Micro Strategy and Strategizing: Towards an Activity-Based View

Gerry Johnson, Leif Melin and Richard Whittington

University of Strathclyde Graduate School of Business; Jönköping International Business School; Saïd Business School, University of Oxford

ABSTRACT   This introductory paper provides a background to the origins, themes and papers of this Special Issue on Micro Strategy and Strategizing. Our overarching argument is that, while the field of strategy has traditionally concentrated on the macro-level of organizations, it needs now to attend to much more micro-level phenomena. We propose an activity-based view of strategy that focuses on the detailed processes and practices which constitute the day-to-day activities of organizational life and which relate to strategic outcomes. The paper develops this view by considering two bodies of theory, those of the resource based view and institutionalism; two bodies of empirical work, those on corporate diversification and structure; and finally the process tradition of strategy research. The paper identifies the benefits of the activity-based view and introduces some challenges for further research. It concludes by introducing the papers in this Special Issue.

INTRODUCTION

This paper argues for a shift in the strategy debate towards a micro perspective on strategy and strategizing. More specifically we are calling for an emphasis on the detailed processes and practices which constitute the day-to-day activities of organizational life *and* which relate to strategic outcomes. Our focus therefore is on micro-activities that, while often invisible to traditional strategy research, nevertheless can have significant consequences for organizations and those who work in them. In our call for papers for this special issue we wished to attract scholars who have a concern for 'the close understanding of the myriad, micro activities that make up strategy and strategizing in practice'. Indeed we have come to think

of such work as an activity-based view of strategy. As this introductory paper progresses, we develop this approach; and the papers that follow in this special issue help provide more specific examples of its significance and relevance.

This special issue has grown out of an interest in micro-level activity and practice that the editors, along with others, have been developing over the past few years (Johnson and Huff, 1997; Lindell et al., 1998; Whittington, 1996). The events that have led to the special issue give some measure of research community's growing attention to micro activity. The initial 2001 workshop, hosted by the European Institute for the Advanced Studies in Management, attracted over 60 submissions and was attended by about 50 scholars from around the world. This was followed by a substantial track at the second EURAM conference in Stockholm in 2002, at which some of the papers presented here were further developed. The call for papers for this Special Edition attracted 36 submissions and, apart from the ones published here, papers have been referred to standard editions of the *Journal of Management Studies*. Pettigrew, von Krogh and Grand have also organized related tracks at EGOS in 1999, 2001 and 2002. Other scholars are advancing similar themes, as we acknowledge later. There is, then, increasing momentum behind the micro-strategy and strategizing agenda.[1]

We shall show in the following sections how this momentum towards a more micro perspective responds to growing frustrations in the contemporary academic strategy literature. But we want first to recognize the urgent pull from practice. The more micro approach which we advocate here reflects both the changing economic environment in which we work and the nature of the actors with whom we, as teachers, interact.

The economic drivers for a more micro approach are twofold. First there has been a change in resource markets. The economic environment is moving rapidly towards open markets, mobile labour and information abundance. Resources are increasingly tradable and security from market entry and strategic imitation is falling. From a resource-based view (Barney, 1986, 1991), the consequences are clear: the transparent becomes a precarious foundation for competitive advantage. In these fluid resource markets, sustainable advantage must lie in micro assets that are hard to discern and awkward to trade. Profit, not just the devil, lies in the detail. The second driver reflects the shift to a much more 'hypercompetitive' environment in which speed, surprise and innovation are the winning bases of competitive advantage (Brown and Eisenhardt, 1997; D'Aveni, 1995). Hypercompetition impacts both the level and the frequency of strategy activity. Fast and innovative responses to competition require organizational decentralization, so that strategic decisions are taken by the line-managers who are actually close to customers and skills (Whittington et al., 1999; Zenger and Hesterley, 1997). Strategic innovation increasingly involves managers at the periphery, rather than just those at the centre (Johnson and Huff, 1997). The hypercompetitive pace of change also collapses strategy-making from the measured cycle of well-defined

episodes into a much more continuous process (Brown and Eisenhardt, 1999). As a consequence, strategy-making becomes a chronic feature of organizational life. With this dual impact on both levels and frequency, hypercompetition makes of strategy something in which more people are involved, more often, than ever before.

On top of these changes in the economic environment, there is increasing pressure for a more micro perspective from the actors with whom we engage. No doubt in part reflecting the changing economic environment, managers and would-be managers are becoming more demanding in terms of their expectations from business schools. Their frustrations are recognized in the growing debate around connecting academic research with practice (Huff, 2000; Starkey and Madan, 2001; Tranfield and Starkey, 1998). Part of our problem has been a macro focus in research that is remote from what the managerial actors with whom we interact really influence. Quite simply, a strong instrumental reason for the importance of a more micro activity based view of strategy, therefore, is that managers manage activities. If we are to aid management and the managing of organizations we need to achieve a higher degree of reflexivity amongst those actors about what they are doing at that level and its effects. Much of the influential literature on strategy, important as it is, has left the manager bereft of insights, let alone guidelines for action, at this micro level.

In the next section we shall show equivalent pressures for a micro perspective within important contemporary streams of strategy research, in particular the resource-based view and institutional theory and the issues of diversification and organizational structure. If both the worlds of practice and academic research demand a more micro perspective, then the task is one of empirical investigation. We go on, therefore, in the following section to explore the potential of the 'process' perspective (Pettigrew, 1985; Johnson, 1987) for approaching micro issues in strategy. While recognizing important contributions, we argue that the process literature is still insufficiently sensitive to the micro. The final parts of this paper, therefore, outlines the case for what we propose as an 'activity-based view' on strategy.

The activity-based view that we shall outline starts from the proposition that value lies increasingly in the micro activities of managers and others in organizations. Thus the activity-based view is concerned with the consequential details of organizational work and practice (Whittington, 2003). It goes inside organizations, their strategies and their processes, to investigate what is actually done and by whom. We recognize that this activity-based view is demanding in terms of research and we outline some of the key challenges that face us. However, we believe that there are substantial pay-offs to grasping these micro activities and we take encouragement from the other papers in this special issue. We introduce these other papers in the final section of this paper: they indicate a fascinating – and challenging – agenda for further research on micro-strategy and strategizing.

PUTTING THE MICRO IN THE MACRO

This section will argue that progress on many 'content' issues in strategy (Rumelt et al., 1994) will rely increasingly on taking a micro perspective. In advocating this micro perspective, we do not intend to dismiss the contribution of traditional content strategy research. It has introduced important concepts and insights to practitioners and academics alike. However, a good deal of this research has confined itself to macro-level analysis. It has trapped itself into a cul-de-sac of high abstraction, broad categories and lifeless concepts. Researchers in the macro tradition are themselves increasingly acknowledging that the way forward lies not simply in the more sophisticated construction and manipulation of such abstractions, but in a more direct confrontation with the complexities of managerial and organizational action. We argue that a more micro perspective on strategy and strategizing can take forward existing questions at the same time as opening up new.

We illustrate the frustrations of the traditional macro approach by examining four influential bodies of work informing the subject: first, two important theoretical perspectives, the resource based theory and institutional theory; second, two foundational topics in empirical strategy research, the content issues of corporate diversification and structural design. We choose these four examples not just because they are so central to the contemporary strategy discipline: as we shall see, the existing weaknesses of each are clearly traceable to a simplifying detachment that leaves out activity.

The Contribution of Theory 1: The Resource-Based View (RBV)

The resource-based view is important to our argument. Within the strategy field more widely it has of course provided an agenda that promises much in terms of its insight into the value-creating capacity of organizations and the sources of sustainable competitive advantage. The resource-based view is, then, particularly significant here because our approach, insofar as it is concerned with organizational practices and activities that could presumably constitute bases of competitive advantage, is central to some of the basic concerns of a RBV view. However, the debate over the resource-based view has so far been largely conducted either theoretically or empirically at the macro-level, with large-scale statistical studies the weapon of choice (Bowman and Helfat, 2001; McGahan and Porter, 1997; Rumelt, 1991). With no firm conclusions emerging, the macro approach to the resource-based view is in danger of failing to deliver against its promises.

First, macro research on the resource-based view suffers from its broad categories. Priem and Butler (2001) complain that the definition of resources is typically all-inclusive, poor at discriminating between those resources that managers can practically manipulate and those beyond managerial control. The large-scale

statistical studies preferred in the literature impose upon the discipline a flat, featureless characterization of resources, ironic given that their value is supposed to lie precisely in their uniqueness (Rouse and Daellenbach, 1999). Second, much research from the resource-based view marginalizes the activities, managerial or otherwise, that go on in organizations. The questions of how valuable resources are built and how they generate superior returns are left undisturbed in the 'black box of process' (Priem and Butler, 2001). The value of a resource depends not on its existence but on its utilization. Here a shift to a sensibility of verbs rather than an assumption of nouns is part of the solution. As Eisenhardt and Santos (2002) observe of recent knowledge-based interpretations of the resource-based view, the need now is to move from a preoccupation with knowledge to a theory of knowing. For Rouse and Daellenbach (1999), finer appreciations of resources, and greater understanding of managerial action, rely upon close fieldwork and 'thick description'. After all, they recall, Penrose's (1959) original insights arose from her detailed case study of the Hercules Powder Company.

A micro perspective takes up these challenges. It highlights the value generated in the seeming minutiae of organizations, and in the periphery as well as the centre – after all, an insight of the resource-based view is that the roots of superior performance lie in the unique and hard to copy, attributes that are not likely fit into broad, all-inclusive categories or leap out to the detached observer. The micro-strategizing agenda is appreciative of action because it recognizes that managerial activity and those involved in the activity of organizations – whether managers or not – are essential to the actualization of potential value. The resource-based view will advance as it shifts towards a micro-perspective capable of capturing both details and activity.

The Contribution of Theory 2: Institutional Theory

The contribution of institutional theory has also historically tended towards the macro – a concern with 'the behavior of organizations as entities and the nature and effects of their formal and collective parts' (Tolbert and Zucker, 1996, p. 75). It has provided some fascinating insights, some of which counterbalance the implications of the resource based view (Deephouse, 1999). Organizations seek for legitimacy rather than competitive advantage; there are isomorphic strategic tendencies rather than a search for differentiation; and indeed this may be beneficial to organizations. However, in seeking to explain these observed phenomena, DiMaggio and Powell (19991, p. 16) admit: 'Most institutionalists prefer to focus on the structural environments, macro to micro-level effects, and the analytic autonomy of macro structures'. The bias of institutionalism is towards the macro.

This macro bias is perhaps surprising, since the acceptance by institutional theorists of a socially constructed world suggests that individual actors and groups of

actors play a significant part in institutional processes. So much is acknowledged by Barley and Tolbert who argue that we can understand institutions in terms of the: 'history of negotiations that lead to "shared typifications" or generalized expectations and interpretations of behavior' (1997, p. 94). There is, however, little evidence in institutional literature of an exploration of such 'negotiations'. The concern has been to understand organizations in terms of norms and rules: and the emphasis has been on how individuals are captured within these, as distinct from the role they play in creating and amending them.

There are those who recognize the need to introduce more micro level explanation into institutional theory. In principle, it underpins much of what has become known as neo-institutional theory (DiMaggio and Powell, 1991). As yet, though, there is little empirical work that engages with a truly micro level. Scott's (1994, 1995) plea for the need to understand the influences of meaning systems and cognition on institutional processes argues the need to address the issue of the influence of organizational actors. This is a view echoed by Weick who argues that: 'Sensemaking is the feedstock for institutionalisation' (1995, p. 36); and he has done much to provide insight into how this might be. If institutionalization assumes shared sensemaking, we need to understand this as a recursive process of enactment to be explained in terms of how organizational actors influence and are *influenced* by organizational rules and norms. For example, Barley and Tolbert (1997) and Johnson et al. (2000) seek to understand links between 'action and institution' by employing the concept of scripts.

There is then the potential to understand insights from institutional theory by building a bridge into the sensemaking literature and undertaking empirical work which links the two. If this is to be done, the need is to examine sense making as organizational activity (Eden and Ackermann, 1998); but with few exceptions, such research is not yet much in evidence.

We have argued so far that at least two central theoretical traditions within strategic management would benefit from a more micro orientation. Next we shall show how progress in two longstanding areas of empirical research – diversification and organizational structure – also relies upon an increasing sensitivity to micro-level activity.

Empirical Contributions 1: Corporate Diversification

The theme of corporate diversification dates from the discipline's foundational work, Alfred Chandler's (1962) *Strategy and Structure*. Yet decades of macro-orientated research have done very little to establish any positive relationships between diversification and performance. Grant concludes a recent review of diversification research thus: 'After nearly half a century of research, the advice we academics can offer managers in designing and implementing their corporate strategies is tentative at best. More than a hundred academic studies have failed

to determine if diversification enhances profitability or whether related diversification outperforms unrelated diversification' (2002, p. 91).

Two reasons for this failure of the macro approach to diversification are close to our argument for a more micro and active way forward. In the first place, it is clear that the traditional metrics rely too much on convenience. Synergistic relatedness or its absence is typically measured either by SIC-codes designed for quite other purposes or the categories developed 30 years ago in Richard Rumelt's PhD thesis (Rumelt, 1974). As Nayyar (1992) and Stimpert and Duhaime (1997) have found even form simple surveys, managers' perceptions of true economic relationships can differ widely from those presumed by SIC codes or Rumeltian categories. The standard frames of traditional macro research are too crude to capture the subtleties of synergistic relationships.

The second problem in diversification research recalls Priem and Butler's (2001) critique of the resource-based view: we cannot assume that apparent relationships are really exploited in practice. Nayyar (1992) makes the useful distinction between potential and actual relatedness, pointing to the role of managerial action in actualizing the latent economic value of these relationships. As Tsai (2000) suggests, it takes the existence of active social networks realized by people working together for real value to be extracted from strategic relationships. Moreover, relationships have to be managed and renewed if their value is not to decay (Markides, 2002). The activities involved in realizing and renewing relationships are not to be observed from a distance. As Grant et al. (1988) concluded some time ago, progress in diversification research requires small-sample, fine-grained investigations capable of capturing both the subtlety of economically-valuable relationships and their sensitivity to managerial action or inaction.

Empirical Contributions 2: Corporate Structures

The history of macro research on the design of corporate structures illustrates very similar limitations to that of diversification. This stream of work has the same origins in Chandler's (1962) foundational work and has attracted an equivalent volume of large-scale statistical studies. It has also arrived at an equivalent failure to establish unambiguous performance relationships, especially around the advantages of the multidivisional structure and its fit with various diversification strategies (Whittington, 2002). Again, there are problems both with crude measures and with the exclusion of action.

First, the traditional categories of organizational design are too broad to differentiate between significant variations in structure, for instance between various types of multidivisional (Markides and Williamson, 1996). Multidivisional structures may be more or less centralized, with significant effects (Hill et al., 1992). Second, structural categories are too static to pick up the continuous structural changes of contemporary business (Brown and Eisenhardt, 1997; Galunic and

Eisenhardt, 1994). To take one instance, the European engineering giant ABB underwent two major structural changes in the decade following its creation in 1988, but neither would have been recorded as taking the company away from its original basic structural category of multidivisional (Achtenhagen, 2001; Ruigrok et al., 2000). If structures are in near continuous motion, then we need a better appreciation of the activities involved in creating and implementing structures – structuring. This is the more important because, while we have only equivocal evidence regarding the relative performance of structures at points of time, we do have evidence for the potentially negative impact of episodic structural change (Brickley and Van Dunen, 1990; Whittington et al., 1999). Yet, as Bowman et al. (1999) note, organizational restructuring has attracted negligible research in the last decades. We know very little about the actual managerial activity involved in designing new organizational structures (Bate et al., 2000).

The disappointments on both diversification and design have their roots in a similar problem: the prioritization of nouns over verbs (Garud and Van de Ven, 2002). As Grant et al. (1988) observed, the word 'diversification' refers also to a process of change – movement towards increasing diversity – but what we tend to study is diversification as a state. The same is true of 'design': we typically interpret it as a noun, rather than the verb involved in the structuring process. In other words, these two central traditions in strategy research leave out action.

In sum, key theoretical and empirical traditions within the macro approach have arrived at a similar conclusion: the need to put the micro into the macro in order both to uncover plausible linkages to performance and to offer tangible guides to managerial action. Some important insights on micro issues of managerial activity have already been offered by the process tradition of strategy research and it is to this tradition that we turn next. Understanding both the contributions and the limitations of the process tradition puts us in a better position to develop a distinctive agenda for the micro-strategy and strategizing perspective.

THE AMBIVALENT LEGACY OF PROCESS RESEARCH

Since the 1970s, strategy process research has made substantial contributions to the strategy discipline. The initial steps were taken simultaneously in Canada (Mintzberg, 1973; Mintzberg et al., 1976), the UK (Johnson, 1987; Pettigrew, 1977) and Sweden (Jönsson and Lundin, 1977; Melin and Hellgren, 1993; Normann, 1971, 1977; Rhenman, 1973). We are strongly sympathetic to this process turn in the strategy field and we shall highlight three major contributions that we want to hold on to. However, we shall also identify some significant limitations that help establish our own agenda for the micro-strategy and strategizing perspective.

First, on the credit side, the process school has irrevocably opened up the black box of the organization. Strategy is now recognized as an organizational

phenomenon rather than a macro strategy problem detached from the internal dynamics of the organization. Internal politics (Pettigrew, 1977) and organizational tensions (Normann, 1977) have been introduced into the strategy process, not just as inevitable failings of organizations, but as significant for strategy outcomes, even attributes to be exploited positively for strategy development. We wish to stay inside this black box, indeed delve even further into it.

A second contribution of strategy process research is to have 'humanized' the field (Pettigrew et al., 2002, p. 12). Unlike studies which purport to shed light on key organizational phenomena, such as top teams, by reducing them to sets of demographics, the process studies have populated the arena of strategy development with human beings. All forces and activities driving or counteracting change emerge from human actions. With the application of social construction and socio-cognitive perspectives, process research has demonstrated the potential to capture micro aspects of strategic actions made by human beings. The implications, that we will come back to, are that strategy researchers must be in direct and close contact with the actors, and that theoretical constructs must, figuratively speaking, be filled with people. We want a theory of social action within the strategy context.

A final important contribution of strategy process research is the legitimation of small sample in-depth studies. Single case studies such as Pettigrew's (1985) or the close observation of managerial work by Mintzberg (1973) have proved to be the source of rich and enduring insight. Such an approach allows us to develop the holistic and contextual understanding essential to unpacking the complex driving forces of strategic change and stability (Melin, 1986, 1989). These in-depth studies are not only valid in themselves, but vital complements to the large-scale, aprocessual studies typical of traditional strategy performance studies. In-depth studies will be a necessary feature of the micro-strategy and strategizing perspective as well.

However, we need to be alive to the limitations of the process tradition. There are six points to be made, which together set some tough challenges for our own micro-strategy and strategizing perspective.

First, while they have opened the black box of organizations, the process researchers haven't dug far within. A good deal of process research relies on second-hand retrospective reports, given typically by senior executives. Too often it is reminiscent of the early anthropologists' accounts of tribal customs based on conversations with local chiefs on colonial verandas. Progress in anthropology involved ethnographers directly engaging with – indeed living in – the world of action and practice. Something similar is true for management studies. Part of the problem is the level of analysis. Brown and Duguid (2000) have observed that practice is what is inside the process. In defining process research as about the systems and processes of organizations as wholes (Chakravarthy and Doz, 1992), the process tradition does not oblige us to probe far into what is going on inside. Process research might tell us a good deal about the overall processes of organizational decision-making and organizational change, but it has been less interested

in the practical activity and tools necessary to make these processes happen. What managers actually do, and with what techniques, is left obscure. The process tradition has tended to fix upon the organizational level at the expense of the practical activity of those who actually constitute the processes. If we want to grasp the micro activity of practice, we shall need to get off our 'verandas' and get a good deal closer to the actual work that makes up the organizational systems and processes of the process tradition.

Second, process research has been reluctant to query the role of managerial agency (Pettigrew, 1985). Even if some studies have shown that managers can be central to strategizing (e.g. Hellgren and Melin, 1993), we still need to explore further if, and how, they make a difference. As our principal point of contact, we may easily exaggerate the importance of managers, particularly those at the strategic centre, whereas activity at the periphery can have crucial effects (Johnson and Huff, 1997). Moreover, managerial agency is dependent on both general and situational constrains (Whittington, 1988). Managers can be captured in wider belief systems and technologies that constrain their possibilities for action. As both institutionalist theorists (Abrahamson, 1996) and critical theorists (Knights and Morgan, 1991; Sveningsson, 1999) suggest, micro activities may easily be dominated by macro societal forces. In exploring how this might be so, an activity-based view should be neutral on the question of managerial agency.

A third challenge for process research is the extent of its practical implications. Does it really help managers in their daily strategizing activities – for example, how to run a strategy meeting or engage in everyday strategic debate in a convincing and motivating manner? In so far as the process tradition has been prescriptive, the typical focus has been on the overarching design of strategic change or decision-making processes, remote from what mantagers are doing in the day-to-day (Hickson et al., 1986; Pettigrew and Whipp, 1991). Beyond that, process case studies have provided rich descriptions, but largely left to the reader the hard work of interpreting these into practice. These process case studies may be food for reflection, but the fare has been pretty indigestible. The challenge for an activity based view will be to transform descriptive contributions into more helpful models of managing.

A fourth limitation in process research is due to its separation from content issues such as diversification or structure – indeed, the process tradition has tended to define itself as the opposite of content (Chakravarthy and White, 2002). When demonstrating that managers are networking, communicating, politicking or deciding (Kotter, 1982; Mintzberg, 1973), process studies don't show in what regard. We still know little about the processes of diversifying, structuring, allying or internationalizing (Melin, 1992). There is too sharp a dichotomy between process and content. A major implication of the micro strategizing perspective is the dismantling of the content – process dichotomy where content instead is regarded as an inherent and indissoluble part of ongoing processes.

The fifth limitation derives in part from the separation from the content approach, which has traditionally focused on organizational performance. Although there are notable exceptions (Pettigrew and Whipp, 1991), process research too often lacks explicit links to strategy outcomes. We should recognize that process research is dealing with a complex and ambiguous reality, meaning that we have to deal with networks of mutual causation (Melin, 1987). The same is true for a micro-strategy perspective, but at least it is helpful to be clear about what we are trying to explain and in what terms – a point we take up later.

Finally, processual case studies are too often trapped within the particular. The empirical richness of much process research is both a strength and a weakness. We have many thick descriptions, but too few rigorous analyses and systematic comparisons around general theories or issues. The result has been poor accumulation of knowledge. Rather than emphasizing the rich idiosyncrasy of their data, case study researchers should use their empirical observations to establish patterns across similar issues and build theories with greater leverage in the real world. Data in itself is less valuable than theory that can explain why, how, with what consequences and in what circumstances. Again, this is a challenge we return to later.

In sum, the process tradition has got us some way into the black box of organizations, but we have further to go if we are to truly understand the micro-activities that make up organizational processes and contribute to organizational value. In good part, the agenda for the micro-strategy and strategizing perspective is set by the limitations against which the process tradition has run. The micro-strategy and strategizing perspective should get much closer to the detailed activities that go on inside organizational processes. It should incorporate a broader context simply than the organizational, recognising how managerial activity depends upon norms and technologies that are defined at the societal level as well. The micro-strategy and strategizing perspective should be practical, capable of actually helping managers do their work differently. The issues of strategy content and performance need to be connected to micro-activity. To do all this, the micro-strategy and strategizing perspective will have to go much deeper into organizations, working from the kind of systematic theoretical base that will allow the accumulation of practical knowledge.

TOWARDS AN ACTIVITY-BASED VIEW OF STRATEGY AND STRATEGIZING

As we have argued, virtually all the constituents of the strategy debate warrant a more micro level of explanation and therefore of analysis. This is true for the resource-based view, institutional theory, research on diversification and structure and even the process tradition. We would argue that the same applies to the challenges of product development, the problems of acquisition and acquisition integration, the management of multinational businesses, the corporate control of

businesses and many other important topics in the strategy literature. All have tended to be explained in terms of the outcomes of what goes on in organizations, rather than the activities which constitute them. Micro strategy and strategizing is concerned with the same strategic issues, but in terms of the organizational activities and practices which are their fabric; to repeat our opening definition, with 'the detailed processes and practices which constitute the day-to-day activities of organization life *and* which relate to strategic outcomes'.

Our agenda is distinct in that it comes at enduring issues in strategy from the bottom-up, from the activities that constitute the substance of strategic management. We acknowledge that others adjacent to the strategy field have a similar agenda. For example, in relation to management and organizational cognition, Walsh (1995) argues that it is necessary to understand it in terms of people's behaviour; Weick and Roberts (1993) show that collective cognition is embedded in organizational activities; and Eden and Ackermann (1998) argue for close engagement with practice to understand the relation between cognition and strategy. Within organization theory more widely, Blackler et al. (2000) have also recently been developing an explicitly activity-orientated approach, based on the activity theory of Engeström. Our argument is parallel, but more specific to strategy. It is time to shift the strategy research agenda towards the micro; to start not from organizations as wholes – corporations, business units and so on – but from the activities of individuals, groups and networks of people upon which key processes and practices depend.

The Benefits

The benefits of an activity-based view of strategy are at least threefold: extending existing traditions of research; transcending divisions within the discipline; and offering practical, actionable guidance to practitioners.

First, and in line with needs expressed in many fields of strategy research, there is a need to link macro phenomena with micro explanations. An activity-based view sets out to do just this. It does not deny the importance of research that has raised to the awareness of managers and researchers alike key macro issues facing strategists; the challenge of achieving and sustaining competitive advantage, of identifying and building on unique organizational assets or competencies, of managing multinational organisations or diversifying and acquiring businesses successfully are all informed by such work. However, here we respond to the call from within those very studies of the need to extend explanation to the practices and activities which underpin and constitute such phenomena. The macro work on resources, institutions, diversification and structure all leads in the same direction, towards a greater sensitivity to micro activity within organizations.

In so doing an activity-based view does away with the content/process split which has riven the strategy debate for so many years. The activities and processes

which underpin strategy content are equivalent to those which explain strategy development or the management of strategic change. The level of analysis is the same. By emphasizing this activity-based perspective we are able to overcome – indeed the perspective demands we overcome – this established but regrettable divide. We can apply similar lenses to both such classic content issues as diversification and structure and the central process issues of strategic decision-making and change.

A third benefit is that it is activities that are the day-to-day stuff of management. It is what managers do and what they manage. It is also what organizational actors engage in more widely. So the research agenda matches the lived world of organizational actors. At least potentially it therefore provides the opportunity to translate research findings into organizational action more directly. As academics, we may make a bigger impact at the micro level than at the traditional macro level.

This does, however, raise an important question, which is the extent to which these practices and activities are actually manageable. On this an activity-based view is agnostic or neutral. It does not presuppose the primacy of managerial agency but rather encourages the exploration of the centrality of management within the complexity of the processes that go to make up and influence organizations. An activity-based view of strategy allows for, but does not commit to, managerial agency. What it does, however, is encourage reflexivity by both academics and managers on the role and effect of managers within the more inclusive notion of organizational activity; and in so doing avoids breaking out the concept of strategy as somehow separate and distinct from that world.

The Challenges

The activity-based view offers an agenda worth following, but there are major challenges ahead: we highlight four.

First, one advantage research at a macro level has is that the dependent variable is fairly clear: the aim is to explain organizational performance. This leaves the question of *what an activity-based view is trying to explain*. One legitimate concern is the macro phenomena that micro activities are explaining; and in this sense there may be a link to relatively conventional performance measures, if second hand. For example, the debate on competitive advantage informed by the resource based view poses the challenge of how resources in the form of routines, assets and processes are configured to provide sustainable rents. It may not be possible to link this directly to organizational performance, but an activity-based view might be able to demonstrate how configurations of such assets take shape. Mapping technologies for identifying such asset-shaping activities have only recently emerged and are now being applied to questions of competitive advantage and sustainability (Ambrosini and Bowman, 2002; Eden and Ackermann, 2000; Johnson and

Johnson, 2002). Other macro issues, such as diversification or organizational restructuring, lend themselves equally to a micro analysis of the activities by which they are done.

An alternative focus is for the activity-based view to concentrate on the effectiveness of practices and practitioners in more immediate terms. Just what do managers have to do to make a difference and what is their impact? What works for them and what does not work? Here appropriate measures are not so much overall economic performance as recognition of managerial influence, the adoption of strategic initiatives, or perceived success of strategy-making episodes. A good deal of the literature on leadership already shades into this area by discussing the activities and behaviours of effective leaders (Grint, 1999). We have already noted the difficulties of the mainstream strategy literature in finding consistent economic performance relationships at the organization level. There might be higher returns by extending the leadership tradition to explaining the effectiveness of strategy managers, projects and techniques.

Of course there are those who would argue that the search for some sort of dependent variable is irrelevant or pragmatically premature. A less ambitious but nonetheless worthwhile aim for an activity-based view might simply be to encourage reflexivity. Research that exposes and explains the consequential complexities of strategic activity is unlikely to yield many easy prescriptions. It can, though, provide moments for reflection. In explaining complexity, the activity-based view may contribute to the formation of 'reflective practitioners,' more subtle and sensitive in their strategic practice (Schön, 1983).

A second concern is that of *knowledge accumulation*. As noted earlier, a criticism of the case study orientation of process research is it too often fails to accumulate insight and knowledge beyond the specific context of the particular study. The challenge for micro studies of strategy activities is to rise beyond the specific.

An agenda for more cumulative work can be taken directly from prior research in the strategy field. The content tradition gives us major issues to study at the micro level, diversification, restructructuring and much more. Process studies have contributed further crucial questions, such as strategic decision-making and change. Linking micro studies to such prior empirical work is both possible and desirable. Moreover, micro research can draw on extant bodies of theory to increase their leverage, just as the better process studies have done. There is no shortage of theoretical perspectives upon which to build. Certainly the resource-based view provides a theory just as relevant to a micro approach as a macro approach. But elsewhere we have cited researchers who draw on structuration theory, institutional theory, complexity theory, evolutionary economics, social psychology and cognition. All of these theoretical approaches offer frameworks within which knowledge can cumulate and be systematically compared.

There are also challenges to do with the *design of research*. Almost inevitably micro studies have to be constrained in terms of their scope and unit of analysis.

However, it is possible for researchers to identify particular units of analysis that can contribute to the more general. These could include the events or episodes that are typically critical to strategy development, for instance board-meetings or away-days; activities and processes which commonly underpin and explain competitive advantage, for example the transfer of knowledge; or the ways in which the standard tools and techniques of strategy, from portfolio planning to Porterian analysis, are actually employed. While each of these can be studied quite minutely, wider resonance is given because these are activities that are common and comparable across many organizations. Here it is important that such fine-grained studies are located in their wider context. They need to span levels: the level of individual interaction, the organizational level and the level of the organization's context. This is a significant challenge. The danger is that studies veer to one extreme or the other: they are so micro – so concerned with explaining detailed processes – that they have little to say about how such processes contribute to general outcomes. Or more commonly, they claim to address micro issues when in fact they are merely deducing them from macro phenomena or outcomes.

Such work also requires a *close engagement with practice* rather than a reliance on surrogate measures. The challenge is to uncover strategic activities in their real rather than just their reported form. Most process studies actually rely on retrospective accounts of process by the actors involved. As anthropologists have long known, such accounts do not always reflect what actually goes on. The onus is on the researcher to provide convincing evidence that such processes and activities have been captured as accurately as possible; or at least that retrospective accounts are convincingly crosschecked. In any case, engaging closely with practice at this micro level will typically require the cooperation of practitioners. The plea to regard organizational actors, not so much as subjects of study, but as research collaborators is growing (Eden and Huxham, 1996; Tranfield and Starkey, 1998). An activity-oriented approach to research will benefit from the joint production of knowledge – not just studying practice, but directly involving practitioners.

This is a challenging but, we believe, worthwhile agenda around which, we hope this Special Issue will help galvanize further work. We turn now to a brief explanation of the papers which follow.

The Papers in the Special Issue

We have not been able to include all the papers worthy of inclusion in this Special Issue. What we have tried to do here is to include papers that we think provide a useful, diverse and interesting range of insights on the agenda we set. An important criterion was how they advanced the micro perspective empirically, either by illustrating useful empirical methods in their own research or, in the case of the papers by Balogun et al. and Hendry and Seidl, by offering helpful methodologi-

cal guidance for the future. We hope these papers will prompt more empirical research in the future.

The paper by Paula Jarzabkowski uses Engeström's activity theory to explore the interrelation of organizational actors, structures and systems in the making of strategy and strategic change in three UK universities. She takes up our earlier challenges in several way. She carefully relates what she refers to as organizational practices to the macro phenomenon of strategic change. She allows for the accumulation of knowledge by using a comparative case method and by developing a framework capable of extension to other situations. Finally, her research involves close engagement with her subjects, while also maintaining a cautious neutrality on the role of managerial agency.

Patrick Regnér's interest is in the different roles of the centre and the periphery in organizational innovation. Thus he explicitly links the process issue of strategy formation with the content issue of innovation. He does this, moreover, with a micro lens that exposes the critical influence of the periphery and the detailed activities undertaken there. All this is achieved within a systematic comparison of several Swedish cases that allows him to build a distinction between inductive and deductive modes of strategy-making capable of being applied and developed over further examples.

Carlo Salvato links the resource-based view with micro-activities in his evolutionary account of two successful Italian firms. In both of these firms, he identifies 'core micro strategies' consisting of routinized activities carried out at a level of detail that would be easily missed by competitors or detached researchers. As such, these core micro strategies offer bases for sustainable advantage over many years. The significance of Salvato's research is both to emphasis the microfoundations of competitive advantage and to sensitize managers to the careful adaptation over time of routines that they could easily either miss or take for granted.

The practice of strategy-making, or rather the failure to make strategy, is the concern of the paper by Sally Maitlis and Tom Lawrence. They provide a longitudinal case study of a UK symphony orchestra on the basis of which they build an explanatory model of such failure around strategizing episodes in which organizational discourse and politics come centre stage. As well as offering a detailed account of strategy development, a strength of this paper is its clear propositions regarding the conditions under which strategy-making episodes are likely to fail. Maitlis and Lawrence thus provide a strong foundation for further research on successful and unsuccessful strategy-making episodes.

This theme of strategy making as discourse is picked up at the most micro level in the paper by Dalvir Samra-Fredericks. This draws on the 'talk' of board-room strategists encountered by the author in her ethnographic study of a manufacturing investment decision. In particular the paper considers how one particular strategist becomes influential through effectiveness in a particular strategic con-

versation. Samra-Fredericks' contribution lies both in identifying six critical characteristics of effective strategy talk and in developing a detailed analytical method for further research on strategy talk.

John Hendry and David Seidl do not provide an empirical account, but their careful theoretical framing of strategic episodes, such as board meetings or strategy away-days, does offer systematic guidelines for future empirical research. Drawing on the German social theorist Niklas Luhmann, they highlight important elements in the initiation, conduct and termination of strategic episodes. By identifying a common unit of analysis and in providing a theoretical frame for systematic comparison, Hendry and Seidl suggest possibilities for empirical inquiry that promise both convincing detail and practical pay-offs.

The final paper by Julia Balogun, Anne Huff and Phyllis Johnson specifically addresses the methodological challenges of undertaking work on strategy at the micro level of activity. They address the need to uncover processes of strategic activities in their real settings, going beyond retrospective studies and involving the close engagement with organizational actors as research collaborators rather than passive informants. They describe and discuss three methodological approaches: interactive discussion groups; self-report methods, including personal diaries of strategic actors; and practitioner-led research involving practitioners as researching their own practices. Researchers will find here many practical and inspiring ideas for taking forward the activity-based view in the field.

We do not propose that these papers respond definitively to all the challenges we have set for an activity-based view. Nor do we claim that the activity-based view comprehends all the issues raised in these papers. Our sub-title is tentative and only suggests progress in the direction we advocate. However, we are confident that collectively these papers do add significant insights beyond those of the established content and process approaches to strategy. They do so particularly by grappling closely with the detailed activities involved both in the building-blocks of competitive advantage and in the processes of strategy-making and implementation. We hope they will provide a stimulus for others to continue and develop this emergent appreciation of the intimate realities of strategy practice.

NOTE

[1] A related website has recently been established: www.strategy-as-practice.org.

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