary economics', while Geoffrey Hodgson may be seen as an evolutionary institutional economist working in the tradition of the 'old' institutionalism.

Geoffrey Hodgson's work is a good guide to understanding the subtle differences between these different approaches to evolutionary theorizing in economics. His book, *The Evolution of Institutional Economics: Agency, Structure and Darwinism in American Institutionalism* (2004), is first and foremost a history of ideas, and an impressive one at that. It continues earlier work on evolutionary ideas in economics (Hodgson, 1993) and is the sequel to *How Economics Forgot History: The Problem of Historical Specificity in Social Science* (Hodgson, 2001). As Hodgson notes in his 2004 book, his aim is to 'recover materials from the past to help build something new' (p. xvii). Central to the materials from the past are ideas from the German historical school of the nineteenth century on the historical specificity of social science theory (in the 2001 volume), and ideas of the American institutional economics of the early twentieth century on the relation between agency and structure (in the 2004 volume). In the book that is reviewed here, Hodgson's particular focus is on reviving, updating, and extending Veblen's evolutionary framework to help build something that allows social scientists to adequately deal with these two fundamental philosophical problems.

Hodgson's 2004 book documents the rise and fall of American Institutionalism. Those with an interest in either economic thought or the philosophy or sociology of science will certainly enjoy the detailed and engaging way in which this story is told. Those who would expect to take away a little more than history when reading over 400 pages can learn a lot about the problems inherent in studying the interaction between agency and structure. Hodgson's argument is that neither methodological individualism, nor methodological collectivism, nor attempts to circumvent the problem by conflating agency and structure will do to solve these problems (Giddens' structuration theory, among other approaches, is exposed as belonging to the third category). Instead, there is a call

to arms for the reconstruction of the social sciences around the recursive causality of an evolutionary explanation. I have reviewed these aspects of the book elsewhere (Stoelhorst, 2007) and will here focus on how Hodgson's effort to exhume Veblen's view of an evolutionary institutional economics helps put North and Nelson's work in perspective. Positing that '[t]he significance of Darwinism for the social sciences has been largely unrecognised since Veblen' (p. 9) is an overstatement that overlooks, among others, the work of Donald Campbell (1969) and its impact on organization theory (more on this below). But exploring Veblen's views with Hodgson as a guide is certainly enlightening.

Thorstein Veblen was an economist, sociologist and social critic with a PhD in philosophy. He became one of the leading figures in American Institutionalism as it took shape around 1900. Today Veblen is perhaps best known for coining the term 'conspicuous consumption' in his critical assessment of American culture in *The Theory of the Leisure Class* (1899). But Veblen also combined his diverse interests to outline an ambitious research programme to make economics an evolutionary social science. Such an evolutionary economics had to be 'a theory of a process of cultural growth as determined by the economic interest, a theory of a cumulative sequence of economic institutions stated in terms of the process itself' (Veblen, 1898, p. 393). Veblen contrasted an evolutionary approach to economics with the typical teleological mode of explanation in economics that assumed a passive, inert, and immutable 'hedonistic' human nature. The following quote is typical of Veblen's prose and has often been used to summarize his critique of this assumption:

The hedonistic conception of man is that of a lighting calculator of pleasure and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. He is an isolated, definitive human datum, in stable equilibrium except for the buffets of impinging forces that displace him in one direction or another. (Veblen, 1898, p. 389)

The crux of Veblen's critique of prevalent theorizing in economics, be it in the German historical school, the Austrian school, or the neoclassical school, was that it explained economic outcomes in terms of 'natural' tendencies towards equilibrium rather than in terms of causal mechanisms. What was needed instead was a theory in terms of human action that offered a causal explanation for economic development, which Veblen saw as the process of cumulative change by which 'the methods of dealing with the material means of life' (1898, p. 387) were transformed over time. Human action, in turn, was to be understood in terms of the instincts that are the products of hereditary traits and the habits that are the products of past experience. A theory of economic development should specify the recursive causality between individual agency and the 'body of traditions, conventionalities, and material circumstances faced by the individual' (1898, p. 390).

Hodgson recovers the building blocks of Veblen's institutionalism: the pragmatist philosophy of Charles Sanders Peirce, the pragmatist philosophy and habit-instinct psychology of William James, and the way in which Veblen took inspiration from Darwinian ideas to understand economic change. But he also shows how Veblen's

explicitly evolutionary view of economics got lost in the subsequent rise and decline of American Institutionalism. One reason for this development" was that Veblen never achieved a systematic statement of his evolutionary approach. Another reason was the way in which the disciplines to which he had looked for inspiration developed. Pragmatist philosophy was displaced by positivism, habit-instinct psychology by behaviourism, and, in biology, Darwin's emphasis on selection was relegated to a secondary role in the explanation of biological evolution until the modern synthesis of Darwin's theory of natural selection with Mendelian genetics in the 1940s. When institutional economics became a recognized school of thought in 1918, it had already lost much of its Veblenian ballast.

Under the impulse of John Maurice Clark, Wesley Mitchell, John R. Commons, and others, institutionalism arguably became the leading school of thought in economics in the interwar period. However, Hodgson goes on to show how internal divisions and a string of external events resulted in the loss of this mainstream position to the neoclassical approach to economics. Institutionalism never produced a systematic theory and, without agreement over fundamental methodological and theoretical issues, what united its proponents was largely their opposition to neoclassical economics and a shared political agenda that opposed free-market ideology. Today, these characteristics still define in large part the identity of the Association for Evolutionary Economics and its outlet, the *Journal of Economic Issues*, which have kept the ideas of the original institutional economics alive.

The work of later institutional economists such as John Kenneth Galbraith, William Kapp, and Gunnar Myrdal notwithstanding, institutional economics became a marginal stream of research until Oliver Williamson, Douglas North and others heralded the 'new-institutional economics' in the mid-1970s. However, picking up on Coasian transaction costs rather than Veblenian evolutionary themes, the new institutional economists have typically distanced themselves from the 'old institutionalism on the grounds that the latter was a-theoretical. Hodgson's book is in part meant to redress this unwarranted view of the original Institutionalism. Not only does Hodgson recover the theoretical work of Veblen and others, but he also reverses the criticism by pointing out that the new institutionalism still builds its theories on the basis of the very things that Veblen took issue with: given, immutable individuals and a 'natural tendency' of institutional arrangements towards efficiency. Nelson (2005) agrees and refers to those that entertain such views as 'neoclassical institutionalists'. This particular brand of institutionalism, of course, has been very successful within the economics community, with Ronald Coase and Douglas North both receiving a Nobel Prize for their institutional work. A likely explanation for this success is how close they have stayed to the neoclassical view of economics. But, as Nelson notes, in his latest work North, at least, seems to have changed his position on the very two issues that were central to Veblen's critique of economic theorizing in the neoclassical tradition.

The title of Douglas North's book, *Understanding the Process of Economic Change* (2005), flags its purpose. This monograph is an extension of his earlier work in the 'new institutional economics' (North, 1981, 1990). North's work has always been historically oriented and has always placed the role of institutions, 'the incentive structures of economics', centre stage. As Nelson (2005) observes, North's earlier view that institutions

tend to evolve towards greater efficiency had, in his 1990 book, changed to an emphasis on how lucky societies are that have relatively efficient institutions. In North's 2005 book, he also relaxes the assumption of immutable individuals. In this book North takes a closer look at how human beliefs and preferences affect the process of societal change. His starting point is the observation that we are a long way from completely understanding this process and that such understanding 'is a necessary prerequisite missing in the economist's rush to model economic growth and change' (p. ix).

North acknowledges that a complete theory of economic change would require an understanding of demographic change, changes in the stock of knowledge, institutional change, and the ways in which these three processes relate. However, the focus of the book is on the role of institutional change. Institutions, be they political, economic, or social, are the formal rules, informal norms, and enforcement characteristics that lay down the 'rules of the game' for human interaction. Institutions can be understood as ways to reduce uncertainty. While institutions have greatly reduced the uncertainties associated with the physical environment, they have at the same time resulted in a vastly more complex human environment. A central message of the book is that we still do not understand this institutional environment and the way it changes very well. As a consequence we are very ineffective in dealing with the problems we cause as we create increasingly complex and interdependent human environments.

North makes an important point regarding the historical transition from personal to impersonal exchange. This transition requires a different set of institutions. Much of the disparity in economic performance across cultures can be understood in terms of the degree to which institutions that effectively support impersonal exchange have evolved. 'Personal exchange by its very nature restricts the range of economic activity to clientism and repeated face-to-face interaction. Impersonal exchange entails a host of political, social, and economic institutions that "violate" the innate genetic predispositions that evolved in the several million years of hunter/gather environments. Both successful market exchange and political democracy hang on our ability to deal with these issues' (2005, p. 71). Moreover, in the long run, what matters most is the adaptive efficiency of the 'institutional matrix'. Adaptive efficiency 'entails a set of institutions that readily adapt to the shocks, disturbances, and ubiquitous uncertainty that characterize every society over time ... it is important to understand that we do not know how to create these conditions in a short period of time' (p. 78).

It is a pleasure to read North. His prose is dense but crisp (some of the praise here is apparently due to his wife) and the text concise and very tightly organized. The importance of a better theory of economic change is driven home from the outset. Such a theory 'would enable us to account for the diverse performance of economies, past and present ... A real understanding of how economies grow unlocks the door to greater human well-being and to a reduction in misery and abject poverty' (North, 2005, p. vii). Part I of the book puts in place the building blocks that North sees as crucial to building a theoretical framework to understand economic change: Knightian uncertainty (Knight, 1921), belief systems, human intentionality, and institutions. Part II of the book applies these concepts to an analysis of the mixed historical record of economic development in the Western world in general, the United States in particular, Latin America, and the Soviet Union. The message of the book is crystal clear: to understand economic

change and to be able to build institutions that are conducive to economic growth, we need to take human cognition into account.

The fundamental issues at stake and the opportunity to see the mind of a leading economic theorist at work are sufficient reason to read and enjoy this book. At the same time, the book shows the signs of the gestation period of more than ten years that is mentioned in its preface. There are few, if any, references to recent theorizing in either the evolutionary or the broader institutional literature. Arid on a grander time-scale, it is difficult to escape the conclusion that North's work has taken us back to Veblen. 'There is an intimate relationship between belief systems and the institutional framework' (2005, p. 49) is a quote from North, but could as well have been from Veblen. And North's emphasis on the need to incorporate human intentionality into an evolutionary mode of theorizing and on the need to acknowledge that institutions can both facilitate economic growth or lead to stagnation and decline can be read as a restatement of Veblen's research agenda.

Whether or not North is conscious of the fact that he is returning to the view of economic change advocated by Veblen is unclear. Veblen is mentioned in passing on page 65, but there is no reference to his work. In as far as North pays homage at all to other economists who inspired him, it is to Austrian economist Hayek, who was one of the few leading economists to embrace an evolutionary style of reasoning in the 1950-70 period, but who was on the opposite side of the political spectrum from institutionalists like Veblen. Whatever the reason may be for North's positioning, the nature of his most recent work is testament to Nelson's observation that '[o]ver the past quarter century . . . evolutionary and institutional economics have shown new life, and they are beginning to join together again' (2005, p. 3).

Richard Nelson's *Technology, Institutions, and Economic Growth* (2005) is another nice example of both this new life and the rejoining of the evolutionary and institutional threads of theorizing that developed largely independently of each other after Veblen. Nelson's book is a collection of nine essays that were published in different journals between 1995 and 2004. For a collection of essays, the book is remarkably coherent. As indicated by its title, it has much the same focus as North's monograph, although with an added emphasis on the role of technology in economic growth. Given Nelson's earlier work, this particular focus should come as no surprise. A long time student of the relationship between science, innovation, and economic growth, his work with Sidney Winter in the 1970s, which culminated in their book *An Evolutionary Theory of Economic Change* (Nelson and Winter, 1982), helped launch modern evolutionary economics. With the successful inauguration of such journals as the *Journal of Evolutionary Economics* and *Industrial and Corporate Change* around 1990, evolutionary economics is now a vibrant area of research. If anyone were ever to be honoured with a Nobel Prize for his work in advancing this branch of economics, Nelson would be an obvious candidate.

Although Nelson retains the 'Schumpeterian' focus on technology as the driver of economic change that is typical of modern evolutionary economics, his emphasis on institutions in this collection of essays brings him much closer to North's implicit 'Veblenian' view than may have been expected on the basis of the limited cross-referencing between the two authors. Reading North and Nelson back to back means being exposed to a very similar argument: mainstream economics is unable to adequately deal with the

dynamics of economic change and we should, therefore, embrace an evolutionary approach in which institutions play the central role in the explanation of economic growth. There is more of the typical modern institutional emphasis on transaction costs and institutional constraints in North and more of the typical modern evolutionary emphasis on technological change and the growth of knowledge in Nelson, but the overall line of reasoning is remarkably similar. In that sense, Nelson's work may be seen as complementing North's by explicitly targeting an aspect of economic change that North acknowledges but leaves aside in his analysis: the growth of the stock of human knowledge.

Nelson's book mounts an explicit attack on the neoclassical theory of economic growth, which he describes as 'hopelessly inadequate' (p. 1). In Part I of the book, Nelson criticizes in detail the modern manifestation of the neoclassical approach to economic growth, or 'the new growth theory'. With its exclusive focus on formal models with strong rationality assumptions this theory is still too far removed from the real process of economic growth to give much insight into the phenomenon. The remainder of the book is an argument for an approach that accepts two premises that 'fly in the face of current orthodox thinking' (p. 5): first, that 'it is misguided to look for a simple formal model, or a few simple empirical laws, that will capture the essence of what we know about economic growth'; and second, that 'many of the most important variables defy simple quantitative characterization'⁷ so that 'much of theorizing in economics needs to be verbal, with formal analysis playing a supporting but not dominant role' (pp. 4–5).

The building blocks for an alternative approach that rests on these premises are laid out in three essays in Part II of the book. The first is a review of evolutionary theorizing about economic change. It is the oldest of the essays included in the book, published in the *Journal of Economic Literature* in 1995 (most of the other essays are from the period 2001–04), but it is probably still the best review article of the topic available to date. The subsequent two essays address the nature and evolution of human know-how and the role of institutions in shaping economic performance. Together these three chapters detail Nelson's view of how economic change should be understood: as an evolutionary process in which physical and social technologies co-evolve. These chapters also highlight an important difference in style between Nelson and North. Whereas North is single-mindedly focused on building his own argument, Nelson is much more careful to embed his view in the broader literature. This results in three chapters that are both very good guides to the literature and clear statements of Nelson's own views.

Nelson's proposal to understand institutions as 'social technologies', or patterned human interaction including 'the division of labor, the way work is coordinated and managed, and so on' (pp. 153–4) is useful, although his further proposal to understand social technologies in terms of 'routines' is unnecessarily restrictive (cf. Stoelhorst, 2005). The distinction between physical and social technology leads to an interesting discussion of why we have been much more successful in advancing physical technologies than in advancing their social counterparts. This is the topic of Part III of the book, where the following passage sets the scene for some interesting links to management studies:

Economists have long recognized the advance of human know-how as the central driving force behind the remarkable increases of living standards that have been

achieved over the past two centuries. However, less attention has been paid to the fact that the advance of effective know-how has been extremely uneven across different economic sectors and classes of human needs. Some areas of human know-how today are extraordinarily powerful . . . at the same time. . . many broad areas of human activity have seen little progress in know-how . . . Despite a lot of huffing in business schools and books on management, there does not seem to have been much improvement over the years in management know-how. Why not? (p. 173)

The chapters that follow are not an indictment of the quality of management research, but a convincing analysis of why it is so difficult to advance the sciences that underlie such social technologies as the management of organizations, or education. Part IV concludes the book on a similar theme as where it started: with a critique of neoclassical views. In this case Nelson targets the pro-market bias in economic theory and elaborates on his worries that policies informed by this market bias are beginning to negatively affect the scientific commons and the function of this commons as the motor of economic change.

EVOLUTIONARY THEORIZING IN MANAGEMENT

Organisations Evolving (2006) by Howard Aldrich and Martin Ruef is the second edition of a singly authored book by Aldrich that was originally published in 1999. Both editions continue Aldrich's search for 'an overarching framework that organizes an inquiry* into the issues surrounding organizational *change*' (p. xi; emphasis in original). This search goes back to Aldrich's earlier work, *Organizations and Environments* (1979), which has recently gone back into print. The goals of the 2006 book are threefold. The first is 'to write about the challenges of studying organizations, not just organization theory' (p. 3). This leads to an emphasis on research designs that go beyond an analysis of *Fortune 500* companies to capture the variegated nature of organizations. The second goal is 'to write about the emergence of organizations, not just their existence' (p. 3). This leads to much more emphasis on the early days of organizations, populations, and communities than is typical of most texts on organizations. The third goal is 'to write about the evolutionary-processes through which new organizations, populations and communities emerge, using an approach that cuts across academic disciplines' (p. 3). This goal is achieved by taking Campbell's (1969) variation, selection, and retention view of evolutionary processes as a framework to integrate insights from numerous streams of research in the literature on organizational change.

This book shows that evolutionary theorizing in organization studies has its roots in sociology rather than economics. The leading American sociology journals and the *Administrative Science Quarterly* figure prominently as outlets for the many publications referenced in this book. The book draws on work from population ecology, institutional theory, the interpretive approach, resource dependence, and transaction cost economics. In contrast to the books reviewed above, institutional theory here refers to work in sociology'- (e.g. DiMaggio and Powell, 1983; Meyer and Rowan, 1977; Scott, 2001) so that the main link to economics is through the new institutional economics in the style of Williamson's transaction cost economics. The book also makes clear that the 1970s were a remarkably fertile period for new theoretical perspectives. The six perspectives on

which this book builds all originated in this period, as did the modern version of evolutionary economics exemplified by IS-eTson's work.

All these theoretical perspectives are primarily concerned with the relationship between organizations and their environment, and there is an interesting passage in the book that documents Aldrich's struggle to find an adequate label for the perspective that he felt was emerging across these different theories in the 1970s. He admits 'to a certain inconsistency over the last several decades in labeling the research stream to which I was contributing' (2006, p. xii). The labels used include the organization-environment perspective, the resource dependence perspective, the natural selection model, population ecology, and the population perspective. The label 'evolutionary perspective' only came much later and is meant to convey something that is broader than the resource dependence or organizational ecology views and also captures entrepreneurship and organizational emergence. What this book documents is both Aldrich's evolutionary perspective as it took shape in the wake of the diversity of new theoretical developments of the 1970s and the insights in the phenomenon of organizational change that developed in 'a period of fervent empirical refinement' (p. ix) of these new theoretical perspectives in the 1990s and the early years of the twenty-first century.

The book is a remarkable achievement. Its one thousand references make it a wonderful guide to the literature, but it is much more than a literature review. Its main strength is that it succeeds in both organizing a large body of empirical literature and pointing to a number of issues that have been underexposed in extant research. Among these issues are the role of entrepreneurship in the emergence of new organizations, the transformation of organizations, and the emergence of new populations and communities. Each of these topics is treated in detail and in doing so Aldrich and Ruef not only bring together scholarship on organizational change, but also expand the frontiers of this scholarship. This dual achievement is strong testimony to the power of the evolutionary perspective to integrate and inspire empirical research and theory development on organizational change.

Organizational Evolution and Strategic Management (2006) by Rodolphe Durand links theory about organizational evolution to the body of literature in strategic management that is concerned with competitive advantage. The principal aim of this monograph is 'to provide a clear foundation for strategy and strategic management and to recast them in a general framework of organizational evolution' (p. 6). The book contains a critical assessment of organizational ecology in the tradition of Hannan and Freeman (1989), evolutionary economics in the tradition of Nelson and Winter (1982), the dynamic resource-based view (Teece et al., 1997), and the co-evolutionary approach (e.g. Barnett and Burgelman, 1996; Volbercla and Lewin, 2003). Its main concern, however, is to lay down the philosophical groundwork for better theories of organizational evolution and strategy. Durand's book:

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endeavors to scrupulously follow the biological traditions, to present their arguments and limitations, and to show that the current approaches to organizational evolution enjoy a complex genealogy. It strives to bring to the foreground questions regarding the ontological and epistemological implications of our use of evolutionary models, and to propose a tentative model that enables us to reconcile intellectual lineages that give every appearance of being incommensurable. (2006, p. 1)

This quote signals much of the content of the book, which is as much concerned with philosophical questions as it is with theory in strategy and organization as such. Part I, with two chapters that position the questions addressed in the book, is a somewhat slow start, but Durand is in full swing by Part II. Here he switches between detailed discussions of issues in evolutionary theory in biology, on one hand, and evolutionary theory in management, on the other. This results in the construction of a 'checklist appraisal grid' for evaluating the soundness of evolutionary models. Part III of the book presents Durand's view of strategy in an integrative 'organizational evolution and strategy model' and discusses its epistemological, theoretical, and managerial implications.

Durand's view of strategy as 'the necessary counterpart of organizational evolution' (p. 6) is convincing, and his in-depth analysis of crucial philosophical issues in evolutionary theorizing is unique in the management literature. Like Aldrich and Ruef, Durand acknowledges Campbell's variation-selection—retention model as central to evolutionary theorizing in strategy and organization. However, his main concern is to point out that a simplistic application of this model is not going to help organization theorists or strategists move their theories forward. Unlike Hodgson in his book reviewed above, Durand is in search of an integrative, transdisciplinary approach to theorizing in the social sciences that acknowledges the philosophical problems that are inherent to understanding organizational and institutional evolution. Durand's style is not as clear as Hodgson's, but he does have the ability to bring the philosophical discourse back down to earth with real world examples and applications of evolutionary insights to managerial concerns.

One of the ways in which Durand's philosophical approach does organization and strategy theorists a favour is by going back to the original contributions of Charles Darwin, Jean-Baptiste Lamarck, and Herbert Spencer. He highlights many commonly held misconceptions about evolutionary theory that originate from ascribing the wrong ideas to the wrong authors. Two of these misconceptions are especially widespread in management. The first is the false opposition between Darwinism and Lamarckism on the grounds that the former only allows for blind variation and puts all the explanatory power in environmental selection, while the latter sees evolution as a process that results from intentional adaptation to changes in the environment. Such use of the Darwinian and Lamarckian labels is not at all justified by the actual content of the work of these two evolutionary theorists. Neither is the idea that the Darwinian theory of evolution is about progress-towards some final goal. The notion of the 'survival of the fittest' is often mistakenly interpreted in these terms. One reason for this mistake may be that the inventor of this notion, Spencer, saw evolution as necessarily being progressive. Darwin never did. It was also the Spencerian view of evolution that inspired the mistakenly labelled 'Social Darwinism' movement, which was responsible for a number of misguided social experiments to 'improve' society in the early twentieth century.

In the last part of his book Durand faces up to the challenge of putting forward a model of strategy as organizational evolution. At this point he has made clear that Campbell's variation—selection—retention triptych is not enough to capture the role of intentional human action and the interrelationship between different levels of analysis in organizational evolution. Like Baur and Singh (1994) and Volberda and Lewin (2003) before him, Durand proposes a multi-level ontology that distinguishes a genealogical hierarchy and an ecological hierarchy. His criticism of previous proposals to capture organizational evolu-

tion in terms of such a dual hierarchy is convincing, but I do not think that Durand's 'tentative' model is going to be the *lasy-verd* on evolutionary ontology. A meaningful dual hierarchy identifies 'units of selection of in the ecological hierarchy and 'units of selection for' in the genealogical hierarchy (Sober, 1984). 'Units of selection *of*' (such as organisms) interact with their environment and 'units of selection *for*' (such as genes) retain information about what has worked in the past. It is doubtful that 'competitive advantages' can usefully be seen as 'units of selection of that interact with their environment. Nor am I convinced that it helps to portray markets as 'units of selection for' that retain information about what has worked in the past. The problem of identifying adequate units of selection in the social sciences will likely remain a contentious issue, but at least Durand's analysis forces us to consider fundamental questions like this. In that sense, his book is most certainly successful in realizing its stated goal of raising questions about the ontological and epistemological implications of our use of evolutionary models.

In *Strategizing, Disequilibrium, and Profit* (2006) John Mathews is even more focused on strategic management theory than Durand. Mathews also brings us back full circle to evolutionary theorizing in economics, albeit with an interesting twist. Like the books of the economists reviewed above, Mathews launches an attack on neoclassical theorizing. However, his target is not economic theory as such, but its application to strategy in the management literature. The aim of the book 'is to offer an account of strategizing by firms without introducing any of the equilibrium assumptions that are found in mainstream economics . . . My goal is to demonstrate that the interesting issues encountered in the strategy literature today . . . all share a common feature, namely that they are best illuminated and analyzed as disequilibrium phenomena' (p. 1).

Mathews is a good guide to scholarship in economics that is directly relevant to strategic management. He develops a view of strategy that builds on Schumpeter's (1934) theory of economic development, Knight's (1921) theory of profit, and Penrose's (1959) theory of the growth of the firm. This view is captured by the three concepts in the title of the book, a title that seems to be a play on Knight's famous study. The focus is on strategizing as an entrepreneurial process in a world that is in constant disequilibrium. In such a world entrepreneurs face uncertainty and make conjectures about profit opportunities. Strategizing is about acting on these conjectures by configuring resources, undertaking productive activities, and developing efficient routines. Such actions may lead to entrepreneurial profit when they are successful in exploiting increasing returns, resource complementarities, and organizational learning.

There are two reasons why this is an important book. The first is that it successfully articulates an explicitly dynamic view of strategy that is well grounded in previous scholarship in economics. The second reason is that it demonstrates the importance of such a dynamic view for the field of strategy. It does so in part through illustrations of real world strategizing by firms. Among the cases discussed are the DRAM and the Flat Panel Displays industries that Mathews has studied in detail. But the most important way in which the book makes its case for a dynamic view of strategy is by showing that the use of neoclassical equilibrium notions in mainstream strategy holds back theory development. As Mathews notes, '[Y]here is still the sense in the field of strategic management that we are "applying" the lessons of economics in the business world and that behind the strategizing of firms and their search for "rents" . . . there lies the solidity and depend-

ability of economic equilibrium and the methods of comparative statics' (p. 8). This is a very apt characterization of the view of strategy that is espoused in mainstream theories of strategy such as Porter-type I/O (Porter, 1981) and the resource-based view (Barney, 1991; Peteraf, 1993). It is insufficiently acknowledged that these theories are grounded in neoclassical equilibrium notions. Their explanations of performance differentials in terms of market imperfections in product or factor markets derive their rigour from industrial organization economics and Ricardian economics, respectively. And at the heart of these economic theories is the neoclassical view that models the firm as a black box and the market in terms of equilibrium. This neoclassical heritage makes mainstream theories in strategy incapable of developing convincing theories to explain or guide strategizing in dynamic environments.

At times Mathews overstates his case somewhat. Lending to set up mainstream economic views as the proverbial straw men and overstating the differences between Ricardian and neoclassical concepts, on the one hand, and Schumpeterian and Knightian concepts, on the other. Schumpeter's and Knight's approaches to competition derive their coherence from an explicit comparison to a perfectly competitive market in equilibrium. In that fundamental sense, equilibrium notions are a necessary touchstone for any theory of profit, even a theory that acknowledges the difference between entrepreneurial profits and rents. As long as we do not mistake models of perfect competition for a description of the real world, these models can serve an important theoretical purpose, not just in comparative static explanations of performance differentials between firms, but also in dynamic explanations. However, the overall argument of the book, that the most interesting aspects of strategy are better understood from a disequilibrium perspective is entirely convincing.

Whether the book will be successful in triggering the 'disequilibrium revolution' in strategy that Mathews predicts remains to be seen. A statement like '[t]he current approaches to strategy all utilize the doctrine of rents, which masks a miscellany of woolly and incoherent thinking' (p. 59) both points to an important problem in strategy research and is worth a smile, but it will hardly win established scholars in the field over to a disequilibrium perspective. And the 'provisional way' (p. 4) in which the book explores the 'new territory' (p. 4) of strategizing as seen from a disequilibrium perspective may yet need some additional work to achieve paradigmatic status in strategy research. The end result of the exploration in this book is the RARE framework, for Resources, Activities, Routines, and Entrepreneurial endeavour. While this spells out important building blocks for a dynamic theory of strategy, it does not as such offer a causal logic to explain specific outcomes of competitive interaction between firms. Much like North points to a road towards a better understanding of economic change, Mathews points to a road towards a better understanding of the dynamics of strategy. In both cases the authors present the building blocks for an evolutionary approach to the important problems they tackle rather than a full-fledged theory.

CONCLUSION

What do these six books tell us about the history and the state of evolutionary theorizing in the social sciences in general and management studies in particular? It may be clear

that their authors hold out the hope of an evolutionary social science that is able to deal with organizational, institutional, economic, and societal change. They also share a view that theories to explain such change must be sensitive to historical specificity and to the recursive causalities involved in the interaction between human agency and social structures. They are all evolutionary in the sense that they are interested in explaining how social systems transform themselves over time. And they all see an evolutionary approach to social science as a way to allow the integration of findings from disparate disciplines like psychology, sociology, and economics.

How little, therefore, we have progressed since Yeblen. and yet how far we have come. The history of evolutionary theorizing in the social sciences has been deeply affected by the abuse of pseudo scientific arguments for eugenics programmes advanced by social and political movements like Social Darwinism and Nazism. As a result, evolutionary theorizing became anathema to the post World War II generation of social scientists. A generation later there was a second setback with the controversy over the alleged biological reductionism of sociobiology. In the mid 1970s, social scientists reacted strongly to what many saw as an imperialistic attempt by Edward Wilson and his followers to reduce the social sciences to genetic explanations of human behaviour. Yet another generation later it is clear that the dust that was kicked up in this debate clouded many judgments. Today there are few remaining supporters of the blank slate view of human nature that social scientists felt compelled to defend against the perceived onslaught of biological imperialism. Human behaviour clearly has both a genetic and a cultural component, and both good science and successful policy depend on a better understanding of how these two components of human behaviour interact. If we have made only limited progress in advancing the research agenda outlined by Yeblen over a century ago, three generations later we are at least able to pursue this agenda without any of the misconceived ideas that evolutionary theory would be racist, reductionist, or for that matter, a theory of progress.

Against this historical background, the question why management is not an evolutionary science is easy to answer. It is not an evolutionary science because the social sciences have historically resisted an evolutionary approach. The social science heritage that we work with in management studies is one that has been handed down from the a-historical views of neoclassical economics and functionalist sociology that became dominant in the 1950s and 1960s. The many theoretical perspectives that emerged in the 1970s, including the new institutional economics, modern evolutionary economics, and organizational ecology can in part be seen as reactions to these a-historical views. However, a shared sense of the possibility of an integrated evolutionary social science is a much more recent phenomenon. In addition to the work reviewed here, advances in evolutionary epistemology, evolutionary psychology, evolutionary anthropology, and behavioural economics all contribute to the excitement that is building over the prospect of an evolutionary social science. As Hodgson observes: '[d]espite considerable resistance to its radical message from within both economics and sociology, the Darwinian movement within the sciences has now built up such a huge momentum that it is unstoppable. A vital task for the social sciences is to come to grips with the nature and importance of Darwinism' (2004, p. 450).

What the books reviewed here contribute to an evolutionary social science are important building blocks for an analytical framework that applies to different levels of analysis and that cuts across social science disciplines. They emphasize historical specificity, the interaction between human agency and social structures, the importance of intentionality and uncertainty, and the emergence and transformation of social systems. They also convincingly show that framing problems of organizational and institutional change in these terms leads to useful theorizing. At the same time, this theorizing typically takes the form of a narrative that is not always couched in a convincing causal logic. Evolutionary approaches in the social sciences still face the challenge of moving beyond informed description. To be able to do so, Campbell's variation, selection and retention framework is a good start, but it is not enough. What is needed is an explicit recognition of the recursive causal logic of an evolutionary explanation (Stoelhorst, forthcoming) and a convincing ontological commitment to specific units of selection in social evolution. And, in addition to a more explicit causal framework, we need to bring in psychology in a more substantial way than theories that focus on organisational and institutional change are wont to do.

Many have lamented the fragmented nature of theory in management studies, but this fragmented nature is simply a reflection of the fragmented nature of the social sciences in general. As the 'paradigm wars' have made clear (Pfeffer, 1993; Van Maanen, 1995), suggestions to move management studies forward by aiming for a more paradigmatic approach are unlikely to gain widespread support in a field that prides itself on its theoretical pluralism. If there are any lessons to be learned from the history of economic thought, management studies is right to be proud of its pluralism, and a hardening around a specific theoretical core should be avoided. But there is a middle way between fragmentation and paradigmatic orthodoxy. An evolutionary approach to social science insists on variation, selection and retention as a meta-theoretical framework for the study of social change, but it is entirely ecumenical about the auxiliary theories that are needed to fill in the details of these three mechanisms at different levels of analysis. Its metatheoretical framework would be able to guide process research and a narrative mode of theory building, as well as more quantitative research designs. And it holds the promise of a tightly knit system of theories that integrate insights from psychology, sociology, economics, and other social sciences. -

As Nelson notes of economics: 'Many years ago Veblen (1898) asked, "Why is economics not an evolutionary science?" In my view, economics would be a stronger field if its theoretical framework were expressly evolutionary. Such a framework helps us see and understand better the complexity of the economic reality. That, I think, is its greatest advantage. But it does not make the complexity go away' (2005, p. 114). Given its historical concern with the real world of organizations, and in contrast to economics, management studies has always been ready to embrace the complexity of socio-economic reality. But when it comes to addressing questions of change and transformation, the a-historical views of mainstream economics and functionalist sociology that have, often implicitly, informed much of the research in strategy and organization have limited value. Like economics, management studies would be a stronger field if its theoretical framework were expressly evolutionary.

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