Commentary

A Hegelian Dialogue on the Micro-Foundations of Organizational Routines and Capabilities

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This paper aims to further the alignment among different theoretical approaches and future scholarship on the complex themes related to the micro-foundational processes characterizing the emergence and development of organizational routines and capabilities. It has been constructed with a typical Hegelian structure represented by a thesis, an antithesis and an attempt of a synthesis, each presented by different scholars, primed by a common set of questions related to the role of individual actors in shaping organizational change processes. The dialogue is introduced by Koen Heimeriks, followed by Sidney Winter proposing the ‘thesis’ for an evolutionary perspective on the problem, and an ‘antithesis’ is offered by Nicolai Foss with a number of critical points made from different economics and social science perspectives. Finally, Maurizio Zollo offers an attempt of a synthesis between the two positions with some proposals for conceptual and empirical advancement. The dialogue points to a convergence on the usefulness of evolutionary theory as a theoretical lens, but also on the need to add to the central notion of routinized behavior, other stable organizational traits of evolutionary relevance, such as the cognitive, motivational and identity-based antecedents to behavior, as well as key dimensions of intentionality and consciousness of change, which have been so far given either implicit or axiomatic roles. An overarching model of firm evolution that includes these micro-level factors in the context of more collective-level dynamics might help forging collaborative work across different schools of thought, as well as bridging the two levels of analysis in a way that is both theoretically well-grounded and empirically testable.

Keywords: micro-foundations; organizational routines; organizational capabilities; organizational knowledge; organizational evolution; motivational processes; emotional dynamics; cognitive frames; shared values

Introduction by Koen Heimeriks

Where do organizational routines and capabilities come from? To try and further scholarly work on the ‘origins’ question within the micro-foundations project (Felin and Foss, 2005), it is relevant to identify two sub-questions: (1) What elements are key to explaining organizational routines and capability development (and change)? and (2), How can these elements be studied (i.e., where do they reside and what are the underlying theoretical and empirical assumptions)? Viewpoints and ideas on these questions have potentially far-fetching implications for the future academic inquiry, since such answers direct attention to the what and how of studying...
organizational routines and capabilities (e.g., role of agency, context).

Thirty years have passed since Nelson and Winter’s (1982) seminal work *An evolutionary theory of economic change* was published. Their work has informed various fields of study, such as behavioral economics, institutional economics, population ecology, and organization and strategy. Most notably perhaps, their work helped advance our understanding of drivers of competitive heterogeneity (for overviews see e.g., Hoopes et al., 2003; Becker, 2004, 2005; Acedo et al., 2006). Though, since their work was published, progress has been made (Cohen, 1991, 2007; Staw, 1991; Egidi, 1996), there is today a remarkable consensus about the importance and the pressing need of addressing the origins question (see Cohen et al., 1996; Murmann et al., 2003; Felin et al., 2012).

The origins question goes back to the Carnegie roots of evolutionary economics, especially to Simon (1947) and Cyert and March (1963), as well as early work in psychology (Dewey, 1922; Polanyi, 1962), sociology (Weber, 1946/2007; Kerr and Fisher, 1957), and economics (Arrow, 1951). Since then, it has evolved in several fields of inquiry, surfacing among others in the 1960s in the organization structure literature which – in referring to the ‘formal’ versus the ‘informal school’ that argued respectively for ‘organizations without people’ and ‘people without organizations’ – tried to answer related and fundamental questions (e.g., Pugh et al., 1963). More recently have related issues been heavily debated in contemporary sociology and cognitive psychology, for instance regarding the role of intentionality and deliberation in complex decision-making (see e.g., Dijkstra and van den Brink, 2006; Rey et al., 2009) or the role of the individual in the dispersion of social phenomena (see e.g., Watts and Dodds, 2007). Recently, similar discussions have surfaced in the field of organization and strategy, discussing the roles of intentionality and mindfulness to further our understanding of organizational heterogeneity (Levinthal and Rerup, 2006; Weick and Sutcliffe, 2006).

Essential as it may appear to management scholars, the origins question has proven hard to answer. For any answer to this question requires a discussion of ‘the basic elements’ (Lippman and Rumelt, 2003), or the main constituents driving firm heterogeneity, as well as their aggregation to explain differences in firm-level outcomes. Moreover, there are different approaches or viewpoints on how to study these constituents. Traditionally, founding and shaping the field of evolutionary thought, Nelson and Winter (1982) emphasized the role of organizational routines as key phenomena to explain evolutionary change in organizations. More recent work, arguing that early work largely neglected the role of lower-level phenomena, directs attention to individual action and (causal mechanisms that influence social) interaction (Felin and Foss, 2005). When it comes to philosophical assumptions, the former approach pointed to zero- and higher-level phenomena such as organizational routines (see also Winter, 2003), whereas the more recent and latter approach directs attention to individual-level antecedents of organizational routines and capabilities. Echoed in a number of recent contributions to behavioral strategy by Powell et al. (2011) as well as Foss and colleagues (Abell et al., 2008; Felin and Foss, 2005, 2009), the explicit and broader study of the micro-foundations of routines and capabilities has intensified.

Essentially, recent work takes issue with the fact that the routines and capabilities literature examines higher-level phenomena without fully explaining how these influence and are influenced by lower-level processes. This viewpoint diverges from more recent work as the prevalence of the higher-level focus has left many questions unanswered regarding a number of fundamental aspects, for example, the degree of managerial choice, stable or dynamic patterns of behavior, homogeneity or heterogeneity of organizational agents (Becker, 2004). Indeed, a comprehensive overview of prior work demonstrates that the past three decades the majority of scholarly work has analyzed recurring action patterns at the macro or firm-level (Heimeriks and Felin, 2012).

Yet, while the viewpoints on how the origins question should be tackled diverge, viewpoints on what aspects of the origins question to study seem to converge (i.e., what mechanisms or antecedents are critical to analyze). Though recent critiques suggest early work has caused a prevalence of firm-level research, which in terms of empirical validation is an accurate observation (Heimeriks and Felin, 2012), early work seemed to clearly prioritize the role of ‘choice’ and ‘deliberation processes’ in studying organizations and addressed the need to theorize about factors internal to the organization (e.g., March and Olsen, 1976; Nelson and Winter, 1982). In other words, there seems to be convergence on

The Call for psychological foundations clearly has its roots in early work. As said, beyond Simon’s (1947) and Cyert and March’s (1963) contributions, note that March and Olsen’s (1976: 150) work on the ‘cycle of choice’ explicitly incorporates the notion of ‘variation among individuals.’ Calling for a (modified) set of theories on organizational choice, organizational attention, and learning, they explicitly acknowledge that ‘the connection between individual action and organizational action is sometimes quite loose’, suggesting it is critical to ‘consider the timing of different individual actions, as well as the changing context of each act’ (March and Olsen, 1976: 341). Similarly, in their 1982 book, in particular throughout Chapter 3, 4, and 5, Nelson and Winter repeatedly acknowledge the importance of, for instance, ‘knowledge’ (Nelson and Winter, 1982: 60, 62, 67, 68, 76–82), ‘locus of knowledge’, ‘choice’, ‘deliberation processes’, intentionality, thereby fundamentally addressing key phenomena requiring scholarly scrutiny. Nelson and Winter (1982: 67) already explicitly called for a ‘theory of attention’ to account for bounded rationality assumptions when studying organizations. Hence, the acknowledgement of the need to address micro-level issues is well-established in early as well as
what phenomena to study (the ‘what’ of the origins question): both early and more recent contributions call for further inquiry to disaggregate factors endogenous to the firm to understand antecedents of repetitive patterns of behavior and action in firms. Though an initial read of recent debates on the origins question may nurture a different intuition, both early and recent work seem to diverge predominantly on how to study phenomena critical to the origins question. A more traditional or top-down program explaining antecedents of organizational routines and capabilities constructs its models around the institutions shaping the firm (and arguably thereby also its ‘elements’). Models building on the more recently proposed bottom-up program will instead center around characteristics of individuals involved (who are arguably also subject to a shaping effect of associated institutions) in firm processes. Phrased differently, a more careful assessment may reveal the debate to revolve around the fundamental question to what degree there is ‘choice’ and ‘intentionality’ for actors involved in organizational decision-making.

Against this backdrop, the ‘micro-origins’ or ‘micro-foundations’ debate is gaining pace and intensifying recently. More importantly, whereas recent conceptual work unequivocally stresses the importance of greater inquiry about the role of individuals and the causal mechanisms involved in social interaction (e.g., Zollo and Winter, 2002; Felin and Foss, 2005, 2009; Augier and Teece, 2009; Mauboussin, 2009; Katkalo et al., 2010), extant empirical work in this domain has only scratched the surface of what many consider to be the next challenge for scholarly work in the field of strategy and organization as well as related fields. Despite notable exceptions (see e.g., Clark et al., 1987; Burgelman, 1991; Cohen and Bacdayan, 1994; Feldman, 2000; Feldman and Pentland, 2003; Gavetti, 2005), more recently studies have started to more consistently validate theoretically grounded notions of organizational routines and capabilities and demonstrate in what way individual level antecedents explain the higher-order constructs (i.e., organizational routines and capabilities).

Fitting Felin and Foss’ (2005) ‘bathtub’ model which uses Coleman’s (1990) framework for the study of individual foundations of social phenomena, some studies have started to analyze certain micro- or meso-level elements of routines and capabilities at different levels in a variety of contexts. For instance, some attempts have been made to unravel key lower-level factors, for example, interest alignment (Gottschalg and Zollo, 2007), new product development (Salvato, 2009), absorptive capacity (Volberda et al., 2010; Lewin et al., 2011), ambidexterity (Jansen et al., 2009; Mom et al., 2009) as well as founder characteristics (Hallen, 2008; Gruber, 2010). Other empirical work has recently revealed manifestations of higher-order (i.e., ad hoc problem solving) routines in the form of risk management practices which help counter potential downsides of codification and thereby help optimize acquisition integration performance (Heimeriks et al., 2012). Similarly, recent work in the context of strategic alliances has demonstrated the importance of an alliance function (Kale et al., 2002) and how advantages of other learning mechanisms, for example, dedicated functional positions, alliance metrics, and alliance intranet, are contingent on the level of prior experience (Heimeriks et al., 2007). These insights have helped to open the black box of ‘capability development’, by uncovering specific causal mechanisms in complex tasks such as alliances and acquisitions. Similarly, related work has recently examined how executives balance efficiency and flexibility in dynamic environments (Eisenhardt et al., 2010). These recent contributions mark an important first step toward understanding ‘where organizational routines and capabilities come from’. Yet, however crucial, this work is but a start toward an improved understanding of human action and microfoundational processes.

So why the origins question?

Hence, despite repeated calls (as for instance by Zollo and Winter (2002: 339): ‘the need for a better understanding of the origins of capabilities becomes increasingly apparent.’), the origins question looms large for a number of reasons. First of all, given the all-inclusive nature of some definitions of central concepts (i.e., routines, (dynamic) capabilities, competences; Felin and Foss, 2005), it is hardly surprising the vast majority of studies have relied on relatively distant proxies which make it hard to disentangle true causal mechanisms. As a consequence, the ‘building blocks’ of the key concepts remain under-specified, and there seems ample room to clarify the linkages and interdependencies between the higher-level concepts (routines and capabilities and the organizational level) and its lower-level elements that influence the evolution of routines and capabilities.

Second, absent an understanding of the ‘building blocks’, key concepts may remain unnecessarily ‘mysterious’ (Hodgson, 2003). As Dewey (1922: 227–228) stressed: ‘[many] suppose that if one is told what to do . . . all that is required in order to bring about the right act is will or wish on the part of the one who is to act . . . The prevalence of this belief . . . is the greatest bar to intellectual social progress; it bars . . . inquiry to discover the means which will produce a desired result.’ In other words, the mechanisms involved in the ex-ante and ex post decision-making processes can shed important
light on our understanding of social phenomena, such as routines and capabilities, in organizations.

Third, perhaps as a consequence of extant definitions being as they are, early work seems to have sparked relatively isolated advances of theoretical traditions rather than fostered an integrated view of highly related concepts such as resources, routines, capabilities, competences, and the like. In spite of recent attempts to link different theoretical traditions (e.g., Acedo et al., 2006), theories such as the resource- and capability-based views of the firm and of competitive advantage, as well as evolutionary and organizational learning theories, have typically advanced on parallel tracks, causing a flurry of loosely coupled work.

Last, beyond the theoretical implications, these central questions ultimately have great relevance implications to practitioners (Pfeffer, 2009). Requiring more ‘engaged’ types of methods (cf. Van de Ven, 2007), the use of firm-level level constructs does not ease management scholars’ ability to inform management practice; rather it only seems to weaken it. Relying on higher-level proxies to measure specific types of routines and capabilities is likely to be less informative to managers if the very antecedents to those proxies remain unaddressed in empirical enquiries of different kinds (e.g., case study, ethnographic, experimental, or large scale survey data). Therefore, it is important to discuss how we as management scholars can be more informative and better design our studies so as to advance our understanding of the micro-level origins of routines and capabilities.

Given these reasons, it is hardly surprising the origins question is resurfacing and attracts more and more scholarly attention. Indeed, many important questions remain unanswered. For instance, how do individual-level factors (such as traits, abilities or motivation) aggregate and interact with social mechanisms to create firm-level capabilities? And, how do initial organizational or institutional conditions influence subsequent individual and group-level search and learning processes? In other words, notwithstanding important conceptualizations (e.g., Giddens, 1984), the ways in which levels interact and their degree of interdependence is largely open for study. There seems to be ample room to examine the role of individual action and interaction. Empirically, it is important to move beyond accounting for threshold values (e.g., mean aggregation), diversity within levels (e.g., standard deviation, coefficient of variance), to better understand how levels aggregate and variables interact at multiple levels. Theoretically, the question of when and why such aggregations and interactions are to be expected also looms large.

In what follows, an attempt is made to address some of these issues in the format of an Hegelian dialogue. As such, a thesis gives rise to an anti-thesis followed by a synthesis intended to resolve the tension. Hence, in the next sections of this article, Sid Winter and Nicolai Foss subsequently articulate their different perspectives on the origins question. Thereafter Maurizio Zollo presents a set of ideas that might help link the two positions in a broader framework of organizational evolution, and points to some opportunities for future research in this area.

Evolutionary origins of capabilities by Sidney Winter

I will discuss first about how we might narrow the question of the origin of routines down to something that is sufficiently narrow and tractable for our purposes. Then, the key question arises: ‘What does the understanding of origins potentially yield in terms of interesting insight into questions about routines?’ I will conclude with some brief remarks about possible research directions.

There has been some comment, for quite a while, about the fact that the term ‘routines’ has been used fairly expansively in some contexts, and it has been used as a kind of shorthand reference for a wide range of phenomena. This expansive usage is legitimate in the sense that these phenomena do have things in common in terms of their origins and implications. But on the other hand it tends to make it rather difficult to pursue the subject of routines as such. Suppose, for example, you raise a question about the origin of routines, or the strategic significance of routines or some similar question. When you confront the breadth of views just on the terminology, you see that you have a very ill-defined project before you, trying to respond to the question across this very broad front.

The ‘broad front’ is quite real, and in the present context it implies that we could take this problem of origins and we could go in various directions with it. There are several different directions that are potentially interesting, but we have to make some choices to get started.

In trying to tidy up things a little bit here, my idea is to put aside some of the alternatives for a while and offer you a focus for the discussion that is consistent, first, with the existing understanding of the concept of routines, which itself is alleged to be controversial in some quarters. And secondly, my proposal is consistent with the punctuation requirement that is implicit in any question of origins. If we understand by ‘origins’ the question of historical / evolutionary origins – that is, in the time-dimension – then of course any account of such origins always is somewhat artificial in the sense that it always can be challenged by the question ‘What happened before that?’ For example, if we want to inquire into the origins of *homo sapiens* in the evolutionary process, that discussion

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typically begins at a stage where a lot of past evolution has already been accomplished. But one could easily challenge that punctuation and argue for some alternative definition of the problem of human origins.

You face the same thing in the context of routines and capabilities. To engage questions about origins, you really have to have a punctuation that gives you a workable idea about a starting point, the alternative being to start with the Big Bang. But it is a very long story if you start at the Big Bang.

My proposal very broadly – and I will continue to spell this out – is this: Let us address the problem primarily in the context of deliberately created, goal-directed, complex, large-scale organizational routines. I think that that focus is still plenty broad enough, maybe vague enough, to embrace a very large proportion of the work that goes on under this general heading. At the highest or macro level, this proposal acknowledges a role for intentionality; there are goals that are being pursued. The ‘micro’ part, then, the locus of the micro-level origins of routines in such a setting, is about what happens inside the macro frame. Goals do not determine the answer to that question; they do not control what happens. That is perhaps one of the most fundamental observations about the situation. High level intentionality and goals do not define or control what happens, so there is the micro-level to be filled in, to explain: ‘What in fact happens in the context of these macro-level considerations?’

On the question of conceptual consistency with the traditions in the subject, I will review a few definitions of the concepts of routine and capability that I have been associated with at some time in the past. These definitions go to questions of reliable execution, of learning as the origin of automaticity in execution, and to the distinction between the individual level and the organizational level. One framing of the latter issue, which I adopt here, involves viewing organizational routines as multi-person skills. And finally, for the capability idea, I previously put forward (Winter, 2000: 983) this definition: ‘a high level routine (or collection of routines) that, together with its implementing input flows, confers upon an organization’s management a set of decision options for producing significant outputs of a particular type.’ That definition was put forward, in particular, to try to isolate conceptually the information or knowledge component of the routine or capability concept, from the usual input level of analysis with which economics is traditionally concerned (the ‘implementing input flows’).

In my long history of working on these problems, of course it has not been a secret that economic theory as practiced in the neoclassical tradition was one of the targets. One of the questions to which I tend to return is the distinction between what I am trying to say and what the economics tradition, the economics textbooks says. Just to spell that out a little bit, capability in the evolutionary context or in the management context plays a role that is parallel in some ways to the role played by the notion of a production function or a production set in standard economic theory. That parallel is sufficiently close, so that if you talk to many economists about these concepts, you will get a blank look and then the query, ‘Hey, are you not just talking about the production function?’

So it is important to explain the difference. The difference turns on the fact that in that economic understanding, there is nothing (except perhaps a notional ‘optimization’) in the gap between what is essentially a ‘recipe’ or a ‘blueprint’-based conceptualization of productive knowledge and productive action itself. When economists take the trouble to explain the idea of a production function or set, one generally gets recipe or blueprint metaphors as being the explication of the real-world counterpart for the theoretical idea. But then that leaves open the question of how production or action actually happens. I like to say, ‘You never ate a meal that was cooked by a recipe or lived in a building that was built by a blueprint.’ A related comment is that the symbol strings I consciously run through my head do not directly control my muscles – much less your muscles, if you are assisting me with the task. (Indeed, the symbol string part could proceed in much the same way if I were physically paralyzed, except for the difference in the feedback I would receive.) There are intervening processes that have to be given some kind of account, and that is where the departure from standard economics most significantly occurs. One of the questions Nelson and I put forward early in our book was: ‘What is the story about where the knowledge lives?’ (Nelson and Winter, 1982: 62). In the routines and capabilities conceptualization, the knowledge does not live in any single place, and certainly not exclusively in symbolic records. It says that the knowledge lives in diverse places, that it is distributed and situated, and that the coherence that you ultimately see in productive action is the product of evolutionary processes and learning, which put it all together in the course of time.

As I briefly sketched before, I am talking here about a framework for these micro-level origins questions that presumes intentionality at the macro level. We can imagine a discussion in an organization that starts with the observation: ‘We need to get this done. We need to figure out how to get this done. We need to know who is in charge of getting this particular thing done.’ These sorts of questions, or aspirations, actually are expressed in the world. They are asked in particular times, in particular places, and by particular people. So, given the need for punctuation, explained before, those occasions are what I put forward as the punctuation. This means that, if you want to study a question about the origins of routines and capabilities, you should locate a point in time, and a place,
and some people, where these kinds of questions have been put on the table – and take the story forward from there. Looking at historical situations, it is not hard to find examples of the kinds of occasions when these questions are raised. So, this provides the punctuation I am talking about. This provides a dating, a not-entirely arbitrary starting point for discussing where something came from in a historical or evolutionary sense.

Now, within that frame we have to emphasize that the understandings of all the key terms are not necessarily sharply defined at the origin point that we are talking about. The identity of the ‘we’ that ‘need to get this done’ may be something that can shift over time. It may turn out, for example, that the answer to the ‘how’ question comes more easily if we change our mind about who ‘we’ are and add to our team somebody who has requisite knowledge and skills in a domain where, we perceive, something important seems to be missing, and so on. You can think about the sorts of contingencies that might induce some motion in the prevailing definitions of these key terms.

Nevertheless I think it remains true, most of the time and in a great many important cases, that indeed these questions are asked and at any particular time there is a general understanding about the answers. That this scheme covers a broad range of interesting cases is all I claim. I am not going to claim that it covers the full range of interesting cases. To attempt that would require a different, complementary, discussion. I would have to talk about accident and contingency and bottom-up instead of top-down, and I would ultimately have a different class of suggestions to make about the origins of routines. But, in the interest of getting this subject narrowed down to the point where we can make progress with it, I think this is a good move.

To reprise, we have this high-level structure of intentions and objectives, but then there is the question, ‘What actually happens?’ The fact that the high-level structure exists, does not imply much of anything with respect to what actually happens, because, there is a series of questions that are left unanswered by simply describing the situation at the level of intention and objective. We do not know, just because intentionality is present, the answer to the question of what is actually going to happen. This particular version of a macro-micro distinction could be thought of as corresponding roughly to the distinction between ‘wanting to get this lock open’ and ‘knowing the combination that opens this lock.’ ‘Wanting to get the lock open’ is a high-level macro orientation. ‘The combination to the lock’ is a micro detail phenomenon, without which the macro goal cannot be accomplished. It can be a nasty little problem, if you do not have the combination handy, because outcomes are very sensitive to the details of what is tried. In the setting of a complex organization trying to do a complex thing, there are lots of dials and lots of hands on them, and as noted there is continuing room for distinctions and dispute and drift on the question of what it means to successfully accomplish the task – ‘how open the lock has to be’ – before you can say that the problem has been solved, or not solved. In contrast to the typical situation with the metaphorical lock, it is very rare for there to exist, at the start, a symbolically-encoded recipe that conveys the informational aspect of the solution in such detail as to lay all implementation concerns to rest.

Although I did not look up whatever I have said about it in the past, this emphasis on a contrast between high-level intentionality and low-level automaticity is an idea that has been featured in my work for a long time. A distinctive thing about skills and routines is the fact that, in most cases, once they exist, the transition from intention to accomplishment is a short and easy one, because you are exercising existing skills, routines, capabilities. Often the transition becomes so easy that the very existence of the implementing skills, routines and capabilities ultimately falls out of conscious awareness; they become ‘taken for granted.’ The ‘origins’ question is, of course, ‘What happened before that?’ Before that, there was a very different situation, in which the transition from intention to action, and even more to outcome, was problematic and usually experienced by participants as being so.

If we accept my proposed framing, and we identify a particular case with its punctuation, its dated start, its objective and so on, we then look at what happens thereafter. We see a process that occurs in time, and if, at the end of this process, there is by some reasonable standard a routine in place, then we have answered for that case the question about the origins of the routine. Again, this understanding relates to the time dimension of the idea of origins. The ‘answer’ is in the form of a process account, and it could not be merely descriptive. It necessarily embraces some element of theoretical explanation, an understanding of why that process would produce a reasonably reliable routine.

Of course, not every such quest for a way to get the thing done ends in success. Many end in outright failure and are abandoned. When, therefore, we look at the outcomes that we can actually see and ask: ‘Where did that come from?’, we have to remember (as usual in an evolutionary context) that of course the achievement is the dependent variable on which we are sampling. In the background there is often a long record of failed and abandoned attempts, which in some sense is the existential balance of the success that we are looking at. In some cases the conclusion on success or failure may be seriously ambiguous, and in virtually all cases there are context-dependent ‘matters of degree’ affecting the conclusion (Winter, 2000). Because successes come from a population that includes failures, and because the process is often far from deterministic, part of the answer to the origins question is simply ‘good luck.’ But it is not the ordinary, transient, win-the-lottery kind of luck,
because it congeals to form part of the successful ‘recurring action pattern’ in the organization (Cohen et al., 1996). The mechanisms of the ‘congealing’ are diverse, but one important one is ‘serendipity’ – a path to success openness by chance, but is noticed, seized and exploited, and that last part is not a reflection of mere ‘luck.’

I will borrow here from Geoff[rey] Hodgson the word ‘mystery’ and the phrase ‘mystery of the routine’ (Hodgson, 2003). What he had in mind was basically a question about the status of routines as phenotypes versus routines as genotypes in the evolutionary theory put forward by Nelson and Winter. Alas, his claim that there was some ambiguity about this in the book of 1982 was quite well substantiated. There was in some sense a legitimate mystery about it. More recently, there has been discussion about origins and micro-foundations of routines, which we now continue, with a sense of ‘mystery’ stated or implied. I am not persuaded, however, that there is such a mystery, a mystery of the origins of routines. I tend to think that the sense of mystery arises mainly from the fact that we are dealing with context-sensitive dynamic processes of some complexity, and that such processes are not easy to describe in a concise way. It can be relieved by close study of detailed historical accounts, including accounts that deal with ‘technologies’ and particular artifacts, such as aircraft designs, as well as ones that are more squarely on the subject of routine. It can also be relieved, however, simply by attending more closely to the small examples that come by on a day-to-day basis – as I shall now illustrate.

The day in question was yesterday. At my request, I had been provided with a codified ‘recipe’ for how to get from Schiphol Airport to Rotterdam Central Station. I attempted to execute this recipe, and as directed I went to the area in the airport from which the trains depart. There I found ticket machines at which, I had been advised one could acquire a ticket. So, I put my credit card in the ticket machine and it asked me for my PIN. In the US, we do not typically use PINs with credit cards, except when we are trying to withdraw money from an ATM. And since I do not use my credit card for that purpose in the US, I did not know the PIN of my credit card – although it certainly exists in some codified record some place, possibly even in my home or office. Fine, so I conceded defeat on the attempt with the credit card. ‘Not to worry,’ I have a debit card from my bank, which serves also as an ATM card, and for that I do remember the PIN, because I do use it to withdraw cash. And in fact I have already done just that and acquired a supply of Euros. So I put the debit card in the machine. Alas, this machine does not know my debit card, does not acknowledge its true worth. In fact, it simply refuses to continue with the process, without any comment whatsoever. Forlornly, I wander away from the ticket machines, wondering if there is some place where I can spend my recently-acquired Euros to get a ticket to Rotterdam. And I wander in the wrong direction. But I do find a place where a sign says ‘Information.’ And although the information on offer was not obviously supposed to be about the train, I ask anyway. And the answer was: ‘Past the flower shop and turn right.’ OK, so I go past the flower shop and turn right. I find a place where they are actually taking Euros for tickets and I successfully acquire my ticket. I am sure I could come up with further complexities in the total process that I might have encountered, or certainly could have encountered, but that is the basic story.

So this is an example: I had a certain high-level intention; I wanted to get to Rotterdam, and my proximate idea of the solution, provided from the codified account, was to take the train. As is typically (actually, always) the case, the intention and the codified solution did not provide all the detailed knowledge required for successful performance, and consequently I hit some snags. Fortunately, I was not in a hurry. In a context where I was in a hurry, this would all have been quite stressful. Oh, I do recall one more complication: I passed up on the first train that came up on the board, because it required a change of train, and I did not want to do that, so I decided to get my first Starbucks latte for a long time. And that caused me to miss the next train. But anyway, the point was that I was not in a hurry, and so it was not a very big deal.

The more important point is, there is a Schipol-to-Rotterdam (Central Station) mini-skill, which I learned yesterday. If I had to re-execute this journey today or tomorrow, I would not have the rough spots that I encountered yesterday, because I learned various things on my first attempt – trivial things, in one perspective, but nonetheless crucial to the performance, like the difference between the ‘6’ and the ‘9’ on the dial for combination lock. I am not going to try again to put my credit card in the ticket machine or if I do, it will be because I went to the codified source and found out what the PIN is before I tried. So I have acquired this mini-skill. As I say, if I do it today or tomorrow, I think I can perform it with high reliability. Now, if I come back next year, will I remember all this? Not so clear. There is a possibility of ‘rustiness’ there in the performance of skills and routine.

I should emphasize that my difficulties did not reflect deficiencies in the codified account provided by my Rotterdam hosts, except to the extent that all codified accounts embody similar deficiencies. The problem was the account did not anticipate all of the possible contingencies of implementation. But how could those be known in advance? And if they were known, how could such a comprehensive, consequently massive, codified account be of any practical use? It would be just like the missing PIN code: the information is there, somewhere, but who knows where? For a similar view of the problem of taking all contingencies into account, see the discussion of the problem posed by the color of the circuit boards in the knowledge transfer study of von Hippel and Tyre (1995).
When you look into the origins of skills and routines, you find something much like my little story of yesterday. You find it over, and over, and over again in the acquisition of routines and capabilities, understood as multi-person, multi-level skills. When a large organization attempts a new, complex performance, something like my story occurs on a massive scale. We know what we are trying to do; we do not know how to do it. We think we know how to try, at least, and we do try, but it doesn’t work. We identify some specific difficulties and iron them out, and we try again – and then we do all of that over again. After a while, we get something that is in some narrow sense a ‘success.’ We try to repeat that, and find we can’t, because the ‘good luck’ didn’t happen again. Eventually, repeated attempts begin to deliver technical success on a reliable basis, and from there on it is relatively easy going, and we gradually move on towards a reliably smooth performance. Whether it is an economically viable smooth performance may be the very last question to be answered, although the whole effort is likely a waste if it is not.

This is, I suggested, a complex and context-dependent dynamic process. It is not so easy to describe; consider the space used above for a very simple example and a more general abstract account. I claim, however, this is not a mystery; this is life. I do not think that it is really mysterious. If you look at the cases and you look in detail at the examples, you will find that basically that same kind of story plays repeatedly in various scales, settings and so forth. I am less clear about the foundations question, as distinguished from the origins question. I understand the foundations question to relate to the basic model of human behavior that underpins the learning story I just gave. I think there may be an arguable question as to how much mystery about foundations there is, in other words, there are probably issues there that ought to be addressed.

What I want to propose, then, is that what makes the origins question interesting is not its generic version, which I think is actually not a particularly hard question to answer in a sweeping kind of general way. What I think is the proper focus is the relevance of the origins to the actual characteristics of the routines and capabilities that are established. The subject of routines and capabilities is one where we know there is a great deal of interest associated with various dimensions of contrast among the various examples. These dimensions of contrast include: efficient versus inefficient, inert versus relatively variable, heterogeneous versus standard across sites, and so on. There are different dimensions of contrast, which you could illustrate from your own experience. You can illustrate it in a more substantial way from the literature on these subjects.

The reason one should study origins is, first and foremost, because it would likely help us understand why these particular kinds of contrast between different examples of routines are appearing. In the US today for example, we are more than usually struck by the fact that there seems to be some sort of important contrast between GM and Toyota, in terms of their efficacy as automobile companies. That basic contrast has been there for a long time, more than a quarter century and counting. Its sources are understood in some part. There is a wonderful paper by Paul Adler (1993) on the NUMMI plant, where GM and Toyota got together to try to have an automobile assembly plant as a joint venture. As you read that paper you begin to understand a lot of the differences between GM and Toyota, and how deeply rooted those differences might be. This might be relevant to the origins of the problems that the US auto industry has today, which among other things contributed to the recent demise of the NUMMI plant. But that is just one example of a major contrast documented in the literature, among many.

Actually, for the recent history of contrasts, just read the newspaper. You will see – if you still do read newspapers, there are alternatives of course – you will see that there is one case after another where very significant propositions about incompetence or competence in the domain of routines and capabilities are right at the fore. We tragically lost a scheduled airline flight off the coast of Brazil, not long ago. However, of course the important thing about scheduled airline flights is they are, statistically, of enormously high reliability and safety. It is a very, very routinized operation. If you have been listening, as instructed, to the safety presentation at the start of your flights, you have been well exposed to one of the many aspects that is highly routinized. Overall, it is an astonishing social performance. The remarkable safety record of scheduled airlines is a great social production, and a very complex one. It stands in sharp contrast to the safety of general aviation, the practice of which is much less routinized. In the US in 2005, the fatal accident rate per 100,000 flying hours in general aviation was almost two orders of magnitude larger than the rate for air carriers (i.e., the various forms of carrying passengers for payment). I doubt that anybody believes that narrow motivational issues – differences in the pilot’s desire to stay alive – are a significant factor in such a huge discrepancy. Here is an example of a life-and-death distinction in organizational performance, having to do with the quality of the routines and capabilities that are in place. Another relatively well-studied case is hospitals, which present another set of life or death contrasts in the quality of organizational routines and performances. Those contrasts and others still provide the essential motivation for the study of these organizational phenomena. And it is to further our understanding of the origins of these contrasts, of the contrasting performances, that we should study the origins of the routines themselves.
Thus, in studying origins we should be trying to discover what it was that made this example work well while that example works badly, what was it that made this one turn out so recognizably similar across the face of the Earth while that one is so diverse, and what accounts for the persistence of highly idiosyncratic ways of doing things that appear to be a very localized phenomenon in a particular firm or a plant of a particular company. Corresponding questions arise for the other distinctions that have been discussed. To understand how these very different outcomes occur, thus how routines are created, we have of course to pursue the ‘origins’ question through enough of the learning process, so that there is good reason to think that in the end, yes, there is a reasonably stable routine or a capability that has emerged from the particular process. That is, we should not take the origins question excessively literally – so literally that we neglect to make the crucial connections to the character of the routines that ultimately appear in practice.

Consistent with this, I think it is reasonable to subsume under the ‘origins’ rubric the analysis I presented a few years ago under the heading ‘The satisficing principle in capability learning’ (Winter, 2000). That analysis focused on the end of the initial learning phase, the point at which a ‘reasonably stable routine’ has recognizably emerged. I observed that once a process of capability learning gets underway, there is a question about where it stops. Rarely, if ever, is there an objectively clear demarcation point indicating that the desired capability has been achieved. I argued that, realistically, this situation basically illustrates the satisficing principle in action. There is an aspiration level defining satisfactory achievement, in the sense that reaching that level of capability triggers the organizational decision to stop what I called ‘overt learning’ – that is to say, the learning effort motivated precisely by the perception that the objective has not been accomplished in acceptable degree. I offered a set of proposals there about the things that drive the aspiration levels, which in turn govern the satisficing trigger that determines when the overt learning phase will end. Learning will almost certainly continue beyond that point, but it will be incidental to the actual exercise of the capability, rather than a focal goal in its own right. Of course, as is usual in satisficing models, the realized achievement at any point of time affects the evolution of the aspiration level, tending to pull it down when progress toward the aspiration level is difficult. There is an obviously important question about how fast that mechanism operates and how far it can go, which I also discussed to some extent.

Thus, one way to get a lousy, inefficient routine, is to have a low aspiration level that leads the organization to satisfice when that routine is achieved. Similarly, one way to get a routine of extremely narrow scope is to start out with the objective of having a solution to a very narrow problem, and to stop by satisficing when you have merely the solution to that very narrow problem. And so on. If you ask why an organization might have such low aspirations, one key reason is ‘because it is a satisfactory achievement given the competitive environment that currently exists, and doing better is in various ways costly.’ My paper discussed several other candidates for the external drivers of aspiration levels, beyond the prevailing competitive standard. Pursuing these questions empirically would be one line of inquiry that I think would be worthwhile; case studies would be the obvious way to go, but other possibilities should not be ruled out. As a part of that inquiry, the operational validity of the idea of an observable ‘end to overt learning’ would need close examination. That could be thought of as a quest for reasonable ‘punctuation’ at the end of the learning process, symmetric to the earlier question about punctuation at the beginning.

Returning to that beginning point let me add that success in identifying an ‘origin’ of the kind I proposed – involving the asking of the questions that I referred to – should not foreclose all inquiry into ‘what happened before that.’ We can be sure that routines and capabilities are often strongly shaped, at birth, by a variety of contextual factors. In particular, there are obvious and well-illustrated possibilities for influences from the backgrounds of participants, both leaders and frontline workers. And so on. You can probably provide your own list of hypotheses about things that might affect the appearance of inefficiency versus efficiency, or any of the other significant contrasts, in the context of creating a routine.

How should the empirical task be approached? As I suggested, the most obvious path is case studies, looking carefully at some examples. This could include both historical studies and longitudinal ones that may be ongoing; there are familiar advantages and disadvantages under both headings. There are good examples and good models in the literature. Some of them were collected in Dosi et al. (2000). In particular, the one that is usually at the top of my list in these discussions, is Szulanski (2000) on the ‘Appropriability and the challenge of scope: Banc One routinizes replication’. I claim some credit for the general availability of that study; I pushed Gabriel to write up that piece of his dissertation and submit it for a chapter in the book. I think it is a great example of an origins story. It tells you a good deal about the origins of new operating routines at the establishment level, but also, in more detail, the origins of the dynamic capability of replicating those same routines across diverse sites. It is always on top of my mind. As I started to lay down further examples, I said to myself: ‘Oops, Winter, this is your routine again; you have done this thing many times.’ I included my favorites in the reading list for my graduate course and I have mentioned them in various articles where a string of these citations
can appropriately appear. I tend to reel them off in response to some stimulus; it is not exactly a literature review.

My point is, if you want to join the empirical issue of origins, you might begin with the systematic literature review that I have not done. There is a lot of material out there, a lot of studies that are very illuminating on these questions. The studies would be even more illuminating, and easier to locate, if the question of origins were more prominent in the questions being asked in those studies – and also if concepts of ‘routines’ and ‘capabilities’ were taken as conceptually central, which often they are not. For example, many studies frame the issues in terms of the introduction of ‘new technology,’ and do not feature the evolution of the organizational arrangements that accompany such introduction – but there is still a lot of useful information about it provided. The variation in focus and the absence of consistent labeling of the topics means that both the searching and the reading aspects of the literature review are more challenging than they would otherwise be.

The fact that there is already a lot of insight-provoking material available does not mean that there is nothing more to learn. On the contrary, the existing body of scholarship might be considered primitive and inadequate when measured against the importance and interest of the questions involved. Thus, overall, I am in full agreement with the premises of this conference, which are consistent with views that I have previously expressed. I agree that more emphasis on the origins question would be a good thing to have. This is something we should do.

In addition to some useful case studies, we also have in the literature at least two interesting experimental paradigms for organizational learning situations. We have the Cohen-Bacdayan ‘Target the Two’ paradigm (Cohen and Bacdayan, 1994), which has been extended by others who introduced variants on the original experiments. And we have Linda Argote’s origami-folding paradigm (Kane et al., 2005). That is an interesting experimental paradigm for studying routines and capabilities; the cited study reaches very close to the sorts of issues we are discussing here, and the methods could certainly be extended further in that direction. In both of these paradigms, you could manipulate initial conditions at the ‘origins,’ designing experiments to explore some of the dependencies that I have just talked about. You could manipulate the aspiration level for example; you could see what kinds of effects on the achieved efficiency or speed of the routine that manipulation might have. You could explore the important question of the effect of opportunity costs on the cessation of overt learning. When do people decide to satisfice and move on to an exploitation phase for what they have already achieved?

In the existing experimental literature, as in the historical case studies, you can obtain quite a bit of insight into the origins question, if you read closely enough. But studies more specifically focused on it would be very useful. I do think we could use a more ambitious, bigger scale, harder-to-implement experimental paradigm. It should attempt higher verisimilitude in the treatment of ‘organizational’ aspects, and attend more to external influences like prevailing competitive standards. Since I am not going to implement such a project myself, I will not treat that here.

And finally, we clearly need better theoretical models, better computational models in particular, of these processes. I think this is feasible. In fact, it is to me so obviously feasible that I tend to slip into the mindset that it has already been done. When people ask me, ‘So, how does it happen?’ I begin, ‘Well, you know . . .’ – and then follow with a sketch of the logic of a computational model that would illustrate very well how it happens. But this particular computational model has never been created, notwithstanding recurrent effort on my part to get somebody else to do the hard work. We could have a bigger and better computational model of organizational learning.

Just as we have learned a good deal from case studies and experiments, we have learned a lot from previous modeling efforts, in particular those that invoke the rugged landscape metaphor for organizational search. However, in most of these efforts, the rugged landscape metaphor is explored in the context of the NK modeling scheme, introduced to the organizations literature by (Levinthal, 1997). The trouble is that NK does not offer the virtue of suggesting easy ways to subject the theory to test, or link its concepts to real problems. The general idea of search over a rugged landscape remains a very promising approach for understanding the quality of the outcomes that emerge from a search process. It is a very powerful idea, but I think we need better representations of it in formal modeling to capture its full benefit.

**Micro-foundations for routines: What are the questions? And how do we answer them? By Nicolai Foss**

I will start the following highly informal remarks by noting the heterogeneity that characterizes the routines literature. Becker’s (2004, 2005) useful overview papers

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4For example, another one of my favorites is Tyre and Orlikowski (1994) on ‘Windows of Opportunity.’ I drew on this fine study for some of the examples I used in (Winter, 2000). But the terminology and conceptual apparatus of ‘routines’ and ‘satisficing’ is not employed in the study.

5For one alternative approach, see Winter et al. (2007).
identify rather different conceptualizations of routines. Perhaps rather than reflecting markedly different understandings of what is the ‘essence’ of routines, such heterogeneity reflects that scholars consider different aspects of routines. Thus, some look at the emergence of routines, while others may look at the maintenance of routines (incidentally, I think much too little attention has been devoted to routine maintenance). Others look at the performance consequences of routines, and still others look at how routines spread in a population of firms.

Similarly, different scholars think that routines do many different things for organizations. Some emphasize that routines store locally produced firm-specific knowledge; others argue that routines are first and foremost coordination devices, and so on.

Thus, there is clearly heterogeneity in terms of what it from a micro-foundational perspective makes sense to explain, that is, the explanandum phenomenon. However, there is also heterogeneity with respect to how we use routines as part of the explanatory structure of our management theories. And I think that this heterogeneity is so strong that it seriously threatens the micro-foundations project. The basic reason is that it is far from obvious that we can construct a coherent micro-foundations project based on a shared set of behavioral assumptions that will allow us to deal all of these heterogeneous aspects of routines in a reasonably unified manner. This may be asking too much of the micro-foundations project.

This is my basic argument. Now, let me continue with the current status of the routines construct, which we can all agree is a very important construct in management research, even a dominant one. The routine construct is rather heavily invoked in various research literatures in management, though perhaps not as much as, say, capabilities. However, citation figures conceal that routines are very often used to define, for example, capabilities, or dynamic capabilities. If you look into Teece et al.’s (1997) famous dynamic capabilities paper, you will see that they say that there is a routine dimension to even dynamic capabilities. So, such numbers disguise the actual influence of the routines construct and its strong importance as an explanatory primitive, and therefore a building block for other constructs.

Note that the importance of routines also derives from it being a kind of the management research equivalent to a multi-purpose tool; it does a lot of different things for researchers. Thus, it helps us to tell stories about organizational inertia, and it is a yardstick against which we can talk about change. Some have even argued that the fundamental reason routines are important is that it allows us to conceptualize organizational change. It helps us to make sense of the notion of firm-specific localized knowledge. It is a story of how productive knowledge is transferred in time. Routines supply organizational coordination as a control element, because you can check whether something was actually performed according to routine. Nelson and Winter (1982) argue that routines represent truces, presumably meaning that routines represent negotiated divisions of labor and task sequences. Organizational sociologists argue that routines contribute to legitimacy, sense making, socialization, and so on.

Perhaps because the notion of routines performs all these many functions, very few people really question the need for the construct. However, note that there are also lots of important questions that are not raised. Questions related to ‘routine ontology,’ for example, are not really asked with a high frequency. So, we don’t ask questions like: ‘Which organizational items exactly are part of routines – and which are not?’ It may well be because scholars have spent little time on carefully laying down such routine ontology that the routines literature is surprisingly weak when it comes to actual operationalization of this important construct, and therefore also very little actual measurement of routines. To be sure, there is much qualitative research on routines. However, I do not know of any measurement scale developed from routine research that would allow us to measure routines in a meaningful manner, the extent to which an organization is routinized, or grapple with the measurement of various dimensions of routines, and so on.

To sum up, so far it is clearly the case that some questions have been asked, other questions have not really been asked. In order to understand which questions have been raised and why, it may be useful to look at the history of routines.

The history of the routines construct

I am sure that if you look into Max Weber’s (1946/2007) work on organizational sociology, you can find ideas that point in the direction of organizational routines. However, the scholar who seems to have coined the notion of organizational routine is arguably Edwin O. Stene, a public administration professor who was writing in 1940 defined organizational routine as ‘that part of any organization’s activities which has become habitual because of repetition and which is followed regularly without specific directions or detailed supervision by any member of the organization’ (Stene, 1940: 1129). We can clearly discern in Stene’s definition some of the key characteristics of routines as they are described in today’s literature, for example, the emphasis on the automaticity with which routines allegedly work themselves. Stene’s a neat definition, and it may be superior to some of the modern definitions that have been offered, for example, in the often-cited paper by Cohen et al. (1996). For example, in this paper, it seems to be a necessary condition in the definition of a routine.
that it has been selected in response to selective pressures. This makes a lot of sense in an evolutionist context of course, but I would not see that as a necessary, strictly logically necessary component of what a routine is; Stene’s definition does not require it.

When we speak of routine, the big thing in the context of management research, the punctuation event if you like, is not Stene’s definition or the Cohen et al. definition. It is, of course, the publication in 1982 of Nelson and Winter’s *An evolutionary theory of economic change*. There is something around that you may call the ‘standard account’ concerning the influence of this book on management research; you may find it in, for example, Pearce et al. (2002). The standard account stresses historical continuity. Thus, as Pearce et al. (2002) tell the story it begins with Cyert and March (1963) and ends with Teece et al. (1997). However, they also manage to say a good deal about Nelson and Winter’s seminal book; specifically, that the book revitalized the concern with behavioral programs and bounded rationality introduced by Cyert, March, and Simon; identified the main source of stability and change in organizations, and founded all this in the routines construct, in the process linking up with the resource-based perspective and also population ecology, which were also emerging perspectives in the first part of the 1980s.

However, I think if you do a little bit of more history friendly investigation, I think one is led to the conclusion that the standard account gets it partly wrong. It is true that Nelson and Winter are indebted to the behaviorists. After all, Sidney Winter wrote one of the first reviews of Cyert and March (Winter, 1964b). A glowing review, I may add, and the Cyert and March book was clearly important to his doctoral thesis (Winter, 1964a), which is arguably the first statement of modern formal evolutionary economics. So, of course, there is an influence from the behavioral theory of the firm, but the Nelson and Winter book still seems to me to be taking much more up with tacit knowledge than with rationality per se, as I have argued in detail elsewhere (Foss, 2003). Thus, the key message in the chapters that have been so heavily cited in management research, that is, chapters 3 to 5, is the localized, collective and path-dependent nature of productive knowledge as embodied in routines. The ‘standard account’ misses out on this. It also neglects the evolutionary context of Nelson and Winter’s work on the routines construct, which includes: (1) the role of routines as analogues to the heredity mechanism of evolutionary biology – a crucial component of population level evolutionary theorizing; and (2) imperfect environmental matching. Of course, this is where routines become important and the role that they serve in that evolutionary theory. Observe in passing that there may actually be other candidate mechanisms that could perform similar roles. So to the extent that we want to address inertia, we could also look to organization structure and organizational roles. If you want to talk about how knowledge is localized and transferred across time, we could talk about, for example, knowledge sharing and creating networks. If these are the explanatory things we want to accomplish, it seems to me that we are not strictly committed on logical grounds to the notion of routines.

Now, routines certainly serve as a compact and useful summary of all those things in organizations that could be sources of inertia. And such summarizing is entirely appropriate if what we want to do is raising and addressing questions on the level of industries and whole economies. However, things may go a little bit wrong if you start moving down one or a couple of levels of analysis and instead address firms in a strategic management context – a context where routines are now routinely applied. We can sidestep the issues relating to the micro-foundations of routines when we address high-level issues on the levels of industries and economies; however, micro-foundations become important, even pressing, when we step down to the level of firms. This is an important reason why we have witnessed the emergence of a ‘micro-foundations project’ for routines and capabilities.

*Debated, less debated, and un-debated routines issues*

Implicitly, I have argued that scholarly debate and awareness have been somewhat lop-sided when it comes to dealing with the many potentially difficult issues that surround the routines construct. At the basic level, it is not really debated whether routines exist or do not exist. Presumably, few would be prepared to deny the existence of routines. However, many things in the literature can be questioned, for example, that routines explain virtually all of the behavior of organizations, code most organizational knowledge, are characterized by a very high degree of automaticity, and so on and so forth. Consider for example the strong association of routines with automaticity in behaviors. To provide some admittedly highly impressionistic evidence, I have served as a department head for four years now, and I think that at least routines in universities do not work with any degree of automaticity at all. They need to be set up, constructed anew, monitored, inspected, pruned, enforced, and so on. You need to be vigilant with respect to your routines. They do not work themselves at all. More generally, it seems to me that when we talk about routines, we take a lot of things for granted and that there are lots of ‘How do you really know?’ – kind of questions that we need to answer.

*The fuzzy notion of ‘routines’*

Debating routines is a little bit like trying to pick up jelly with your fingers. It is slippery business. This is because
proponents, at least those proponents that I have discussed these issues with, tend to adapt various, what you might call immunizing stratagems or argumentative ploys in their effort to put almost all organizational phenomena under the label of routines. So, if I identify an organizational item that I think does not qualify as a routine, these proponents are quick to reinterpret it as a routine. If I say something like, ‘What about managerial decision heuristics – these are in the head of the CEOs; they are individual phenomena and surely cannot be routines,’ the response may well be, ‘Well, if these heuristics are stable from period to period, they may be thought of as routines.’ So, suddenly we have an individual level phenomenon being subsumed under something which is usually thought of as an organization level phenomenon.

In fact, I often wonder what proponents of the routines notion think is not a routine. Here is a definition – admittedly an old one, but also one that is still being cited a lot – from Levitt and March (1988: 324) who define routines as encompassing the

forms, rules, procedures, conventions, strategies, and technologies around which organizations are constructed and through which they operate. It also includes the structure of beliefs, frameworks, codes, culture and knowledge that buttress, elaborate and contradict the formal routine.

Thus, routines are apparently eleven quite different and quite major things in organizations. I think this is problematic. Of course, we should always recognize that social science constructs are fuzzy, perhaps inherently fuzzy. But there are limits to how much fuzziness is tolerable. And problems arguably begin to emerge when so many heterogeneous notions are included in the definition of a construct such as routines. Thus, it seems to me, and I do admit that this is a bit of a caricature but not entirely wrong, that routines have become a label for anything and everything in an organization that is not perfectly flexible. It is a little bit like the definition of transaction costs in economics. You often see transaction costs defined as anything that might make a contract incomplete. This kind of fuzziness is not something that is by any means unique to the routines concept; many of our key constructs in management suffer from similar problems. Of course treating routines in a fuzzy manner makes sense if you want to do population level thinking, if you want to do price theory in an evolutionary setting, if you want to construct a theory of growth based on an evolutionary premises, and so on. But we should recognize that heterogeneity of the routines construct makes the building of micro-foundations difficult.

Consider the following possible way of dimensionalizing routines as an illustration. Along one dimension is the degree of formalization, and along the other one is flexibility. In Winter and Szulanski’s (2001) discussion of replication-as-strategy, they seem to treat routines as highly formalized, almost like standard operating procedures. They are not flexible, and they should not be flexible, because you need to copy exactly and so on. In contrast, the original Nelson and Winter treatment seems to me to treat routines as much less formalized. However, both contributions stress that routines, at least at the low levels, are pretty inflexible. However, Feldman and Pentland (2003) stress the flexibility of routines. So along these dimensions, there is already significant heterogeneity. I am sure it is possible to come up with other relevant dimensions, such as the emergent/designed dimension, or the ostensive/performative dimension. And similar heterogeneity would show up.

Routines heterogeneity and micro-foundations

This matters greatly for the micro-foundations project, because it is not a priori obvious that we can claim the same motivational, for example, or cognitive micro-foundations for these different notions of routines. Consider, for example, Winter and Szulanski (2001), a paper that is first and foremost about routine replication, for example, in the context of franchising networks. As the franchising literature has of course emphasized, the franchisor needs to make sure that the franchisees stick to those routines, which is why you have inspections by the franchisor, why you have some monitoring of the franchisee by the franchisee, why you have certain incentive arrangements built into franchising contracts. The franchisee may be removed from the network if he does not adhere to the routines. So routine rigidity is important and motivational instruments are provided that help to ensure rigidity. In contrast, if you look at the way Feldman and Pentland (2003) think about routines, it is more like something almost playful going on in organizations; to some extent, people modify routines almost because it is fun. There is an element of intrinsic motivation there. So here are two very different assumptions about motivation, underpinning different notions of routines.

More broadly, if we look at what has been done on the micro-foundations of routines, there are of course papers around. Thus, there is a fine paper by Egidi and Narduzzo (1997) about the emergence of routines; one on the maintenance of routines (Postrel and Rumelt, 1992); there are papers on change in routines (Feldman and Pentland, 2003), routine replication (Winter and Szulanski, 2001), and performance consequences of routines (Abell et al., 2008). However, these papers, each taking up different processual aspects of routines, construct their arguments on the basis of widely different micro-foundations. So Egidi and Narduzzo (1997) have an agent-based simulation model with rather very primitive agents. Postrel and Rumelt rely on behavioral econom-
ics. Feldman and Pentland build from anthropology. Abell et al. use neo-classical production theory. Again, all these micro-foundations are highly heterogeneous, and seem to mix like oil and water, that is, not at all. There is very little common ground among these different conceptualizations.

A possible conclusion so far is that it could be the case that our project on micro-foundations of routines is in a sense too ill-defined to make sense. One reason is that the routines construct is so inclusive. Moreover, addressing different aspects of routines typically require different micro-foundations.

**Whither micro-foundations?**

What are the ways forward? Well, we could elect a routine research czar, and he could pick one clean definition of what a routines is and we could work from that. We could also accept the current mess. If we do, I think we will end up with a bunch of papers that construct very partial, essentially unconnected stories that in a sense do not really add up. Most radically, we could throw up our hands in despair and abandon the routines construct completely. We should not do this. I think that if we research things such as the expansion of franchising chains and these kind of things, it is absolutely indispensable.

However, there is a case for perhaps caring a little bit less about the routines construct. And the case specifically is that the routines lens may suppress critical knowledge issues that we are interested in, particularly in strategic management; critical knowledge issues that have a bearing on value creation and value appropriation. Issues like, what do solidarity norms mean for knowledge sharing? What is the importance of communities of practice for knowledge sharing? How does knowledge sharing contribute to value creation? It seems to me that the collective level focus of the routine lens is in danger of suppressing the micro-level subtleties involved here, factors placed at the level of individuals and their interaction, deserving of close scrutiny. I think also that you could see some kind of recognition of this in the literature because after all many researchers have adopted various notions of higher level routines, the notion of ‘dynamic capabilities’ is the prime example of this (but really picking up on Nelson and Winter’s original notion of ‘dynamic routines’). I interpret this as an attempt to grapple with these critical knowledge-related issues. But it also seems to me that we may be in danger of further stretching an already overstretched construct.

A possible alternative is to explicitly address non-routine behavior, what Winter (2003: 992) calls ‘ad hoc problem solving.’ I would like instead to speak of intelligent adaptive effort. I think that we need to grapple explicitly with such effort because of the need to understand better the organizational side of what we, to use somewhat journalistic jargon, may call the ‘knowledge economy,’ as manifest in the increased importance of experts and knowledge workers for value creation; the implications of these changes on bargaining powers in the hierarchy; the apparently increasing prevalence of delegation and decentralization; and so on. Such organizational changes seem to a certain extent to be a response to an increased need for adaptive intelligent effort in the service of knowledge-based joint production. Joint production is production with a shared goal and interdependencies in efforts and outcomes where the participants recognize that they share a common goal and want to work towards it. Intelligent effort is effort that involves the application of individual judgment. It is adaptive when it reacts to situations that are not anticipated by routines, standard operating procedures, contracts and so on. This seems to capture much of what we talk about when we talk about a specialist applying superior knowledge. Intelligent efforts are involved in knowledge processes such as knowledge sharing. They are involved when we talk about local adaptation in business units to rapidly changing contingencies. These are activities that call for discretion and judgment and not ‘routine’ behavior. They are also difficult, meaning costly, to measure, monitor, control, and reward. However, the management of motivations does not become unimportant just because it is particularly costly to provide such motivation.

It seems to me that to understand how to motivate organizational members to undertake intelligent adaptive efforts, we are not really helped very much by the routines construct, because it is silent about motivation. So, the routines lens – while it can be seen as a stab at the fundamental issues of why and how firms work – does not really tell us why firms may succeed in mobilizing and coordinating these kinds of effort.

Here is a highly speculative proposal. I think we should maintain a key original ambition of Nelson and Winter (1982), namely linking up with evolutionary theory when analyzing organizations. However, there are many interesting things going on, for example, evolutionary anthropology that we can usefully draw on, rather than relying indirectly on evolutionary theory in the form of working with metaphors and analogies to the basic evolutionary mechanisms. We can, for example, draw on Dunbar’s social brain hypothesis (2003). The basic argument is that our cognitive and also motivational apparatus is really in a sense pre-disposed to deal with the intricacies involved in coordinating multiple intelligent efforts. So, people who do evolutionary anthropology on the social brain, literally argue that we can construct evidence, based on various experimental techniques, for a view of human beings as hard-wired to perceive and classify situations as involving joint production; to allocate roles and responsibilities; to prod other people who are involved in the joint effort, to
motivate them, to help them and so on; in short, to engage in various solidarity efforts. Some of the strongest evidence comes from Tomasello et al.’s (2005) experiments with toddlers.

Lindenberg and Foss (2011) build on the social brain hypothesis to tell a story about behaviors in organizations. Our basic point is that the motivation to behave in the above highly prosocial ways is not automatic. It has to be called forth; otherwise it may decay. And a specific way in which you can manage the motivation to help, to prod, to motivate their fellow employees is by means of managing high-level goals. This is a process of nudging, as illustrated by recent experimental game theory findings that it matters dramatically to behaviors whether you label a game as the Wall Street game or the community game. Similar things may go on in organizations, and may be an important part of the activity of management.

Our overall argument is that firms can mobilize and coordinate intelligent adaptive efforts in the joint production, something that requires a combination of goal-directed behavior with a partial suspension of opportunism. Nothing in this argument, which is a story about why and how firms work, requires making use of the routines construct. In other words, you can tell a basic story about why firms work, why they are different and so on, which is based on evolutionary foundations, but, which is totally independent of the routines construct.

Conclusions

I have essentially had two things to say in these highly informal remarks. The first one is that the micro-foundations for routines project is not exactly a well-defined project. I think we can all agree that micro-foundations are in principle desirable, perhaps even ‘trivially true’ to use Elster’s (1989) expression. However, the fuzziness and inclusiveness of what one seeks micro-foundations for, namely, the routines construct seem to threaten the micro-foundations project. My second theme has been that the routines lens, at least as I interpret it, provides relatively little insight into how firms can mobilize intelligent adaptive efforts in the service of knowledge-based joint production. It seems to me to be an increasingly pressing issue. One could perhaps even argue that routines, at least low order ones (Winter, 2003), in fact suppress such efforts. Most of you would agree with me that the routines lens is silent in general about motivational issues, and therefore also about how to manage motivation to engage in intelligent, adaptive efforts. Should we seek evolutionary foundations for organizational behavior? Yes, I think we should. It is certainly desirable, but I propose that we go beyond analogies and metaphor and instead look at some real evolutionary theory, such as the social brain hypothesis in evolutionary anthropology.

Toward a complete model of organizational evolution by Maurizio Zollo

I have been asked to provide some sort of a synthesis, a Hegelian synthesis, between the thesis that was put forward by Sidney Winter and the antithesis that Nicolai Foss has offered. This is quite a tall order, not only because of the giant stature of the proponents, but also because of the complexity and diversity of arguments that have been put forth. So, having been ‘volunteered’ to serve in this mission impossible kind of task, I will give it my best shot in the conviction that the effort, however hopeless, might prove somewhat useful to further the debate, to improve the (much more important) dialogue among the various components of this community of scholars interested in exploring the micro-foundations of routines and capabilities. So, what I am planning to do is to discuss a possible way to move towards a conceptual convergence (not really a synthesis, which is probably too big of a word) between the two positions. I will then share some of my experiences in terms of empirical work to give you a flavor of how some of this convergence can actually turn into real action, beyond the conceptual challenge.

Let me start from the excitement about the research program that we are engaged with. The micro-foundation idea is attractive from many points of view. The first one of course, is that it gives us a chance to address questions that we have been tackling for a very long time. So there is nothing new about asking questions about the role of the individual in firm-level processes, like routines. For example, where does learning take place? At what level? More in general, what are the characteristics of learning processes at different levels? How do individual traits, decisions and behaviors influence that learning process? The program is basically just helping push the answers to questions that we already have been working on, maybe with a bit more emphasis on the individual dynamics.

A more interesting reason, however, why we are getting excited about this program, is because it actually pushes us to make explicit what is typically left largely implicit. Our own epistemological beliefs, holism versus reductionism, for example. This program has already pushed us to do some serious thinking about that.

More importantly, is the question of really doing a better job at describing what goes on in organizations. I think the field has, most of you might agree on this, been thriving on a very narrow and certainly incomplete model of human psychology. And that is a problem that has been recognized. And so this program, if there is any potential contribution that it may aspire to make, builds on the fact that there is a lot more than just behavior and (maybe) cognition to be understood if one wants to
describe how organizations operate and change. A broader perspective on what individuals do in their brains, for example, in response to environmental or your own colleagues’ stimuli, is clearly important to understand behavior in organization, routinized or not. Finally, I think there is a methodological challenge that the program poses to us. How do you design research that can straddle micro- and macro-levels of analysis? Eventually, even bridging other sciences and other disciplines that have developed useful theoretical advancements and employed different methods to generate evidence. That to me is one of the key reasons why this program is actually very important for management scholarship.

So, how do Winter’s thesis and Foss’ antithesis help us with this exciting program? Winter focuses his and our attention to the role of the individual in ‘punctuating’ the evolution of routines with ideas and insights about their creation and adaptation. The use of punctuated equilibrium models, originally developed to study first populations and then organizations, to the routine level of analysis is an intriguing idea, since it provides a role for both the individual ‘punctuator’ and the group-level routine. It also changes somewhat the image of smoothness that the standard accounts of organizational evolution had, based on the constant action of small marginal adjustments, in that it highlights the role of intentionality in producing ‘lumpy’ and novel answers to the question ‘how do we get this done?’ Obviously, both accounts of lumpy, highly intentional, origins of routines and their constant, marginal, trial and error based, adaptation, co-exist, and it is an empirical question to determine which one describes organizational change better and under what conditions that change enhances the firm’s performance or even its long term viability.

Foss, in the end of his intervention, does not dispute the importance of the role of routines in understanding organizational change, and not even the need for an evolutionary approach to understand change and performance. He calls into question, however, the seemingly ubiquitous reliance to the notion of routines, and the need for scholars, especially those interested in the micro-foundation program, to focus their attention to other vital aspects of organizational evolution and adaptation, like motivation to share jointly productive knowledge in a socially supportive fashion. So the challenge he poses is to broaden the standard account founded upon the role of routinized behavior and to integrate other key organizational elements that might significantly interact with (I would add) but that are nonetheless distinct from routines.

**Micro-foundations of what?**

A first step in my attempt to reconcile these two positions is to reflect on what is it that we need to identify ‘micro-foundations’ of? We have been focusing on capabilities and routines, but obviously there are a lot of other organizational elements that might be in equally dire need of micro-foundational work. Think about shared goals and aspirations, the dynamics of power, of identity, culture and so on. There is obviously a need to study possible individual-level explanations to understand a whole host of collective-level phenomena. That is problematic in itself, but it becomes even more so when one realizes the fact that the same individual level traits might be responsible for the existence and the change of a large number of, thus profoundly interconnected, organizational phenomena. Therefore, not only do we need to go beyond the study of routines as organizational phenomena, since there are a lot of other missing elements in a complete model of organizational evolution, but the micro-foundational program needs to do this precisely because the individual foundations of one set of elements, say routines and capabilities, are shared (and thus presumably interact in potentially relevant ways) with other elements.

My proposal, therefore, is to consider the broader question of micro-foundations of organizational evolution for a moment, and let us see if that can help us make some progress. In fairness to Winter’s position, I should say that the Carnegie roots — that have been the foundations of a lot of his and Nelson’s thinking — are important, but have never been considered to be the only thing that matters in order to explain organizational evolution. Since the beginning, there has been at least a clear desire to include other organizational traits within the scope of the research program. The paper by Cohen and a bunch of other top class co-authors (Cohen et al., 1996) focuses fundamentally on this notion of quasi-genetic traits, broadening up the scope of the research program to heuristics and strategies as clearly distinct elements of organizations’ ‘genetic’ endowment of stable (but not fixed) traits, over and above routines and capabilities. And that is already an important step forward. It is also true that in the last decade, there has been a lot of work within this tradition aimed at integrating the behavioral story with cognition (Gavetti and Levinthal, 2000; Gavetti, 2005), motivation (Gottschalg and Zollo, 2007) and even emotions (Cohen, 2007). Cohen’s work is particularly important in this vein, building on Dewey’s

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6 On this point, the accusation of ‘fuzziness’ in the conceptualization of the core concept, based on Leviitt and March (1988) definition is probably misleading, and slightly unfair. I would be very surprised if anyone in the evolutionary economics camp would consider to use, let alone agree with, that definition. Moreover, many of the other dimensions of ‘fuzziness’ (intentionality, formalization, ostensive/performative, etc.) are all relevant theoretical constructs that help interested scholars in the development and testing of a cogent theory of organizational learning and change, at least within the perimeter of behavioral ‘objects’, such as routines and capabilities.
foundational work in psychology and trying to give a better and broader base to the fundamental assumptions regarding human nature, which includes habit (the foundation of the notion of routines at the individual level), volition (the foundation for what we would call today motivation) and emotion. All of those elements are essential to explain human nature and behavior results from the complex interplay among them.

At the same time, however, I do think that Foss has a valid point in saying that despite all this recent effort, there is still a bias towards behavioral constructs and against its antecedents (volition, cognition, emotion, etc.). And even though there are good reasons for the bias, in that there is still a lot of work to do to understand how routines evolve, especially at the micro level, it is also less and less acceptable. If one is really serious about constructing a theory of how organizations evolve, relying on routinized behavior alone is becoming, in my view, a strait jacket. So I agree with the spirit of some of Foss’ criticism. But obviously it should be acknowledged, for the sake of truth, that the evolutionary economics tradition, has been mindful for a long time of these limitations and has been trying to do what is possible to tackle them.

Moving forward

Now, the problem is how to think about ways to actually move from here. Consider, as a possible starting point, the story, the admittedly narrow story if you want, of how organizations evolve through knowledge and capability development processes that was written a decade ago (Zollo and Winter, 2002). There are operating routines, as some of the foundational (although for sure not exclusive) behavioral elements characterizing a firm, which change over time due to both ad hoc problemistic search followed by experiential (primarily trial and error) learning, as well as due to the application of specialized kind of routines, now universally known now as dynamic capabilities, stable patterns of behavior that have the function, the purpose of creating or adapting operating routines. So, part of organizational change is consequent to the operation of these change routines, or dynamic capabilities,7 the rest (which could be the vast majority) being produced by non-routine, ad hoc, search for solutions to more or less defined problems. And then organizations can also develop learning routines8 that influence the evolution of both change and operating routines. So, there are a lot of purely behavioral elements in this story, since the whole explanation of organizational evolution and change is founded on behavioral traits (routines of different kinds and order). Cognitive components are not entirely absent, however. Learning routines are obviously deliberate cognitive efforts to actually figure out causal linkages, so that organizational actors can actually improve the efficiency and effectiveness of operating routines.

All this is nice, but if we are serious about trying to develop a more complete model of organizational evolution, there is a heck of a lot more going on that is just not there in the story. We need to be mindful of that and try to integrate all the other stable but not fixed traits of the organization in our account of how firms evolve. Clearly, cognitive schemas, frames, heuristics and strategies are something fairly stable but not fixed in organizations, therefore potentially subject to evolutionary processes. But also, as Foss argued, motivational elements (what drives collective behavior) and organizational structures: the distribution of roles and decision or influence power within the organization tends to be pretty stable, but certainly not fixed. They also qualify as evolutionary relevant traits. Then, consider emotional traits, I am not talking about moods, but about stable, psychological traits in terms of shared feelings among organizational members: excitement, anger, fear, pride, camaratismo, and so on. And finally, shared values and identities, which determine both cognitively and emotionally not only the boundaries of the organization but also the purpose for existence of the organization and the ‘glue’ that ties it together.

We are all mindful of the importance of these foundational elements of the organization, but it is also clear to all of us that they are currently under-represented (or not represented at all) in the current theories of organizational learning and change. The one point that I would like to add to this observation is that there is a fundamental difference between looking at these traits at a collective rather than individual level. Shared cognition, shared motivation, shared emotions, those are collective level constructs, by themselves are not micro-foundations. So, the challenge before us is twofold: the first is to understand how those collective antecedents of routinized behavior at the collective level evolve, actually co-evolve with the behavioral...

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7There is an ontological identity between the notion of organizational routines dedicated to organizational change and the notion of dynamic capabilities, as defined in Teece et al. (1997), Zollo and Winter (2002) and Helfat et al. (2007). Teece’s (2007) broadening of the definition to sensing and implementation processes is problematic, in my view, precisely because of the Foss’ critique: it makes the core concept way too broad (how does it differ from ‘good management?’) and difficult to work with in theory development and testing efforts.

8Examples of these routines include brain-storming, de-briefing, post mortem reporting, various forms of codification to create or refine knowledge artifacts such as manuals, check-lists and decision-support systems. See Zollo and Singh (2004) and Zollo (2009) for evidence of the positive performance impacts of these deliberate learning processes in the context of mergers and acquisitions, and Kale et al. (2002) in the context of alliances.
aspects already present in the existing theories (routines and capabilities). And second, the microfoundation effort should go into trying to understand how the individual traits, the aspirations, cognitive frames, personal values and psychological dispositions, influence both the evolution of these antecedents as well as the evolution of the hierarchy of routines and capabilities.

For example, studying the influence of organizational identity on the evolution of routines is very important, since it tries to broaden the scope of our organizational evolution model to explain one set of antecedents to routinized behavior (first challenge), but it is not microfoundational because it is still working at a collective level. Unless and until we explain how individual traits have evolved and in doing so influenced the acceptance, rejection and filtering of routines, for example, we cannot really say that we are studying micro-foundational issues, can we?

So, how can this be put together in something we can actually study? It is huge, and some might argue, completely untreatable. Perhaps that is true, but we do not make major advancements by throwing up our hands and duck our head in the sand. Let’s see, maybe if we start thinking about the original question ‘micro-foundations of what’?, it might not be that crazy. In my view, essentially we can talk about five answers to that question, big answers by all means, but they are not infinite. We are talking about behaviors, so obviously routines play a big role, albeit not the only role. Then the cognitive representations (schemas, frames), and consequent strategies and heuristics are also important. They also evolve in ways that we only partially understand. Third, we need ways to think about the evolution of the powers, responsibilities, governance structures and their effects on the evolution of knowledge. That is where, for example, Grandori (2001) and others have been pushing us on the importance of those traits, and how they interact with the others, such as knowledge sharing and diffusion, that are more traditional in the evolutionary economics school. The last two elements, the evolution of emotional components and of cultural traits, are somewhat missing from the explicit study in both evolutionary and transaction cost/organizational economics traditions, but obviously they play an important part in the story. Those traits can be seen as the objects upon which evolutionary mechanisms work.

How does it work?

Having at least somewhat narrowed down the list of the objects of change, the next question to tackle is ‘What primes the evolutionary processes of these organizational traits?’ There are two main answers to this question, in my view, and they have both already come up in the previous interventions. Winter, in the spirit of the Carnegie school, elaborated more on the aspiration component and its dynamics connected to the perceptions of performance. Aspirations frame the problem(s) in more or less specific ways, and set the directions of search for solutions. They also set the levels of satisfaction aimed for, connected to the perceptions of the outcomes from prior attempts, which are themselves not fixed but dynamically adjust over time to the perceptions of success perceived and the persistence of a gap with prior aspirations. And then the motivational aspects, discussed and studied by Foss and others; that really powers the search efforts and primes the willingness to change the status quo, rather than to adjust (downward) the aspirations in the face of a persistent gap.

It is clear that the construction of a complete model of organizational evolution requires an explicit role of both ‘engines’, and that the current accounts derived from the Carnegie school are primarily focused on one (aspirations and performance gaps). The sophistication of the literature on motivation with its wide variety of answers to the question ‘why do people act?’ and its numerous accounts of positive and negative interdependence among them, needs to be incorporated in the general model of organizational evolution.

But even a good integration of the aspirational and motivational aspects (the ‘why change’ aspects) is not enough to give a thorough answer to the ‘how does it work’ question. I would suggest that two dimensions characterizing the intentionally as well as the consciousness of change are key components of the story. The field, as I see it, has been stuck in this debate, assuming a position as to whether change is, or should be, more or less intentional (or formalized, or top-down versus bottom-up, etc.) rather than acknowledging that there is a dimension of intentionality that needs to be studied and endogenized in our models, because obviously change happens at varying levels of intentionality and we need to understand the antecedents and the consequence of that variation. The other dimension of organizational change maps on the degree to which organizational actors are conscious about the ex post existence of change, whether change has happened or not and in what direction. Intentionality and consciousness are two

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9Here I take a clear exception with the ‘grand finale’ in Foss’ position. Yes, we do need to study non-routinized decision-making and its antecedents, but no, I see no way to create a theory of organizational evolution without the role of routinized behavior. Understanding organizational learning and change cannot be ‘independent’ from the concept and the functioning of routines.

10See Greve (2003) for a panoramic view of this vast literature.

11See Deci and Ryan’s lifetime work and, for a more parsimonious application, Lindenberg (2001) and Lindenberg and Foss (2011).
dimensions that are not independent of each other; obviously they influence each other. But, first of all, they are distinct dimensions characterizing change: you can have highly intentional change, the results of which organizations are usually unconscious (take cultural change programs, for instance). Vice versa, you can have unintentional change of which organizations become highly conscious (ex post), as in Burgelman’s (1991) account of Intel’s shift from memory to processors and all example of emergent strategic reorientation. A second point is that they influence each other in theoretically meaningful ways. We need to understand how increases of one, for instance, influence the other. Also, we need to identify the antecedents and consequences of both dimensions. For instance, investments in deliberate learning processes through knowledge articulation and codification can influence where the firm lies on these two fundamental dimensions and how the various organizational traits might evolve as a consequence.

A thought experiment

So, follow me please on an intellectual exercise, just to see what I mean. Think about the possibility to map the frequency distribution of change events in the five elements listed above along the dimension of intentionality of change, with the intent to characterize the evolutionary processes undergone (and undergoing) in a given firm. If one spends enough time in an organization, it might be possible to actually track all the key changes that might happen to that firm’s quasi-genetic traits, whether they are related to strategies, structures, routines, emotions and/or culture. And one could actually map them, basically asking the question: ‘To what extent were they intentional?’, ‘How intentional was that change?’. You can imagine that the distribution of frequency of those changes vary quite a bit from trait to trait. For example, most (not all) of changes in strategic intentions might be fairly deliberate, fairly intentional, a very high level of intention, with some exceptions. Burgelman’s (1991) story of Intel is obviously one example to a very low intentionality change in strategic intent. On the opposite extreme, cultural traits change, almost by definition, without any major level of intentionality, except for fairly rare occasions in which there is a deliberate effort to change a given cultural trait in the organization. So, one could put the frequency of distribution of these traits on an intentionality dimension, and the same thing one could do in term of asking the other question, the consciousness level: ‘How conscious was the firm that a given change had occurred?’

For example, strategic change would happen typically, not always, at high consciousness levels, or at least above the consciousness threshold. Consciousness is different from intentionality not only because of its ex post nature (intentions are supposed to be assessed before the actual change event occurs), but also because there is typically some threshold level beyond which one can say that an organization is ‘conscious’ about the fact that a given change has happened. At least, that those members of the organization that are responsible or influenced by that given change are conscious of it. Whether they are conscious or not that the strategy of the company has changed or the structure and the power distribution of the company have changed, or routines have changed, clearly depends on the trait that you are looking at. It is very difficult that people can say that they are conscious about the change in cultural traits, for instance, or in shared emotional traits. The likelihood that they are conscious about a strategic or a structural change is far higher, of course.

One important observation, first of all, is that the baseline on both intentionality and consciousness levels could vary enormously across firms competing in the same environment, which is in itself a very interesting type of firm heterogeneity that we have not really studied. In some firms, for example, senior managers might be particularly conscious about changes occurring to organizational traits, than in others. Not only in firm strategies, of course, for which they are supposed to be responsible, but in operating and change routines, or in the distribution of power with subsidiaries or among business units, or even in some emotional/motivational and cultural traits of their company. Also, in some firms managers might more deliberately try to influence the evolution of their company’s traits, in other firms a lot less. So, there might be a cross-sectional variation that might be interesting to study.

More important, however, might be the longitudinal dimension: ‘What can prime change over time of consciousness levels?’. For example, there might be different attempts and different ways to conduct deliberate learning efforts, self-learning in this case. Managers might decide to invest in learning about their own company and about, say, the cognitive frames or the emotional conditions of their people. That is typically done with consultants, with audits, HR surveys, stakeholder consultations, all sorts of ways. These processes are intentional, but others might be a lot less. Think about major initiatives done for other reasons but might impact significantly the level of consciousness of organizational traits, like mergers or major restructuring processes. What happens, when managers introduce these changes is that they also become more aware about their operations, their traits, their emotional state and so on.

And then finally, there are other external system shocks to the organization: a financial or economic crisis, a major legal suit, a hostile takeover offer received. These obviously force the organization to figure out what to do and therefore how they are doing in various dimensions. So, what happens? Those decisions, those events, those shocks lower the thresholds of
consciousness. Companies become, well at least the senior managers, more conscious about a lot of these change events. What happens in terms of the intentionality of the evolutionary process when the consciousness threshold goes down is that a lot of the learning from that moment on becomes more intentional, more deliberate. Articulation, codification of knowledge becomes a bit more frequent, one might expect. Managers across the firm become more aware of their past history, but in some cases even about the current, ongoing, changes. Maybe even about the causal linkages between changes and performance, the performance implications of change. Not always of course, and it is never easy. But the implications of those events on the levels of intentionality and consciousness of change would be important to understand, in my view. In terms of our frequency distributions along the intentionality dimensions, an increase in the consciousness levels (i.e., lowering of the consciousness threshold) might be represented with a gradual shift of the frequency distribution curves towards higher levels of intentionality. So, one might expect an increase in the level of intentionality of future change processes, consequent to an increase in the level of consciousness about past change processes.

Now, of course there is a cost attached to all of this. Can a firm actually sustain this level of highly intentional change and investments in learning about itself? Clearly not, because the investment in self-learning and self-enhancement activities has significant trade-offs with the amount of attention to be allocated to day-to-day operating activities. Given the stock of managerial attention in the firm (Ocasio, 1997), one could expect a crowding out effect of the attention dedicated to standard operations or change activities (like R&D or new product innovations) by non-ordinary organizational learning and change processes. At some point the investments in levels of intentionality and consciousness about change and learning processes have to go down and so the threshold of consciousness goes back up. And slowly, the patterns of organizational evolution would become less intentional, shifting back to lower levels of intentionality.

This is basically a story that I think has the power of putting together different interests and different theoretical perspectives. On the one hand, it maintains the attention on the explanation of organizational learning and change, in the Carnegie tradition. But the objects of change are much broader than just behavioral routines. We are talking about traits that are traditionally of interest to different schools of thought, including transaction cost economics, agency theory, institutional theory, social and cognitive psychology, etc. But, to the extent we are passionate in understanding how all those different traits evolve (indeed, co-evolve) over time through learning and adaptation; then there might be a chance to have this synthesis.

Some directions for empirical inquiry

Let me close by flashing some of the directions of future empirical efforts that I think could potentially be generated by this type of conceptual synthesis. One excellent empirical arena for the observation of evolutionary change in a more complete set of organizational traits is the study of corporate sustainability. That is to study the complex set of change initiatives as well as emergent processes that comprise the shifting not only of business practices, but also of mindsets about firm purpose and strategic logic, of shared values, of governance mechanisms, to name only a few of the key traits, towards a sustainable model of the enterprise. You could think of this model as one which integrates (long-term) economic, social and environmental notions of sustainability to define the objectives and performance outcomes of the enterprise. Equally important, this model of enterprise is based on a theory of the firm which gives voice to the various types of actors contributing to the achievement of those objectives without giving primacy to any of them.12

It is a fact that, at least at the espoused aspiration level, an increasing number of companies are claiming that they want to be (perceived as) a sustainable and responsible enterprise. Every CEO worth of his name is coming out with statements about how responsible his company is. The question is the extent to which those statements are actually followed by serious change programs in the company’s quasi-genetic traits, whether they are routines, strategies, structural arrangements or cultural traits, with the explicit goal of integrating these principles. And in terms of its diverse stakeholders, what has the company actually done to change the way they manage their relationships with suppliers, or with customers, or with employees and so on. Looking at this from an evolutionary perspective, it is fascinating because it really requires the company to act on all those various traits to be able to make progress in its stated objective to ‘become’ a sustainable enterprise. It is an interesting phenomenon to observe, since it forces us scholars to theorize, design and measure innovation and change processes across all the core traits of the organization, traits that evolve at different levels of intentionality and of consciousness. Consequently, the evolution of these traits require not only a wide variety of expertise in all the key functional areas of the firm, but also very different methodological approaches to observe change and identify its antecedents and consequences.13

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12There is an entire literature on these assumptions. See Freeman et al. (2010) for a recent review of the key tenets and applications across different social science domains of the stakeholder view of the firm. Also, Blair and Stout (1999) for the pioneering work in law on the ‘production team’ hypothesis.

13For a good example of a large scale, cross-disciplinary, study of organizational evolution across a wide variety of traits
With respect to the micro-foundations of organizational evolution, sustainability is a particularly fruitful empirical arena to engage with, since it requires a significant attention to the dynamics in individual psychology. Think about the required shifts in cognitive frames about, for instance, the purpose of the firm and the way it is supposed to compete. Think of the development of longer-term horizons in individual decision-making processes, and the enhancement of weights given to non-economic consequences of those decisions, like the impacts on the environment and on the quality of life in the relevant communities. The likelihood of decisional outcomes with these characteristics is a function not only of organizational level factors (i.e., strategic positions, structural choices, and cultural traits) and contextual conditions (sectoral and institutional characteristics), but also, and primarily, of stable psychological traits of the individuals involved.14

How can we study the evolution over time of these psychological traits, the consequences of this evolution on organizational outcomes of economic, social and environmental salience, and the role of individual, organizational and contextual factors in shaping these evolutionary dynamics? A fascinating set of questions, indeed. Questions that I view as really core to the problem of understanding the micro-foundations not only of routines and capabilities, but of all the other ‘things that matter’ to build a complete explanation of how organizations evolve. Questions, in my opinion, are much more core to the evolutionary economics agenda than the one related to ‘the origins’, understood as the temporal beginning, of routines and capabilities.

On the study of motivational dynamics, perhaps it is worth spending a minute to reflect on the intellectual and empirical challenge. The ambition there is to understand not only the importance of motivation in terms of performance implications, but to identify the factors that might explain inter-firm heterogeneity in their capacity to enhance motivation in their organizational members. How do firms learn how to organize themselves in order to handle the constant dynamics of motivational aspects in their workforce? And, in a stakeholder theory of the firm, those of their suppliers, their investors, communities, etc. Why? Because from a strategic standpoint, it is actually a fundamental missing piece in our theories of sustainable competitive advantage. All our existing theoretical stories about competitive advantage, whether they are resource-based view, knowledge-based view and Porterian competitive positions, essentially talk about potential competitive advantage. But all those assets, and resources, and capabilities can turn into actual competitive advantage only if they are utilized, they are made sense of and they are leveraged to their fullest potential by the people that handle them. For that to happen, the motivational level of people ‘owning’ those resources and capabilities, and making decisions about the strategic positioning or initiatives of the firm, must be at the maximum possible level. But how can firms learn about what that maximum level looks like, how to reach it with the stakeholders that they are currently dealing with, and (most important) how they can adjust the motivational ‘levers’ to maintain the stock at the highest possible level? You can call it again ‘dynamic capabilities’ of some new form, connected to motivational dynamics (rather than to operational change), but that is obviously an important missing piece to the strategic management puzzle, as Gottschalg and Zollo (2007) tried to argue some time ago.

Empirically, my humble suggestion is to identify contexts where motivational dynamics are really at the core of the action. Take, for instance, management buyouts. If you think about it, fundamentally they are really an experiment in motivation and change, because the likelihood of success is all premised upon a radical, huge, shock on motivational drivers. A complete redesign of the way the firm uses and adjusts its motivational levers. And, consequent to that, another system shock on operating, strategic and operating change; that is what happens after a company is acquired by a private equity fund? So, you might ask the question: how do these private equity funds, these specialists in manipulating motivational dynamics, develop these special types of dynamic capabilities specific to the manipulation of motivation? And, how do they manage a fixed (and rather limited given the small number of partners in the typical private enterprise firm) stock of attention to manage, or at least influence, the motivational systems of a large number of companies simultaneously?15

The third, and last, flash for an empirical research agenda on the micro-foundations of organizational evolution points to the need to explore the real micro-level mechanisms explaining change and learning in individuals’ psychological traits. And when I say ‘micro’, I really mean it down to the neural correlates of emotional, cognitive and behavioral responses to organizational and environmental stimuli. That is, in my view, the core (hard) nut of the micro-foundational research agenda. We need to understand these micro-level processes to build a comprehensive theory of how firms learn and change (Cohen, 2007).

The real challenge then is not so much the alignment of evolutionary and other economic perspectives to

towards sustainable enterprise models, see the Global Organizational Learning and Development (GOLDEN) for Sustainability program at www.goldenforsustainability.org.
14See Crilly et al. (2008) for empirical evidence on the influence of individual emotional and cognitive traits on the likelihood of socially-responsible decisional outcomes.

15Those interested in some of these questions might want to look at some recent work by Castellaneta and Zollo (2012).
understand routines and capabilities. It is that working at this level of analysis requires all of us, irrespective of our intellectual tradition, to step out of our comfort zone and learn to collaborate with scholars in psychology, neurology and related fields working on the same issues that we are passionate for, but with significantly different theories, methods and languages. The good news is that there is an increasingly large number of scholars in these areas that are quite interested in collaborating with management scholars. Laureiro et al. (2010) found that some scholars in social cognitive neuroscience, a specific branch of neuroscience, actually talk about exploration and exploitation in attention modulation processes connected to the most complex brain activities (e.g., working memory, planning, what they refer to collectively as ‘executive functions’). They actually used that language, obviously without any connection to March (1991). In the same project we have started to look at what managers and entrepreneurs’ brains do when they are taking exploration and exploitation decisions to understand. First of all, if there are differences among the two groups in terms of neural activation in both types of decisions. And second, if those differences influence the formation of routinized behavior and, ultimately, the performance outcomes of those decisions. It turns out that the answers to all those questions are positive, but they are particularly strong for explorative decisions. It also turns out that routinization processes fully mediate between attention modulation capabilities and decision performance, which supports an important direct role of routines in positively impacting performance. One might have expected only a moderating role of routines on the direct impact of capabilities on performance, but it looks like the capacity to routinize behavior is really a necessary condition to turn cognitive capabilities into performance, at least at the individual level. And, by the way, routinization was the result of higher (not lower) attention modulation capability; that is to say, the rapidity in routinizing decisions appears to be the consequence of higher cognitive capacity, rather than of ‘mindlessness’ (Levinthal and Rerup, 2006).

Conclusion

The position between the thesis and its antithesis are not that distant if one considers the overarching perspective of explaining organizational evolution. The Carnegie and evolutionary economics school have been aware for quite some time of the need to examine ‘quasi-genetic traits’ beyond routines, and among those traits are definitively included the motivational dispositions that Foss and others are working on, as well as several others that also need to be brought into the picture, like purpose, values and emotional dynamics at the individual and collective levels.

I have also suggested that the study of intentionality and consciousness might be useful places to start understanding the complex dynamics of individual and organizational learning and change across these different traits. Particularly important, of course, are the interdependencies among those two attentional dimensions, as they influence the co-evolution of organizational and individual processes.

Finally, I have tried to show that understanding the micro-foundations of evolutionary processes is actually doable empirically. However, that requires a significant effort along several dimensions. First, penetrating deeply inside organizations and staying there for a sufficient amount of time so that we can actually observe the differences, similarities and co-evolutionary processes of these traits as they enfold across the organization. Second, if one wants to obtain generalizable conclusions, this needs to be done in a sufficiently large number of organizations and with comparable designs, which is only possible if we learn to work in large collaborative research programs for which management scholars are particularly ill-prepared. Third, to really explore the individual bases of organizational behavioral outcomes, we need to learn how to work not only in large contingents of like-minded colleagues, but also with scholars from other disciplines with theories and methods that are foreign to us. Learning to feel comfortable in our ignorance as we work with experts in knowledge domains complementary to ours is not at all easy, but I do not see any way around it if we are serious about achieving progress on the micro-foundations agenda.

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