

Generality, Specificity And Discovery

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GENERALITY, SPECIFICITY AND DISCOVERY

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Abstract

This paper offers a meta-theory concerning the relation between the general and the specific in science. This issue was recently called back to attention by Hodgson (2001). A heuristic of discovery, developed in earlier work (Nooteboom 1992, 1996, 1999b, 2000a), is used in an attempt to contribute to an understanding and a resolution of the tension between generality and specificity. That tension can be resolved if we look at general theory and specific conditions (or data, experience) not as separate entities or approaches that one has to choose from, in a static perspective, thus choosing to be a generalist or an empiricist, but as complementary, in a dynamic, dialectical process of theory development, in the process of discovery. The paper argues that there is an alternation of the general and the specific, in an ongoing cycle of formation and application of theory. Generalisation and abstraction are necessary to lift experience from specific contexts and carry it into new contexts with their own specificity. That, in turn, is needed for the theory to face failure and collect the experience that will lead to new generalisation. In the face of failure, adaptations are made to the specific context, in differentiation, and hints are found for novel specific elements to be absorbed, which yields hybridisation. This exerts pressure, and provides the material and the directions, to develop a new unity out of novel combinations, and we are back at the beginning of the cycle.

Key words: specificity, generality, discovery, learning,

Introduction

In a new book in progress, entitled 'How economics forgot history: The problem of historical specificity in social science', Hodgson (2000) rekindles a discussion concerning the relation between generality and specificity in science. He claims that it was a central issue in the German Historical School in economics, in the nineteenth century, and has since been forgotten or ignored. He defines generality as the claim that central features are shared across different contexts in time and space. Specificity, on the other hand, entails that crucial features differ across such contexts. Hodgson focuses on historical specificity, but also includes specificity of place. Here, I will go beyond historical and local specificity to specificity more in general. That includes specificity of the domain of attention (labour, production, trade,...) and what I will call human specificity. By that I mean that different people at the same time in the same place will be entangled in their own specific nexus of genetic endowment, birth, family, friends, work, yielding different experiences and thoughts. This aligns with the notion from Austrian economics that different people think different things.

Hodgson recognises the need for both generality and specificity. Concerning generalisation and simplification, he recognises that 'Without a more general framework, specific theory is impossible' (p. 36). However, at this stage in the development of social science he emphasises specificity because he finds that it is being neglected, and that a one-sided preoccupation with generality has hindered the development of economics. The need for specificity comes out most clearly in historical analysis. The example Hodgson uses is: how can we use the same theory for feudal and capitalist societies? Economic conditions and processes, such as preference formation, exchange, price formation, transaction costs etc, are crucially affected by locally and historically specific institutions. One cannot successfully maintain a general theory and tag on specific institutions afterwards. Institutions enable and constrain conditions and processes, and they are accompanied by pathdependence. The predilection for generality has not only theoretical but also political and ideological repercussions. For example, it presently generates the notion of global convergence of economic systems and the intent and effort to transfer the norms, rules, and conditions of present American capitalism across the world. Hodgson (2000: 26) complains that '.. the typical debate between general theory and empiricism is far too crude. In truth, both sides have their flaws ... Accordingly, any attempt to steer a middle way between these poles is likely to share the limitations of each'. And he sets the agenda:

'A more sophisticated position has to be found, recognising a significant role for general theories but also their limitations. Some kind of middle-range theory .. is required to bridge the general with the empirical'.

That is the challenge taken up in this paper. However, I am not sure how to deal with 'middle range theory', and I would characterise my approach as an attempt to show how generality and specificity are related in a dynamic relation of development; in a dialectic between the two. If this sounds too Hegelian or Marxist to the taste of the reader, so be it. The notion of dialectical process also, I would like to add to Hodgson's comments, is ripe for retrieval from a neglect which is partially based on misunderstanding. There is an urgent and long neglected need to come to grips with dynamics in economics, not in any technical sense of time-subscripted variables in an econometric model, but in the sense of transformational change, including learning, discovery, meaning change, and radical innovation. Even Schumpeter has not given us a theory of the origins of innovation and discovery. In the effort to fill that gap, renewed attention to thought on dialectics might be fruitful.

For a contribution to the debate one needs a methodological basis, including one's view of ontology and epistemology, and that is where I will start. I will combine a constructivist theory of knowledge with a realist methodology, and I call this combination 'constructive realism'. Some time ago I was attracted to postmodernism for its criticism of universalism, its acceptance of context-dependence of meaning, its attention to diversity and ongoing change of meaning and knowledge, and its constructivist view of knowledge. Then I discovered the errors of postmodernism: its extreme and fruitless relativism, leading to neglect or even rejection of critical debate, and the inconsistency of its own position. So I rejected it, for much the same reasons that Hodgson did. But I may have something to explain here: how can I reconcile a constructivist view of knowledge with a realist methodology?

Subsequently, I will briefly indicate the heuristic of discovery that I developed elsewhere, and I will apply it to the relation between the issue of the general and the specific in science. In many places I will use my recent book (Nooteboom 2000), and I will not keep on repeating that reference. Finally, I will train the issue of generality and specificity to my own theory. Is it general, what are the arguments for that, and how susceptible is it to incorporation of and modification to the specific. How does it satisfy its own story?

Realist methodology

The notion of realism can have several connotations, as reflected in the following questions (cf. Vromen 1995). In capitals, I attach the answers that I give.

1. Should our theoretical assumptions reflect our beliefs about how the world works? YES
2. Should our theory aim to provide a basis for application to economic or business policy? YES
3. How much wealth of detail should theory include? AS MUCH AS NEEDED FOR THE PRACTICAL PURPOSE, IN A TRADE - OFF BETWEEN RICHNESS OF DETAIL AND SIMPLICITY OF ANALYSIS.
4. Can we have objective knowledge of the world? NO; AT LEAST WE CAN NEVER KNOW WHETHER WE DO
5. Do facts represent objective knowledge? NO
6. Is there a reality that can help to detect our errors of thought? YES
7. What items exist in reality? NEXT TO THE PHYSICAL FURNITURE OF THE WORLD, AND PEOPLE, THERE EXIST ORGANISATIONS, INSTITUTIONS, CULTURAL PRODUCTS AND SOCIAL STRUCTURE, IN THE SENSE THAT THEY EXERT CAUSAL INFLUENCES

The function of theory is to provide abstraction, and theory cannot and should not reflect all that we observe. Yet I adopt a realist philosophy of behavioural science. I don't claim originality with my standpoint: I share it with many contemporary heterodox economists. I doubt, for example, that my position is different from that of Hodgson, except perhaps for the details. Nevertheless, perhaps I should summarise my arguments.

Economic theory should be plausible in the sense of being based on behavioural assumptions that are realistic, i.e. that can be seen to apply. There are two arguments for this. The first argument is that it is not enough to yield correct predictions on the level of markets and industries. As Popper argued, falsifiability is a necessary condition for such empiricism. It is widely recognised that in economics predictions are shrouded in *ceteris paribus* assumptions that cannot themselves be tested, and as a result the empirical grip of falsification on theory is weak. Especially under these conditions we should recall the implication of the Duhem - Quine thesis: theory is underdetermined by its predictions and implications. When a theory makes correct predictions, there can be an infinity of alternative theories that predict equally well. Thus we need an additional selection criterion to select among empirically equivalent theories. Such a criterion used to be simplicity. However, that is no longer self - evident, given the increased computational power that we now have. Incidentally, this may be an example of historical specificity: methodology depends on conditions.

Another criterion, then, is plausibility, i.e. realism of behavioural assumptions, in the light of direct evidence and insights from psychology and sociology in the motivation and causality of behaviour. This criterion still carries some of the notion of simplicity in the sense that it increases coherence in behavioural science (Nooteboom 1986). That is still important in spite of increased computational power. Note that the implications are that methodology is not the same for economics and natural science. This is an example of specificity concerning domains of knowledge. In physical science there are fewer *ceteris paribus* assumptions and there is more scope for testing under controlled conditions. Furthermore, there explanatory assumptions typically cannot themselves be tested directly, and we can only test them by their implications for what we can observe. By contrast, in behavioural science the assumptions pertain to observable behaviour and can be tested directly. The only question is how much detail from observed behaviour they should include. Such observation is in turn informed by theory: sociology, cognitive science and other branches of psychology.

I grant that the implication of my view probably is that we should integrate perspectives from economics, sociology, psychology and cognitive science in a new, encompassing behavioural science. I accept that

implication, and I think that it is feasible, that the time is right for it, and that some of us are doing it. I look on in astonishment how economists are rediscovering sociology, rather than using it, and present views that are extremely crude and primitive from that perspective. They seem to be flogging the dead horse of mainstream economics rather than getting on with the job. Likewise, sociologists are re-inventing economics rather than using it. And both economists and sociologists make claims in the area of cognition and motivation that ignore cognitive science and psychology.

The second argument for plausibility is that for practical reasons theory should contribute insight into causal processes that produce outcomes on the level of markets and industries. In other words, explanation, in the sense of reproducing observed phenomena from the deductive structure of theory, in the behavioural sciences should include understanding of how causal processes work to produce the outcomes. What use are predictions on the level of markets and industries if they yield no basis for the analysis of action, for policy? Policy makers do not intervene in outcomes but in the processes that yield outcomes. If they could interfere in equilibrium outcomes, those would not be equilibrium outcomes. Often they do not interfere directly or even intentionally, but policy will in principle have direct and indirect effects, and one needs insight in causal process to have any chance of assessing and anticipating the effects. Agents, playing roles in social structures of organisations, form the direct, efficient cause that produces outcomes on the industry or market level, and therefore, in order to contribute insight in causal process, theory should yield empirically testable and tested assumptions concerning behaviour. For example, many economists accept that in technology policy government intervention may be justified 'since there markets are not always efficient'. So, we must understand how technology develops and innovation takes place. Among other things, that requires a theory of learning and discovery. Where is it?

Constructivist epistemology

I employ a constructivist, interactionist theory of knowledge. People perceive, interpret and evaluate the world according to forms or categories of thought that they have developed in interaction with their physical and social environment. This view is not new: up to a point it employs ideas from the symbolic interactionism of G. H. Mead (1934, 1982) and the developmental psychology of Piaget and Vygotsky. Their view is that intelligence is internalised action, constructed from interaction with the physical and social environment. Piaget indicated how cognitive development might proceed in a sequence of stages. Vygotsky indicated, in contrast with Piaget, how the construction of intelligence is socially embedded and supported from the very beginning of child development.

Cognitive construction builds on our bodily and neural make - up, as developed in biological evolution: cognition is rooted in the body (Merleau - Ponty 1964, Lakoff and Johnson 1999). Neural structures develop as tentative structures, selected and reinforced on the basis of success in the physical and social environment (Edelman 1987, 1992). This connects with the pragmatic (or 'pragmaticist') view of knowledge (Peirce 1975) that truth and meaning are based on what works, rather than on untestable claims of coherence with objective reality. It also links with Wittgenstein's idea that meaning and correct reference are based on viable use, reflected in 'rules of the game'.

My view of cognitive development can be summed up as follows: An endowment of genes yields a flexible but still limited ability to construct neural structures. This structuring occurs in interaction with the physical and social environment. Tentative cognitive structures are reinforced by success in that interaction. This is the basis for cognition and language, which yield a basis for action, which affects the environment. This social embedding provides a link with, and a cognitive underpinning for the interactionist perspective taken in all my recent work.

This social constructivist perspective is related to the perspective of communitarianism in political philosophy, as opposed to the liberalist view of Rawls. The communitarian view is that people do not choose their values but are constituted by them. People develop their identity in interaction with their social environment. Liberalists take an unacceptable, solipsistic view of individuals, while communitarians tend towards an authoritarian subjection of individuals to the dictates of the dominant opinion. But there is a middle way between those views. This has been developed, for example, by Habermas (1982).¹ One can very well maintain that people 'make sense' and construct categories, and thereby develop their identity, in interaction with others in a social community, and yet allow for that

¹ Habermas also employed the ideas of G.H. Mead, and even those of Piaget, though in a very rudimentary fashion.

identity to become individualised, so that the individual can exercise a more or less independent view, choice and ethical judgement.

Constructive realism

How can my constructivist epistemology be reconciled with my realist methodology, as specified above? Doesn't a social constructivist theory of knowledge inevitably lead to relativism, in which any theory is as good as any other, and the surrender of any debate in terms of 'truth'? How can I dodge and refute postmodernism?

I acknowledge that my view does indeed yield a brand of relativism. But it is not the radical relativism of most post - modernism. Since cognition is constructed in interaction with the world it is not arbitrary, and is constrained and enabled by reality, at least as a material cause. In that sense it 'embodies' reality (Lakoff and Johnson 1999). Lakoff and Johnson (1999) gave the argument that since our cognitive construction is rooted in the body, and people share bodily processes as an inheritance from evolution, their cognitive processes and constructions are bound to be similar to some extent. This is reflected in the fact that basic metaphors in thought are shared across widely different cultures. Another argument is that the physical environment, which is part of the environment in interaction with which we construct cognition, is also shared and subject to universal laws of nature. However, that does not detract from the fact that categories are constructed and that we are unable to descend from our mind to inspect how our ideas are hooked on to the world. Constructed categories enable but at the same time constrain cognition. Cognition is to some extent idiosyncratic and path - dependent: to the extent that people have evolved separately and in different environments their cognition varies (Nooteboom 1992). 'Higher level' cognitive constructs are built largely on social interaction, and the environment for that varies greatly. Thus, within constraints there is variety of cognition. Note that it is precisely because we cannot climb down from our minds to see how our knowledge is hooked onto the world, and because other people perceive and interpret the world differently, that we must listen to other people in search of truth, or the best approximation to it that we can achieve. Short of the long - term selection effects that reflect reality, at any point in time other people are the only source we have for finding out about our prejudices and errors. Critical debate is more important than ever.

The answer to the problem of how I can reconcile a realist methodology with a constructivist epistemology is this: in the same way that Karl Popper made his falsificationist methodology consistent with the fact that, as he granted, observation statements that form the basis for falsification are 'theory laden'. To the extent that we can agree about observation statements, in spite of differences in theoretical view, we can agree about the falsifiers, so that the procedure of falsification can work. Popper underplayed the possibility that differences in theoretical perspective are so fundamental that no such agreement about 'the facts' can be reached, in claiming that we can 'at any time' step out of the prison of our categories. Consider the following quote (Popper 1976: 56).

'I do admit that at any moment we are prisoners caught in the framework of our theories; our expectations; our language. But we are prisoners in a Pickwickian sense: if we try, we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again'.

But Kuhn overplayed his thesis of 'incommensurability' between different 'paradigms'. Not just in spite of the constructivist view, but indeed following from it, people will agree on the facts to the extent that they have constructed their categories in a common physical and cultural environment. Thus a theory can be realistic in the sense that it takes into account the facts as we construct them intersubjectively. As Popper indicated, this does not give us any 'rock bottom' foundation, since we may be mistaken in our facts, but they are generally more stable and more reliable than theoretical hypotheses. Note that all this is not intended to imply that I accept Popper's falsificationist methodology; just that I accept his notion of socially constructed facts.

On the basis of this constructive realism I reject both the methodological individualism of mainstream (and Austrian) economics and the methodological collectivism of some sociology. To replace both, I propose 'methodological interactionism'.

Human specificity

My constructivist, interactionist theory of knowledge entails that people think different things to the extent that they have developed in different contexts and have not communicated among each other. Since no two people will have identical genetic endowments, and identical conditions of birth, parents, further family, friends, education, work, etc., there will never be identity of thought, feeling and meanings attached to or associated with linguistic or other expressions. This is what I call human specificity, which goes beyond, or deepens, historical and local specificity. No complete communication, without any loss of intended meaning or collateral associations is ever possible. In communication one has to accept loss. One needs to simplify and abstract from the fullness of one's thought and associations. My view of cognition has important implications for the theory of the firm. Those are relevant here for the following reasons. By showing how cognition is linked to the purpose and behaviour of firms, and relations between firms, we may be able to connect theory of knowledge, science, the firm, market processes, and innovation into a wider theory of historical change. Therefore I give a brief summary of the implications for the theory of the firm.

As a result of construction by interaction, cognition is cumulative, and to a greater or lesser extent idiosyncratic and path - dependent (Nooteboom 1992): past experience determines 'absorptive capacity' (Cohen & Levinthal 1990). People and firms have different knowledge to the extent that they have different experiences and little interaction. In other words: there is 'cognitive distance'. As result, the primary function of the firm may be cognitive, as a 'focusing device' (Nooteboom 1996): in order to achieve anything at all, a firm must direct and align perception, understanding and evaluation by the people connected with it. This connects with the Schumpeterian idea of the entrepreneur as a charismatic figure who not only combines resources but also aligns people in their cognition and purpose (cf. Witt 1998). But this solution to the problem of cognition and action raises another problem: by focusing in one direction one runs the risk of missing out on perception of opportunities and threats from other directions. The need for such focusing as well as the problem of myopia that it involves are greater to the extent that the environment is more complex and variable, and to the extent that firms must strive to differentiate their products. For several reasons there is such a tendency towards 'radical product differentiation' (Nooteboom 1999a).

To hedge the risk of myopia, one needs complementary, outside sources of cognition: cognition by others, which is relevant but also different. I called this the principle of 'external economy of cognitive scope' (Nooteboom 1992). Such outside sources of complementary cognition require a 'cognitive distance' which is sufficiently small to allow for understanding but sufficiently large to yield non - redundant, novel knowledge. For the external source to maintain novelty it is crucial to maintain distance. The argument indicates that in order to co - operate one may have to invest in sufficient knowledge to understand the partner and achieve sufficient absorptive capacity.

The argument yields a new theory of the boundaries of the firm, which, I propose, is more fundamental than the argument of transaction costs. One could, perhaps, say that it is in agreement with transaction cost thinking in an extended sense, where transaction costs include the issues of cognition and the reducing or crossing cognitive distance discussed above. But that would seem to stretch the concept of transaction costs so far that it breaks.

The need and motivation for change

Now I proceed to apply, in the present context, the heuristic of discovery developed in earlier work. The central question in that work was: what is the basis and process of cognitive change and innovation, in people, organisations and economies? The process I propose is not necessarily optimal (there would be no basis for that), but generally is reasonable, workable, and most likely to succeed in face of the radical uncertainty involved in discovery. That is why I use the term 'heuristic' rather than 'logic' of discovery. 'Logic' would sound too rational, inexorable, unconditional and universal.

Now, the first question concerns the need and motivation for change. As relations last, among people in similar circumstances, without outside contacts, cognitive distance between them will decrease and learning will slow down. New knowledge, or discovery, will then require that one widen one's scope of interaction. In other words, one needs to enter into new contexts, in a general sense. This includes the involvement of new people. For the firm this may entail job rotation within the firm, or greater personnel turnover, as one instrument to enhance innovation. A further step is to engage in outside relations with firms at the appropriate cognitive distance: distant enough for novelty, but close enough for understanding. In science it may mean the introduction of novel sources of thought, including the

importation of perspectives from other disciplines. It can also include a renewed look at ideas developed in history and forgotten or neglected since.

Another, further step is to introduce one's cognitive or other productions into new locations or domains. For firms this would mean introducing products into new markets, or trying novel applications of existing technology. For science it would mean the exploration of applications of existing theory to new phenomena, thus widening the domain of application. As suggested before, the transfer of thought and associated practice to new contexts requires that it be disentangled from the richness of context specificity. One cannot carry over the whole 'seamless web' (Quine) of one's knowledge and its embedding in specific, local circumstance and history. Detachment is needed, which entails simplification, or abstraction. This is one argument for the reduction entailed in generalisation. This is an extension of the fact, mentioned before, that loss of specificity applies even in communication with other people, no matter how close they are.

Transfer to novel contexts, of whatever type, in whatever realm of cognitive activity, entails the re-absorption of context specificity. While generalisation, to escape from the specificity of context, entails reduction and simplification, entry into novel contexts requires expansion with local specificity. Again, this applies even in communication with people: simplified knowledge and meaning has to be embedded in the human specificity of the interlocutor for it to work, and this is an opportunity to learn on the basis of its failure.

More generally, it is only by introducing generalised theory and practice into novel contexts that one gives oneself a chance of exploring the limits of validity. This serves to build up the motivation to surrender existing frames of thought or practice. As argued already by Kuhn, one does not and should not surrender theory at the first appearance of falsifiers. One surrenders the 'investment' in the building, simplification, abstraction, and formalisation of existing knowledge only when the cumulative weight of evidence makes it worthwhile.

In firms, that investment entails the honing down of practice for increased efficiency, the achievement of a division of labour that is needed for some forms of economy of scale. An important contributing factor is that after innovation increased competition, resulting from spillover and imitation, which erodes profits reaped from the initial partial monopoly of the innovator, forces one to shift attention to costs and to utilise the potential for economy of scale that the expanding market allows, as more customers become familiar with the innovation. There is mutual reinforcement here: the lowering cost of economies of scale and cumulative economies of experience contribute to the growth of demand. Such an investment is scrapped only when the competitive need arises. Also, within organisations, for people to accept the need for change and to surrender the familiarity, ease and efficiency of learning by doing, and the reduction of uncertainty that it entails, the need to make that sacrifice must be manifest and convincing.

In science, the equivalent investment is that of abstraction and formalisation, eliminating what is not crucial, for the sake of cognitive and communicative efficiency, and strengthening the analytical grip of theory, calling into help the razors of logic and mathematics. My first argument for abstraction was that it is needed to disentangle experience from a specific context, in order to profit from it elsewhere. Here there is a second argument for abstraction, and an argument for formalisation: to increase efficiency and analytical grip.

There is an argument here for tenacity: for not surrendering existing forms of thought and action too easily. It is mainly an economic one: do not surrender investment until the need for it is manifest and compelling. The question is, of course, when rational tenacity degenerates into irrational conservatism. I will deal with that question later.

The basis for transformational change

The argument for tenacity is not only economic. In spite of his condemnation of theoretical conservatism, Popper also supported the principle of tenacity. If we do not hang on to theory for some time, in spite of falsification, we will not find out where the real strengths and weaknesses of our theory lie. Then there would be no accumulation of experience and criticism, and we will not discover the limits of validity more fully and systematically. We will not be able to be precise and specific in our criticism, and thereby we will lack the basis for improvement. Search would become blind. We would erratically swerve from one ill-developed framework to another, and there is a danger that each scholar will develop his own forms of thought, resulting in confusion and lack of joint, cumulative effort. This is one of the problems, perhaps the central one, of postmodernism. In fact, some of this shows in the area of business and

management, where there is a tendency for different scholars to invent their own new terms for the same or similar phenomena, with yet another classificatory scheme, overlapping in uncertain ways with those of colleagues, generating ambiguity and unclear duplication. In other words, next to the economic argument for tenacity, there is an issue concerning the basis on which change could take place. I will now analyse that more systematically.

Again, a thesis that is central to my argument is that to learn we must transfer theory from the context in which it was developed to novel contexts. That gives us the chance to discover limits of validity and the motivation, the material and the directions for change. Let us see how this might work.

As one runs into shortcomings, and the need to face the implications, in novel contexts, one tries to differentiate existing knowledge and practice to perform better. A sensible approach would be to try and adapt existing practice in proximate ways, preserving the investment as much as possible. One way to do this is to hark back to past experience in developing it. What other options were there at the time? What alternatives were tried in the search for optimal design? Could they be recalled and tried again in the new context? However, one may have neither the time nor the competitive opportunity for such a gradual form of differentiation. And it may turn out not to suffice. Then one has to look further, across the boundaries of current practice and past experience.

As in crime, the transgression of boundaries requires not only motive but also opportunity and means. It is only by moving existing knowledge and practice into novel contexts, and thereby detecting shortcomings and gaps, that one obtains not only the motive but also the opportunity for dismantling existing structures of thought and practice. In novel contexts one has the opportunity of finding out that alternative or competing practices, geared more to the specificity of that context, in some ways perform better than one's own. This suggests opportunities for importing new elements into existing structure for better fit and survival in that context. For this I have used the term 'reciprocation'.²

However, such importations of foreign elements into existing theory from the novel context may not fit. Indeed, they are not likely to do so to the extent that the new context is removed from the context in which existing structure was developed, with heterogeneous specificities. Then, the inevitable result will be hybrids. One problem with those is they tend to be messy and ugly, and do not satisfy the scientist's, and especially the mathematician's taste for simplicity and 'elegance'. In science, this is the problem of syncretism, which tends to be seen as a scientific sin. And indeed, there are valid objections to hybridisation. It yields inefficiencies in practice, communication and debate. There no longer is a unity of terms. There is ambiguity, as meanings start to expand and shift. Furthermore, hybrid structures entail duplication of elements at different places in a structure of thought or practice.³ That is economically and cognitively inefficient. In economic contexts, it eliminates opportunities to utilise economies of scale. A more important and fundamental problem is that for novel elements adopted from the novel context to be fitted into existing structure, they have to satisfy constraints that may seriously inhibit the realisation of their potential. That is why often innovations start out, not where their potential is fully utilised, but where they can be fitted into existing structures. However, as evidence for this under-utilisation accumulates, the motivation arises to contemplate a more radical, structural, transformational change, with novel structures or architectures of elements from old and novel contexts, in Schumpeterian 'novel combinations'. This I call the stage of 'accommodation' or 'novel combinations'.

What such novel structure should look like is not known: such novelty entails fundamental uncertainty. As a result, there is a lot of trial and error. However, it is not completely blind and uninformed, as the generation of novelty by mutation of genes or cross-over of chromosomes in biological evolution is. It is informed by past experience, built up in the process of discovery outlined here. One heuristic is to switch the point of departure for experimentation. Not existing architecture of theory or practice, but a new set of those elements taken from old and new contexts that appear to most promising, as candidates for eliminating current constraints on their potential. This heuristic can be observed in many innovations that have taken place in history. Another heuristic is reversals of roles or logic.

² Both the terminology and the inspiration for my argument were adopted from Jean Piaget's work on the cognitive development of children.

³ Elsewhere, in Nootboom (1999b, 2000) I specify the theory in terms of scripts, which are networks of nodes that represent different components of activity or thought. In those terms, the problem is that a given node is duplicated in different places in the script.

An example is the invention of self-service in retailing (Nooteboom 1984). In the old form of service firms, the shop attendant collects the goods for a static customer waiting at the counter. In self - service, the customer collects the goods and pays to a static attendant at the check-out point. This invention was enabled by the emerging condition that customers no longer needed advice and help in the selection of the goods. An incentive for the new system was that it saved on labour, while reducing queuing time for customers. That was important as labour became more expensive. Here, roles were switched between attendants and customers.

Trials of new structure require a period of experimentation. Then, when success of a ‘dominant design’ materialises, as the survivor of the selection process in the institutional context in which the new form is developed, we re-enter the stage of simplification and abstraction for the sake of efficiency. That I call the stage of ‘consolidation’.

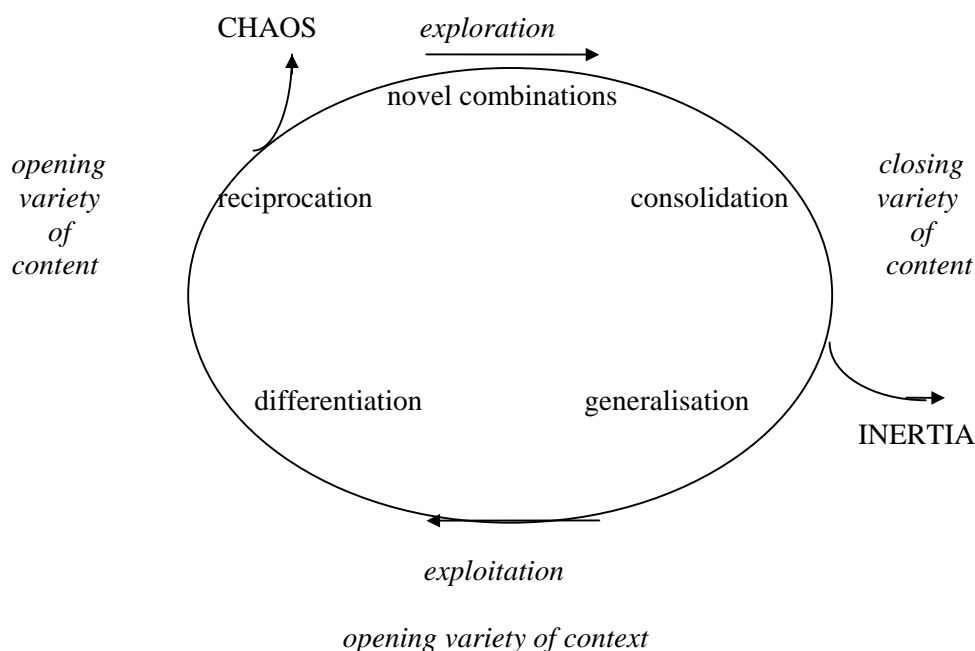
A cycle of discovery

I propose that along the lines indicated, there is an alternation of generalisation and specificity. More in detail: a cycle of consolidation, generalisation, differentiation, reciprocation and accommodation. In earlier work, I framed this in the context of the debate, in the business literature, on how firms can combine the exploration of existing resources or competencies with the exploration of new ones. The literature had indicated that firms must do both. Exploitation is required to survive in the short term. Exploration is required to adapt and survive in the long term. But that is a paradoxical demand. Exploitation is essentially a narrowing process: elimination of redundancies for efficiency, and formalisation for the division of labour needed for (some kinds of) economy of scale. Such division of labour gets translated into cognitive focusing, organisational structure, training, the learning of roles to play, and routinisation. That requires stability and preservation of existing divisions, boundaries and meanings. Exploration, by contrast, requires the widening of scope, loosening of structure, shifts of meaning, transgression and shift of boundaries. How are such contrary processes to be combined? The cycle of development is my answer. It is by carrying existing practice into new contexts that one continues exploitation while contributing to exploration. This is illustrated in figure 1.

Here, I apply the argument to science. To connect the two applications I would say: generalisation in theory, in the sense of reduction, i.e. elimination of local specificity, is needed both for efficient exploitation, and as a basis for moving away from local specificity, in order to carry it into new contexts. That, in turn, provides an opportunity for exploration, with the need to expand theory with new local specificity, and thereby create the motivation and need for differentiation of theory, the creation of hybrids that develop into hopeful monsters that, like Shiva, yield creative destruction. As indicated by Hodgson we cannot choose either generality or specificity. We can combine the two in a dialectic of discovery.

I think there is a connection here with Blaise Pascal’s distinction between the ‘esprit de geometrie’ and the ‘esprit de finesse’ (Pascal 1977). This crops up in methodological debates in economics. Non - mainstream economists object to the excessive formalisation of mainstream economics, in the esprit de geometrie. Like Hodgson, they plead for more finesse of specificity. As characterised by Pascal, esprit de geometrie is difficult, because it entails abstraction: a turning away from the wealth of detail one sees before oneself. It is also easy, because it allows for the strong grip of logical and mathematical argument that keeps us from errors and shallowness of logical inference. The ‘esprit de finesse’ is easy, since the finesse of the richness of local specificity is right before our eyes. It is also difficult: not to enter into errors of thought and inference while preserving all that richness. Pascal was a genius in the first, but later in life contemplated the second (to the point of writing an apology for religious faith). He concluded that we need both, in interaction and alternation. This is incorporated in my cycle of discovery. Consolidation entails esprit de geometrie, differentiation and reciprocation entail esprit de finesse.

Figure 1: cycle of exploitation and exploration



Conservatism

Progress along the cycle of discovery is not guaranteed. There are several possible problems, but here I focus on the problem of conservatism. In figure 1 this is labelled as 'inertia' (a term taken from organisation science and population ecology). In inertia, consolidation leads to a generalisation which is unable to differentiate sufficiently in novel contexts, and is a fortiori unable to allow for hybridisation and novel combinations. This, I think, is the main problem that Hodgson indicates. Such inertia can have several causes. There may be no compulsion or opportunity to transfer theory to novel contexts. When such transfer does take place, there may be no compulsion or ability to face the incompleteness or lack of fit to local specificity. Or there may be no compulsion to accept the consequences, with the need for differentiation. There may be no willingness to incorporate novel elements from novel contexts. There may be a taboo against hybridisation. And when all these hurdles are passed, there may be no willingness or power to demolish existing architectures of thought and practice to allow novel elements from outside to realise their potential. Lakatos' proposal that existing theory or 'research programmes' are to be surrendered only in the light of a viable alternative is conservative (Nooteboom 1990). It ignores what the process of discovery requires: the ambiguities and ugly but hopeful hybrids that have to be allowed along the way.

In the fight against conservatism, an evolutionary argument kicks in: those who do not face and accept the need for differentiation, reciprocation, and accommodation will fail in exploration, and will sooner or later be eliminated by competition. The condition is, of course, that such competition is given a chance, and cannot be warded off by entry barriers. Firms try to build such barriers, and so do scientists. In science, the force of criticism depends on falsification and competitive entry of new ideas.

From this perspective the problems with mainstream economics are manifold. One is that much of it is not even carried into empirical work and application, and is thus deprived of chances of falsification and sources of discovery. It is allowed to dodge the challenge of dealing with new specificity. If it is applied, it is not falsifiable. The unfalsifiability of mainstream economics is well known, and I will not repeat the argument for it. Furthermore, powerful entry barriers are in place. Superficially, these are the doing of editors and review boards.

The deeper problem, whereby their conduct is legitimised, is two fold. First, there is the familiar confusion of the normative and the descriptive. Economic theory is defended on normative grounds: this is how markets should be, or would be under non-existent ideal conditions. And then there is a sly sleight

of hand. The normative justification is a cover for falsely claiming descriptive truth. From the argument that this is how economies should be one moves to claims and applications as if this is how it is. An argument for entry barriers is the argument for theoretical tenacity, indicated before: one should not scrap the investment in theory until the need arises. Underlying that there is, of course, the preservation of reputation: the famous will not easily accept attacks that would destroy the basis for their reputation and position, and they can exert power to block entry through their position as gatekeepers. Obviously, this is never admitted. The argument for tenacity is, as indicated, that one should stick to a theory, or a Lakatosian research programme, until a ready alternative presents itself. What they demand is something like the goddess Athena bursting forth fully armed in birth from the brow of Zeus. That happens only in such myths. If entrants manage to pass these barriers along the unobserved side roads of disreputable journals, and show the fruitfulness of novel elements, the inevitable hybridisation that would result is rejected for its lack of precision and efficiency, its elimination of simplicity, and the resulting unsusceptibility to rigorous formalisation, in violation of the *esprit de geometrie*. The problem is so deep because the arguments are partly valid, and create a semblance of legitimacy. Those are the arguments for tenacity, the value of simplicity, the power of formalisation, and the inefficiency of syncretism; the ugliness of the hopeful monsters of hybridisation. Those arguments, however, are misunderstood and misapplied. Tenacity is valid, ultimately, only as a means for the sharp identification of limits of validity. Hybrids are to be rejected not with the aim of barring the entry of new ideas, but with the aim of facing the consequences of their usefulness, in attempts at transforming existing architectures of thought into novel ones, which can then be made susceptible to novel forms of simplicity and formalisation.

How general is this?

Now let the snake bite its tail. What is my claim for the generality of my own theory? How susceptible is it to incorporation of specificity? Does it allow for the process that it professes?

First the claim of generality. On what grounds do I have the gall to propose a general theory of change, applicable to learning by people, learning in and change of organisations, innovation in economies, institutional change (I have not argued that here, but see my 2000 book), science, and indeed, perhaps, all human cognitive and other productions? To follow the definition of generality proposed by Hodgson: what features do I claim that they have in common? I have two things to say on that. The first is that all these are faced with the need to combine exploitation and exploration, and I have offered a general heuristic for that. The essence of that is the alternation, and dialectical interaction between generality and specificity, whereby they provide the basis for each other, and the process of transformation in which that can result.

The second argument is my conjecture that the process is driven by an inherent human drive of imperialism: of expanding the domain of present thought and practice to novel contexts. My argument is also an evolutionary one: I conjecture that we have that drive because it engenders the process of discovery, which has helped man to evolve as a social animal. In other words, the argument is one of evolutionary psychology. Note that I am not saying that people have the imperialistic drive with the express, conscious purpose of the learning that it may engender. They have the drive, and it may yield learning, and that consequence of it has survival value. A necessary, but perhaps not sufficient, condition for this to hold water is that it is also accompanied by the ability to dodge the threats of conservatism. Again, the argument for that is evolutionary: if one cannot muster that, one will not survive. Again, that applies only if indeed the escape from conservatism is necessary for survival. That may have been valid in past evolution. It may still be valid in many markets. But, as discussed, mainstream economics has managed to dodge that need, and that seems to apply elsewhere, in the history of ideas. The stringency of the selection mechanism in the evolution of ideas may not come near to that of earlier and other forms of evolution. Note, finally, that I do not claim that imperialism is good also in other respects. It has caused great misery. Especially when it could immunise itself against the need to adapt to local and human specificity.

If imperialism sounds too ominous, consider play. Largely, that also entails the transfer of existing skills (first of the sensori-motor kind, then linguistic and cognitive) to novel contexts of application. This is crucial for learning to engage in the process of learning that I have described. There is also a connection with humour. That tends to be based on incongruity, transfers of existing experience into surprising other

contexts. It is also related to metaphor, and its use in poetry.⁴ So, pretentious as it may seem, I propose a general urge to transfer cognitive and other productions to novel contexts, which links imperialism, play, humour and poetry. Our inclination to like all that has survival value in the process of learning. The other question was whether my theory satisfies itself. Is it susceptible to incorporation of, and adaptation to, the exigencies of local specificity, and to a process of transformational change by which it will destroy itself? That, of course, remains to be seen, but there is already much to say on this. First, I have hedged the universality of the cycle of discovery by presenting it as a heuristic, not some inexorable march of logic. There are reasons, consistent with both facts and the theory, why the operation of the cycle, and the salience of any stage in it, depend on the specific setting of industry, technology and institutions. In some industries/technologies the combination of exploitation and exploration is much easier than in others. The problem of inertia depends on the complexity, stringency and needed durability of linkages in division of labour. Those are related to opportunities for economy of scale, and the size and sunkness of investment in capital goods and organisation. It is quite different between, say, the process industry (base chemicals) and consultancy firms. In other words: the cycle allows ample room for differentiation to take into account the specificity of context. I have not yet conducted similar analyses of theories and disciplines. However, my account of the conservatism in mainstream economics, given above, indicates opportunities for such analysis. Is the theory also amenable to reciprocation? Originally, I did not think of the problem of historical specificity emphasised by Hodgson. The application of my theory to that context has caused me to include the possibility of reaching back in the history of ideas as a source for differentiating and adapting theory. Originally I had included only specificity of place and domain, and human specificity. Another challenge is the following. With interaction between people and firms being so crucial in the theory, related issues of governance should be included. That connects back to earlier work on inter-firm relations (Nooteboom 1999a), and currently I am trying to develop that line of theory and connect the theory of discovery with a theory of governance (Nooteboom 2000ab). I have made a beginning of adapting the theory in an analysis of the conditions under which stages in the cycle may be skipped, or reversed, in an attempt to explain what firms are doing now, in the new economy. Can my theory deal with the historical specificity of economic systems? My overall answer would be that rather than trying to supply general principles for those different structures, I would focus on how they have arisen and how they change, and that is where my claim of generality would lie. In other words, I would focus not on the products but on the process of history. Hodgson contrasted feudal and capitalist systems. The test of my theory would be whether it can explain for both how they arose, changed and were or are transformed. I have not yet performed any of such tests.

⁴ In Nooteboom (2000) I include a discussion of language and meaning change, with metaphor as a specific case of what I here call 'reciprocation', in a theory of poetics.

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