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**Bounded Rationality in the Economics of Organization: Present Use and
(Some)Future Possibilities**

By

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Abstract

The way in which bounded rationality enters contemporary organizational economics theorizing is examined. It is argued that, *as it is being used*, bounded rationality is neither necessary nor sufficient for producing the results of organizational economics. It is at best a rhetorical device, used for the purpose of loosely explaining incomplete contracts. However, it is possible to incorporate much richer notions of bounded rationality, founded on research in cognitive psychology, and to illuminate the study of economic organization by means of such notions. A number of examples are provided.

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1. Introduction

Almost three decades ago, Spiro Latsis (1972; also Latsis, 1976a,b) forcefully argued that the neoclassical research program in the theory of the firm was degenerating (in the sense of Lakatos, 1970), since it did not produce new corroborated predictions. Moreover, oligopoly interaction represented insoluble puzzles for that research program, because substantive rationality was not sufficient to close oligopoly models. Although he did not argue that the behavioral perspectives on the firm existing at that time constituted a serious rival research program, he did argue that the serious problems that confronted the neoclassical research program should lead towards more serious consideration of bounded rationality (henceforth, “BR”) in the theory of the firm (see also Loasby, 1971). Although not dressed in the Lakatosian methodological garb that Latsis chose for his arguments, Simon (1976, 1979) made rather similar arguments, often also concentrating on the *ad hoc* nature of “orthodox” oligopoly models. Of course, he, too, argued that work on the firm based on behavioral insights would ultimately supplant the traditional neoclassical research program.

It is somewhat ironic that at about the time that Latsis and Simon were launching their complaints, serious approaches to various aspects of the theory of the firm that all appeared to be solidly based on bounded rationality were mushrooming. Thus, team theory (Marschak and Radner, 1972), transaction cost economics (Williamson, 1971), and the evolutionary theory of the firm (Nelson and Winter, 1973) all appeared in the beginning of the nineteen-seventies (although their roots go much further back). These, still flourishing, approaches all started from bounded rationality. And the motivation for such a starting point was that neoclassical theory of the firm and its behavioral starting point in substantive rationality excluded concern with such vital phenomena as incomplete contracts, the role of organizational structure and organizational routines. Today, many — if not all — economists of organization would likely agree that BR is important to the study of economic organization (Milgrom and Roberts, 1988, 1992). Indeed, some argue that it is indispensable, that is, a necessary assumption in the theory of economic organization (Williamson, 1996; MacLeod, 2000). References to the need to draw more on psychological research are quite common now, even among the economics profession’s foremost symbol manipulators (e.g., Holmström and Tirole, 1989; Lazear, 1991). Thus, a cursory glance at contemporary organizational economics may easily convey the impression that Latsis and Simon’s lessons have been absorbed, and that organizational economists have realized the need to place BR truly centerstage at their theorizing. From a Lakatosian perspective, it may look as if BR has now attained the status of a hard core assumption.

However, closer inspection, to be undertaken in the ensuing pages, of the use and role of BR in contemporary organizational economics reveals that this is not the case, in spite of frequent invocations of BR. The notion of BR that is applied by organizational economists is, I shall argue, a decidedly “thin” one in the sense that the modeler does not stray very far from the safe path of substantive (maximizing) rationality. In fact, in contemporary treatments, BR is little more than a mainly rhetorical device that serves to lend credence to other ideas that are more central in the analysis, such as contractual incompleteness. I shall argue that this has the two unfortunate consequences that BR is neither *necessary*, nor *sufficient* in the theory of economic organization. More precisely, first, thin BR is almost indistinguishable from full rationality, and it is unclear what it adds with respect to *behavior* that could not have been

added equally well by a more sophisticated treatment of informational and computational constraints framed in the context of full rationality. Second, quite little is added with respect to understanding economic organization by including considerations of BR, as recent debate on the foundations of organizational economics suggest (section 2, “*Bounded Rationality in the Contemporary Economics of Organization: Function, Status and Debates*”). Ironically, contemporary users of the assumption BR appear to confront somewhat similar kind of problems that Latsis (1972) pointed to in the “orthodox” research program in the theory of the firm: The use of the assumption does not appear to yield extra intellectual “value-added.”

As I have suggested elsewhere (Foss 2001a), the problem is fundamentally one of adopting a minimal notion of BR that while consistent with Herbert Simon’s overall vision, at least on a rhetorical level, can *also* be made compatible with strict modeling assumptions in game theoretic mainstream economics (see also Schlicht, 1990). In contrast, a major purpose of the present paper is to sketch the contours of an organizational economics that places BR more centrally than what is presently the case, while remaining loyal to some of the overall ideas of organizational economics, notably the idea of economizing with transaction costs as key to explaining contracts and governance structures. It turns out that numerous research strategies are possible. Particular attention is devoted to a program based on the well-established, if not completely unproblematic, literature on cognitive and judgmental biases, associated with such names as Tversky, Kahneman, Thaler and others (section 3, “*Towards Richer Notions of Bounded Rationality in the Economics of Organization: Research Strategy and Examples*”). I end with some methodological reflections (section 4, “*Discussion: Transaction Costs, Efficiency, and Social Learning*”).

2. Bounded Rationality in the Contemporary Economics of Organization: Function, Status, and Debates

Bounded Rationality: Function

Economists’ thinking about the role of rationality (bounded as well as full) in the context of the theory of the firm goes back at least to at least the 1950s and likely earlier. It is hard to underestimate the importance for the development of economics and organization theory of economists being quizzical for five decades about how much sense it makes to ascribe rationality to the multi-person firm, under which circumstances this can be done, etc. Although virtually all of the initial debates concerned how much rationality could legitimately be ascribed to firms’ market behavior (Alchian, 1950; Penrose, 1952; Machlup, 1967), the focus of contemporary debate has changed from market behavior in competitive situations to issues of economic organization, notably the choice of efficient firm boundaries (e.g., Dow, 1987; Hart, 1990; Radner, 1996; Tirole, 1999).

There can be little doubt that bounded rationality is perceived of as crucially important by many contemporary writers on economic organization. Oliver Williamson is not only the flagbearer of the modern literature, but also the most outspoken proponent of the necessity to include BR in the economics of organization. “But for bounded rationality,” he argues (1996: 36), “all issues of organization collapse in favor of comprehensive contracting of either Arrow-Debreu or mechanism design kinds.” While the latter kind of analyses may contain useful insights in, for example, the dependence of allocation on the distribution of information,

there are many things they cannot do, such as explaining the existence and boundaries of alternative governance structures. For this, the notion of BR is a necessary one.

It is quite striking that a notion that is claimed to constitute a necessary behavioral assumption is never defined with much precision in the economics of organization literature. One may speculate that this has to do with the virtual absence in modern economics of a specific, affirmative, generally agreed-upon program for research in BR. Nevertheless, a multitude of differing interpretations of BR do exist, some of them extremely formal (Rubinstein 1998), so it certainly is possible to adopt some precise definition of BR as a foundational assumption in the economics of organization literature. This is, however, never done. Williamson usually confines himself to quoting Simon's famous dictum from the preface of *Administrative Behavior* that BR refers to man being "intendedly rational, but limitedly so." Some writers make hasty reference to Simonian information processing arguments (Hart 1990: 698; Schwartz 1992: 80). Thus, if agents do not have the mental capacity to think through the whole decision tree — for example, in complicated bilateral trading relations —, it seems reasonable to assume that some of the branches of the tree (such as those relating to some future uses of assets) cannot be represented in a contract; the contract is left incomplete. In a central chapter (5, "Bounded Rationality and Private Information") in their well-known textbook, Milgrom and Roberts (1992, p. 128) define bounded rationality as a matter of "[l]imited foresight, imprecise language, the costs of calculating solutions and the costs of writing down a plan." They go on to develop at length the *implications* of this in terms of imperfect contracts and subsequent problems of imperfect commitment between contractual parties. However, they, too, do not develop or truly explain their definition of bounded rationality.

The reason why economists of organization are less than eager to adopt precise, constraining definitions of what BR may mean with respect to individual behavior likely is that they simply do not see any reasons for adopting such definitions. Williamson is quite explicit here. He notes that "[e]conomizing on bounded rationality takes two forms. One concerns decision processes and the other involves governance structures. The use of heuristic problem-solving ... is a decision process response" (Williamson, 1985, p. 46). The latter "form" is not central, however, in transaction cost economics, which "... is principally concerned ... with the economizing consequences of assigning transactions to governance structures in a discriminating way."¹

Now, it is not immediately clear, first, why there is a need to make a distinction between the "two forms" of economizing on bounded rationality, and, second, what is meant by saying that one of these forms "involve governance structures." However, when seen in the context of the comparative-static approach of transaction cost economics and when the explanatory purposes of transaction cost economics are kept in mind, the possibility of making the distinction as well as associating economizing on bounded rationality with governance becomes much clearer. Thus, Williamson is interested in making use of bounded rationality for the purpose

¹ Thus, Simon seems to be justified in his critique that "... the new institutional economics has not drawn heavily from the empirical work in organizations and decision-making for its auxiliary assumptions" (Simon 1991: 27).

of explaining the existence and boundaries of firms rather than for the purposes of explaining administrative *behavior*, as in Simon (1947).² He is not interested in the issue of what “to tell Miss Jones on Monday morning” (Loasby, 1995) in response to some contingency; thus, he is not interested in BR as a “decision process response.” What Williamson says he is interested in is, of course, how BR help to explain the choice between governance structures.

However, if that is the case, one wonders why it is necessary to exclude a concern with decision process responses, since one might expect different governance structures to exhibit different decision process properties. In fact, Williamson’s main interest in BR is to do with its role in explaining incomplete *contracts* and therefore the hold-up problem around which his thinking revolves. The interest in governance structures as *ex post* mechanisms for coordination and dispute settlement is derived from this. For the purpose of explaining why contracts are incomplete, Williamson apparently thinks that it is not necessary to model BR itself; it may be asserted as a “background assumption” that while vital — indeed, necessary — does not need to be explicated itself. Milgrom and Roberts (1992, p.128), as well as most other economists of organization who invoke BR, adopt the same procedure.

Although BR is formally allowed for in these stories, agents are supposed to deal with BR in a substantively rational manner by choosing efficient governance structures or ownership arrangements on an *ex ante* basis. In this way, it is possible to have a halfway house in which BR briefly enters, leaving one essential trace, contractual incompleteness, in an otherwise largely orthodox theoretical structure, but is essentially suppressed for the remaining part of the story. Thus, at least in contract theory and transaction cost economics, it is never BR *per se* that is modeled in the sense of beginning from explicitly stated assumptions about human information processing and perceptions and then examining the implications for contracting and choice of governance structure; there isn’t any perceived need to do this.³

It is therefore not surprising that although organizational economics may have been one of the first areas where BR was systematically applied in theorizing, modern developments have not gone beyond early contributions Simon (1951), Marschak and Radner (1972) and Williamson (1975) with respect to their use of BR. Instead of examining BR *per se*, theorists have examined the multitude of implications of situations where BR is present. From the position of economizing with scholarly effort, this may be seen a considerable advantage: The theorist can still derive interesting implications for organizational design although he has been rather parsimonious with respect to characterizing the agents whose behaviors the design will influence.

² Williamson (1985) does argue that bounds on rationality may be alleviated by means of the “adaptive, sequential decision-making” characteristic of organizations, but never goes into this in any detail.

³ Team theory represents a partial exception to this; team theorists do make explicit assumptions about, notably, information-processing capabilities (see Marschak and Radner 1972; Sah and Stiglitz 1985; Camacho and Persky 1988; Carter 1995). Much of the critique in this paper of organizational economics does not apply to team theory.

Present Status: Necessary Assumption or Rhetorical Device?

Although there is thus a considerable discrete charm to the use of BR in modern organizational economics, there are also many reasons to be uncomfortable with the way it is applied and used. From the point of view of the organizational scholar, the lack of concern with decision processes means that the important possibility that bounds on rationality may be endogenous to organization is not inquired into, as Loasby (1995) noted. From the point of view of the formalist economics scholar, the use of an ultimately *ad hoc*, exogeneously given constraint on the complete contracting benchmark must ultimately stand out as less than satisfactory; he will wish contractual incompleteness to be endogenously derived in a well specified model rather than merely being postulated by means of a rhetorical appeal to BR.

Since it is the latter, rather than the former, kind of scholars who increasingly dominate organizational economics, it is perhaps not much of a surprise that the use, or at least invocation, of the notion of bounded rationality may actually have declined over the last decade and a half.⁴ Organization economics has developed into a highly formal and axiomatic enterprise, and bounded rationality has a bad reputation of only being given to formalization if that formalization is fundamentally *ad hoc* and the axiomatic basis is unclear or non-existent. That reputation may not be entirely justified (Rubinstein 1998), but most economists of organization (particularly contract theorists) certainly act as if it is justified. The prominent contract theorist Oliver Hart (1990, p. 700-1) probably sums up the sentiments of many formal economists when he argues that

... I do not think that bounded rationality is necessary for a theory of organizations. This is fortunate because developing a theory of bounded rationality in a bilateral or multilateral setting seems even more complicated than developing such a theory at the individual level; and the latter task has already proved more than enough for economists to handle.⁵

The reason that BR may not be “necessary for a theory of organizations” is that what it primarily does in the theory — namely, rationalize contractual incompleteness and therefore the inefficient investment levels that are centerstage in much contract theory (Grossman and Hart, 1986; Hart, 1995) — can be done more elegantly by asymmetric information assumptions, particularly the assumption that investments in a relation are unverifiable by a third party (e.g., a judge). Thus, contrary to Williamson (1985), BR may not appear to be a necessary assumption.

Still, however, it is not immediately apparent why substituting *ad hoc* asymmetric information assumptions for *ad hoc* BR assumptions is much of an improvement (except that the former lends itself to more easy formal treatment). Moreover, BR may well underlie asymmetric information. For example, the reason why the judge in Hart’s (1995) story is not able to

⁴ I have no hard data to support this claim, however. A piece of impressionistic evidence is that in Williamson’s work, bounded rationality loomed larger in Williamson (1975) than it does in Williamson (1996).

⁵ It should perhaps be added that Hart’s (1990) notion of what constitutes a “theory of organizations” is quite limited: It is a theory of the boundaries of the firm!

“verify” the investment levels undertaken by plaintiff and defendant *may* conceivably turn on BR: The parties and the judge may hold different cognitive categories, which is a matter of BR rather than asymmetric information, the case may present such a mass of information (not entirely unrealistic in commercial disputes) that much information because of less than perfect information processing remain unverified, etc. In fact, even formal contract theorists (including Professor Hart) continue to invoke BR. Thus, BR continues to enter organizational economics reasoning in a loose background sort of way, in which it lends credence to exogeneously imposing constraints on the feasible contracting space, but is not modeled itself. It continues to supply the rhetorical function of lending intuitive support to the notion of incomplete contracts.

The Incomplete Contract Controversy: The Irrelevance of Bounded Rationality?

A recent debate between Tirole (1999) and Maskin and Tirole (1999a,b) and Hart and Moore (1999) on the foundations of the theory of incomplete contracts and its organizational ramifications (with related contributions from Hart, 1990; Kreps, 1996; Segal 1999; Radner 1996) is extremely instructive with respect to understanding the true status of BR in the modern economics of organization. As the name indicates, the debate concerns whether satisfactory foundations for incomplete contracts are offered in the works of Oliver Hart and his colleagues and students theory (e.g., Grossman and Hart, 1986; Hart and Moore, 1990; Hart, 1995). The main critics have been Eric Maskin and Jean Tirole (Maskin and Tirole, 1999a,b; Tirole, 1999). However, the ramifications of the debate are wider, and include the role of BR in organizational economics, perhaps including transaction cost economics.

Organizational issues have largely motivated the upsurge in incomplete contract modeling during the last decade. In fact, the founding incomplete contract paper, namely Grossman and Hart (1986), was explicitly motivated by an attempt to model the Klein, Crawford and Alchian (1978) and Williamson (1979) asset specificity approaches to the scope of the firm, using modeling conventions and insights already developed in (complete contracting) agency theory (particularly Holmström, 1982). However, the outcome of that attempt was essentially a new theory. This is because Klein, Crawford and Alchian as well as Williamson have unforeseen contingencies at the heart of their stories: It is the haggling that arises when contracts are pushed outside of their self-enforcing range by unforeseen contingencies that is the main problem here. What matters is the *ex post* action. In contrast, most of the incomplete contracting approach assumes that *ex post* bargaining is efficient and that actions (e.g., with respect to investment) are taken immediately after the contract is signed. Thus, what drives these models are misaligned *ex ante* incentives, particularly with respect to investment in vertical buyer-supplier relationships.

A problem then is to motivate what may cause such misalignment. The kind of (complete) contracting studied in the mechanism design literature or its close cousin, agency theory, also won't do. Under this kind of contracting, agents can 1) perfectly foresee contingencies, 2) write contracts, and 3) enforce these. This means that parties to an "investment game" can simply write contracts contingent on the levels of investment and payoffs. The implication is that in order to dilute investment incentives, some transaction costs relating to assumptions 1) – 3) not holding true must be invoked. In other words, some *aspects* of future trades cannot be

foreseen and must be left to future negotiation, and/or writing costs mean that writing a complete contract is seldom optimal, and/or the parties' valuations are not verifiable by a court and therefore cannot be contracted over. The set of feasible contracts is constrained by transaction costs represented by assumptions 1) and/or 2) and/or 3) above not holding true.

The point of contention in the incomplete contracts controversy is whether transaction costs arising from the inability to perfectly anticipate or describe all relevant contingencies and enforce contract terms constrain the set of feasible contracts relative to the complete contracting benchmark. If this is not the case, transaction costs/considerations of BR are not sufficient to establish neither the possibility of inefficient investment patterns, nor a role for ownership (within the particular set-ups adopted in contract theory). In the eyes of the critics, the basic problem with the incomplete contract approach is that although valuations are not verifiable, they may be still be *observable* by the parties. This implies that trade may be conditioned on message games between the parties. These games are designed *ex ante* in such a way that they can effectively describe *ex post* all the trades that were not described *ex ante*. The key to this argument is the assumption (which is routinely made in the incomplete contracts literature) that parties allocate property rights and choose investments so that their expected utilities are maximized, knowing (at least probabilistically) how payoffs relate to allocations of property rights and levels of investment.⁶ Maskin and Tirole (1999a) then provide sufficient conditions under which the indescribability of contingencies does not restrict the payoffs that can be achieved. This is their "irrelevance of transaction costs" theorem. We might as well call it the "irrelevance of bounded rationality" theorem, since the problems of being unable to perfectly anticipate or describe all relevant contingencies are BR problems.

Implications

There are further rounds to the debate than what is summarized here. However, it is not necessary at this point to go into these to see that something fundamental — which may also be relevant to Williamsonian transaction cost economics — is at stake here. Maskin and Tirole (1990a) provide a rigorous demonstration of what exactly it means to choose contractual forms based on, in Williamson's terms, "farsighted contracting," and still operate with some, albeit rather limited, notion of BR. They thus counter those critics, particularly Dow (1987), who have claimed that organizational economics borders on the inconsistent because it assumes farsighted contracting *and* bounded rationality. There is no logical inconsistency involved in assuming that payoffs are fully foreseeable, yet parties are ignorant about the sources of that utility; Savage and Simon may join hands.⁷

However, another result may cause less celebration by the transaction cost economics and incomplete contracts camps. Thus, Maskin and Tirole show that although expected utility maximization and BR may be aligned, the inclusion of BR in contract economics models make essentially no difference. Their "irrelevance of transaction costs/BR" theorem show that the

⁶ In the jargon of the literature, they can perform "dynamic programming," which essentially is intertemporal optimization with discounting in a stochastic setting (see Blackwell 1965).

⁷ For methodological comments on this, see Brousseau and Fares (2000), Foss and Foss (2000), and Furubotn and Richter (1997).

set of allocations that can be obtained under complete contracting (i.e., no BR) can also be obtained under incomplete contracting (i.e., BR is present). An implication is that incomplete contract models cannot really do what their proponents assert, namely explain, for example, the boundaries of the firm (because contractual mechanisms can substitute for ownership).

One may thus be tempted to conclude that not only is BR not *necessary* in the economics of organization (since asymmetric information assumptions can also do the job); it isn't *sufficient* either. However, such a conclusion may well be too hasty. As David Kreps (1996, p. 565) laconically observes in a comment on Maskin and Tirole, "... not everything that is logically consistent is credulous". He argues that the Maskin and Tirole argument (and also the whole incomplete contracting approach) simply takes rationality too far. But there is another problem which stems from trying to combine substantive rationality with respect to some variables with rationality about other variables that is very bounded indeed. This is problematic, because in reality knowledge about the former variables (the expected utility from the relation) is likely to be dependent upon knowledge about the latter variables (the sources of the utility). Thus, Maskin and Tirole, and virtually all formal economists of organization, literally claim that people may be perfectly knowledgeable about the utility that they may expect from relation, yet basically ignorant about the sources of that utility (or, at least so ignorant as to not being able to describe it in contracts).⁸ This may make formal sense, but it is also very hard to connect to reality, and may ultimately not be a tenable assumption (see Barzel 2000 on this).

What's Next?

It is possible to draw three conflicting conclusions from the incomplete contracts controversy. First, it may be argued that we should drop BR in the economics of organization entirely, simply because it doesn't add anything (Posner 1993). Second, it is possible to argue that the incomplete contracts controversy merely suggests that existing ways of working with BR are unproductive, and that slight changes in the way that BR enters models may yield interesting results. Third, it may be argued that it should come as no surprise that introducing a version of BR that is very close to substantive rationality does lead to outcomes that are substantially different from those that result under substantive rationality. Therefore, we should examine wider notions of BR.⁹

⁸ Whether Williamson also makes this assumption is not entirely clear. He does argue that "... but for farsightedness, transaction cost economics would be denied access to one of the most important 'tricks' in the economist's bag, namely the assumption that economic actors have the ability to look ahead, discern problems and prospects, and factor these back into the organizational/contractual design." (Williamson, 1993: 129). On the other hand, he never defines "farsightedness" in a precise manner. It is not clear whether "farsightedness" in Williamson's story may be defined as rational expectations (as in contract theory). Thanks to Siegwart Lindenberg for drawing my attention to this.

⁹ Radner (1996) makes similar points. In a critique of the use of game theoretic mechanism design models in contract theory, he points to the specific, stringent, and *ad hoc* assumptions that have to be made to guarantee determinate results from contract theory models (e.g., *particular* common priors). An implication is that the usual charge of *ad hoc* theorizing against going deeper into BR is not a valid license for institutionalizing complacency about what other disciplines tell us about human behavior: Contract theory is characterized by a fair amount of *ad hoc* theorizing itself.

Contract theorists have pursued the first and the second strategy, depending on whether they consider the notion of incomplete contracts interesting. Thus, theorists have tried to rescue the notion of contractual incompleteness and its implications for economic organization through alternative routes that while continuing to invoke BR do not take fundamentally take it seriously. For example, MacLeod (2000) argues that in realistic situations, contracting costs may be exponential in the number of tasks. Thus, trying to make even simple contracts complete may lead to an explosion in contracting costs.¹⁰ Although MacLeod's argument rationalizes incomplete contracts by introducing complexity in the form of exponentially increasing costs of writing contracts, it does not say much about BR. This is also the case of the somewhat different arguments of Hart and Moore (1999) and (particularly) Segal (1999), which proceeds through introducing complexity in the environment faced by agents.¹¹ Both papers argue that incomplete contracts and inefficient investments may be explained in environments where complexity is introduced by letting the number of tradable goods approach infinity and where some aspects of the goods cannot be described. The increase in complexity means that the benefit of writing an optimal complete contract is reduced, and/or the costs of writing such a contract rises. Because trade becomes *ex ante* incontractible, inefficient investments result. These papers put all the explanatory burden on making the environment more complex than what is the convention in contract theory. Additional economic content is gained by making the situation facing an agent more complex — *not* by going into more detail about the cognitive characteristics of agents. Thus, BR is merely a handy constraint on the feasible contracting space.

Interestingly, this approach has also been challenged by Maskin and Tirole (1999b), who point out that while it may establish that trade can break down under certain circumstances because of the inability to write complete contracts, it does not establish a role for property rights (and therefore firms). In order to so, additional and somewhat *ad hoc* assumptions (e.g., that the parties are risk neutral) are necessary. In fact, they argue that "...*these* assumptions are the lynchpins of the property rights literature, rather than the unforeseeability of future contingencies." (1999b, p. 140; *emph. in original*). Considerations of (this kind of) BR *still* do not make much of a difference with respect to economic content; introducing very watered down versions of BR does not seem to lead any real intellectual gain. The remaining part of this paper considers the alternative strategy(ies) of not treating BR as merely an *ad hoc* constraint on trading possibilities, but rather starting "from man as he is" (Coase's 1984, p. 31) or more precisely, what psychological research tells us about this.

¹⁰ In the specific multi-tasking set up that Macleod adopts and assuming that it costs one cent to write a clause, with two tasks and two cost and performance levels, the contract is inexpensive (0,16 USD). However, with only five tasks and five cost and performance levels, the contract costs are 100.000 USD. And with fifteen tasks and five cost and performance levels, contracting costs are 10 million trillion USD.

¹¹ Segal (1999: 74) justifies this by arguing that "... any attempt to model bounded rationality in a simple environment is doomed to fall into the trap of describing decision makers as either 'completely dumb' or 'perfectly rational.' Neither is an attractive alternative for modeling 'transaction costs.' It is only in environments reflecting the real world's complexities that an intermediate region of 'bounded rationality' emerge." However, the real problem with contract theory may rather be that it has tried to model agents as being *both* "completely dumb" and "perfectly rational" at the same time (Foss and Foss, 2000).

3. Towards Richer Notions of Bounded Rationality in the Economics of Organization: Research Strategy and Examples

Many writers have observed that to the extent that BR enters contemporary economics, it is in rather “thin” forms (Schlicht, 1990; Akerlof, 1991; Lindenberg, 1990; Lane et al., 1996; Furubotn and Richter, 1997; Macleod, 2000; Furubotn, 1999, 2001). And although quite a number of interesting attempts to incorporate richer notions of BR in organizational economics exist (e.g., Williamson, 1998; Mookerjee, 1998; Gifford, 1999; Malcomson and Macleod, 1999; Furubotn, 2001), it is still a minority of organizational economists who take an interest in this. That is understandable.

First, “richer notions of BR” refer to taking into account the wider consequences of imperfect information processing in terms of the strategies or rules that agents may follow to cope with their imperfect computational capabilities, the cognitive frames for representing reality they construct, and the cognitive biases and errors they suffer from. However, this involves going into vast and complicated areas where economists can claim no particular competence, notably social and cognitive psychology. It means being much more explicit about the internal make-up of agents. Second, taking findings in cognitive psychology seriously seems to imply a break with a number of assumptions and modeling principles that are fundamental to at least the formal economist. For example, subjective expected utility theory is contradicted by psychological research on framing effects. Other basic notions, such as independence of payoff utilities from the payoffs of others as well as the path through the game tree, irrelevance of labeling, common prior beliefs, and backward and forward induction are problematic in the light of experimental evidence from cognitive psychology (Camerer, 1998). Third, the psychological literature identifies so many effects and mechanisms that this in itself challenges the bounded rationality of researchers.

However, those who believe that the time is ripe for more seriously considering the implications of cognitive psychology for economic organization have reason to be optimistic. The major journals are regularly featuring papers that somehow incorporate a psychological finding or examines how it challenges the orthodoxy; the Kahneman, Knetsch and Thaler (1990) paper is a striking example. Although much of this research is experimental in nature, papers that formally model findings of cognitive psychology (and do so in the context of economic organization) are increasingly common (e.g., Mookerjee, 1998; Gifford, 1999). However, research efforts are scattered and there is not much of an attempt to define a modeling strategy, perhaps much of a recognition that many strategies are possible. The following discusses a modeling strategy that while taking account of central findings in cognitive psychology, keeps the fundamentals of the economics of organization, that is, an economizing orientation, a comparative contractual perspective, and an emphasis on transaction costs as key to the explanation of economic organization.¹²

¹² Thus, I refrain from discussion the evolutionary/capabilities/etc. theory of the firm, although evolutionary considerations creep in in section 4.

A Modeling Strategy

A possible modeling strategy for how to incorporate richer notions of BR in the economics of organization has recently been sketched by Williamson (1998). He argues that the many ramifications of bounded rationality should be explored with a view to first identify those regularities in decision-making that differ from the classical model of von Neumann-Morgenstern-Savage, then work out the implications of these regularities for efficient organization, and finally fold these into the organizational design (Williamson, 1998, p. 18). The implication is that the efficiency questions of the economics of organization may usefully be reformulated, relying on thick BR models, so that "... organization can and should be regarded as an instrument for utilizing varying cognitive and behavioral propensities to best advantage" (1998, p. 12). A limitation of Williamson's (1998) paper is that he seems mostly intent on demonstrating that findings of cognitive psychology are entirely consistent with "[t]he transaction cost economics triple for describing human actors — bounded rationality, farsighted contracting, and opportunism." Therefore, he is not very specific about what exactly to do with these findings.

However, one interpretation of Williamson's program is take it as an invitation to explore mechanisms (Hedström and Swedberg, 1998), that is, causal connections that may or may not be triggered in specific situations, rather than for searching for general regularities. To be more concrete, it is a call for exploring how a *specific* manifestation of BR — such as, say, reference level biases — translate into transaction costs confronted by agents in a *specific* setting, and how this influences the contract or governance structure chosen by these agents to regulate their trade. This is not necessarily to say that one ends up with a mass of extremely partial models of strongly limited applicability. Insights of rather general applicability may follow.¹³ In particular new insights will be produced and old puzzles may be resolved.¹⁴ As a loose example of what such a program may look like, I next consider a small subset of the kind of psychological work I have in mind and then briefly explore some consequences for economic organization.

Biases to Rationality, Cognition, and Preferences

Increasingly, the massive and rapidly accumulating literature in cognitive science, cognitive and social psychology and experimental economics on biases to rationality, cognition and preferences has been not only cited by economists, but also increasingly used (see Rabin, 1998 for a survey). Strictly speaking, not all of this literature is about BR. For example, work in psychology and experimental economics on "social" (i.e., other-regarding) preferences may lie outside the orbit of BR. On the other hand, much of the literature exclusively concerns cognitive issues, such as the systematic violations of the standard theory of behavior under

¹³ For example, Babcock and Loewenstein (1998) argue that self-serving biases are likely to be a very frequent determinant of a specific type of transaction costs, namely bargaining impasse.

¹⁴ For example, in many work situations, precise signals on output are available, yet monitoring still takes place. Office workers may thus be supervised although it is trivial to count the number of forms they have processed at the end of the day. It seems unrealistic to argue that some random and unobservable factor should intervene in the work process, shifting too much risk on to the agent (Lippman and Rumelt, 1991). A more realistic explanation is lack of self-discipline in the performance of a boring job (Rabin, 1998)

uncertainty that real people engage in. This literature would be considered by most to be directly about BR. However, in actuality the boundaries between cognition and preference are blurred, and an important part of the literature lies on those blurred boundaries (e.g., March, 1999).¹⁵ In a sense, this is not surprising. If one thinks generally of BR as an imperfect ability to perceive, learn about, compare, remember, and order alternatives (cf. Witt, 1996), “strange” (e.g., time variant or context dependent) preferences are to be expected. For example, March (1996) argues that the fact that people exhibit greater risk aversion for gains than for losses in many situations (i.e., a risk *preference*) may reflect accumulated learning (i.e., a *cognitive* activity) rather than given utility functions.¹⁶ Thus, in the following, I deliberately and rather indiscriminately mix preference and cognition when discussing various manifestations of thick BR. Here is a telegraphically stated and arguably incomplete catalogue of biases that, I shall argue, are most obviously relevant to the understanding of economic organization.

The availability heuristic — that is, people tend to think that events are more probable if they can recall incidents of their occurrence. An example is that people typically think that more words, on any given page, will end with the letters “ing” than have “n” as the second-to-last letter (although clearly this is not possible). The availability heuristic has been argued to be particularly relevant to the understanding of risk assessments (Sunstein, 1999), particularly since the availability heuristic implies that risk assessments are likely to be strongly conditioned by social, particularly informational, forces.

Reference level biases — this is a family of biases that includes loss aversion (*aka* the status quo bias), the endowment effect, the diminishing sensitivity bias, and the self-serving bias. Common to these is that they all involve a reference point. The most basic one is arguably loss aversion. Under loss aversion, a loss relative to the *status quo* is seen as more undesirable than a gain relative to the same *status quo* is seen as desirable. A closely related bias is the endowment effect, that is, once a person comes to possess a good, he will value it more than before he possessed it. Kahneman, Knetsch and Thaler (1990) empirically examine the implication of the standard assumptions of economic theory that (when income effects are 0 or small) a person’s maximum willingness to pay for a good should be roughly the same as the minimum compensation demanded for the same good. They find that contrary to theoretical expectation and controlling for strategic behavior and transaction costs, there are systematic differences between these numbers, in the sense that when people are given goods their valuation of those goods increases strongly and instantaneously. The diminishing sensitivity bias implies that the marginal effects of changes in well being are greater when change is close to one’s reference level than for changes farther away (Rabin, 1998). Finally, the self-serving bias is essentially the conflation of what is fair with what benefits oneself (Babcock and Loewenstein, 1997). Thus, people systematically overestimate their own contribution to joint tasks.

¹⁵ In his overview paper, Rabin (1998) explicitly tries to distinguish relevant psychological contributions on the basis of whether they relate to “biases in judgment” (i.e., cognition) or to “preferences.” However, as he admits, the distinction is far from watertight; for example, framing effects “... may in fact partially *determine* a person’s preferences (1998, p.37; *emph. in original*).

¹⁶ The relation between “strange” preferences and cognition is further explored in March (1999), where a key point is that ambiguity in preferences can foster organizational learning.

Adaptive preferences — that is, the phenomenon that preferences, such as risk preferences (March and Shapira, 1992; March, 1996), adapt to experience in a manner that roughly corresponds to people coming to prefer what they experience. This may produce intertemporal inconsistency in revealed choices. Adaptive preferences are evidently closely related to reference level biases, and may to some extent be seen as a dynamic version of reference level biases.

Preference reversal — that is, the quite pervasive phenomenon that people are inconsistent when considering two gambles of equal expected value, one gamble having a high probability of winning a moderate stake and the other a low probability of winning a larger stake. The finding is that many persons who prefer the former over the latter when required to choose between gambles, actually put a higher minimum selling price on the latter than the former, when they are asked to evaluate the very same gambles. Preference reversal may be seen as an instance of a broader class of biases, namely framing effects (Tversky and Kahneman, 1986), which refers to the general phenomenon that people often lack stable preferences that are robust to different ways of eliciting those preferences (Rabin, 1998).

Biases in the Context of Economic Organization

As Williamson (1998) argues, biases to rationality, cognition, and preferences are mitigated to some extent by organization. This is because organization has recourse to specialization, which allows for economizing with cognitive effort. That does not make these biases irrelevant to the study of organization. On the contrary, since specialization cannot cope with all biases, recourse to additional organizational measures is likely to be necessary. In order to see this, it is necessary to inquire into how biases affect economic organization. One take on this issue is to think of biases as influencing economic outcomes because they influence bargaining games (Kahneman, Knetsch and Thaler, 1990; Babcock and Loewenstein, 1997). They may be viewed as determinants of bargaining outcomes on par with asymmetric information, strategic behavior and time preference.

To be slightly more concrete, what people believe that they deserve in a bargaining situation may be subject to reference level biases. In turn, reference levels may change over time as a result of the phenomenon of adaptive preference (e.g., in a repeated bargaining situation). The comparisons people make when evaluating their gains and losses from bargaining, and how they evaluate the same offers in different contexts (e.g., as made by different people), may be subject to framing effects. In the context of economic organization, biases may thus influence how much employees expect to capture of the firm's surplus (and therefore how much they are going to invest in augmenting their human capital), how competitive threats are perceived, how the gains from trade in inputs markets will be shared, etc. In the context of economic organization, biases may influence both explicit contract terms and the bargaining games that take place in the context of relations where contracting is incomplete (i.e., *ex post* renegotiation games). The ramifications are clearly many and complicated; only a few will be considered here.

Example 1: Loss aversion, employee expectations and strategic change. In order to spell out some implications of loss aversion, imagine a dramatic change in corporate strategy so that the focal firm withdraws from a number of markets, downsizing and concentrating on core business. Of course, many employees in addition to those that may be laid off will suffer a loss of utility as a result of this. Since the change is likely to be at least partly negotiated between the various stakeholders of the firm, management and owners are likely to offer various side-payments to reduce these losses of utility. Strategic behavior is likely to complicate the ensuing bargaining. However, the phenomena of loss aversion and adaptive preferences are likely to further complicate bargaining games.

First, loss aversion implies that the proposed strategic change will involve a mixture of painful losses and less-pleasurable gains so that people will tend to resist change. Inertia is predicted by loss-aversion alone. Second, in an employee relationship, employees develop implicit and explicit expectations to the contract governing the relationship, and particularly to the benefits that they believe they deserve under the implicit contract, that is, their “entitlements” (Heath et al., 1993). There is psychological evidence that people tend to be systematically biased in their estimates of their entitlements. More specifically, these are perceived as richer (people think they contribute more than they do) and more systematic (because rare events are often given probability zero, the consistency of others’ behavior is over-estimated) than they would be to a neutral observer (idem.). The combined implication of loss aversion and the development of expectations with respect to entitlements is that side-payments are likely to be much larger than an “objective” evaluation would suggest.

Various implications follow. First, the economics of organization implicitly claims that organizations are plastic. Notions of complementarity between elements of the organization (Holmström and Milgrom, 1994) and strategic behavior may complicate this, but provided that organizations can be changed in a systemic fashion and informational asymmetries do not pose too much of a problem, there should not be any remaining problems of organizational change. However, the phenomena of loss aversion and adaptive preferences are relevant mechanisms that may cause substantial organizational inertia and make organizational change more costly than an organizational economics analysis would indicate. Thus, although alternatives to existing organizational arrangements can be imagined, the set of alternatives that can be implemented with net gains is further circumscribed. The other side of the coin of adaptive preferences and loss aversion suggest is that an important part of *ex post* governance is the management of the formation of the expectations of those agents with which the firm bargains over inputs and outputs. The ultimate sharing of value will not just be a matter of the “objective” contribution of each agent, but will also reflect players’ perception of their “legitimate” entitlements. Management and leadership have an important role in influencing those perceptions.

Example 2: Leadership behavior and the availability heuristic. The previous example indicates that an implication of the combined effects of loss aversion and adaptive preferences is to make any *status quo* salient. However, the availability heuristic may counteract that tendency. The fact that the availability heuristic is likely to be very strongly socially conditioned only helps here. For example, public announcements by a CEO that the competitive situation faced by the firm is threatening may create informational externalities,

because the announcement is taken as a relevant signal by employees. When there is little information about the true state of competition, such externalities may create informational cascades (Sunstein, 1999). If further this announcement is combined with a call for wage reductions, there is potentially ample room for the kind of employer opportunism discussed by Dow (1987) but neglected in most of the economics of organization. Thus, one possible application of the availability heuristic is to broaden the role of opportunism in organizational economics.

Example 3: Context-dependent risk-preference. An implication of preference reversal and adaptive preferences is that risk-preference is likely to be context-dependent. Specifically, March and Shapira (1992) argue that risk-taking is influenced by danger (threats to survival), slack (more slack leads to more risk-taking), aspiration levels (people are risk-seeking under the target level and risk-averse above), whose resources are at risk, and past experience. This suggests that the efficiency of incentive contracts, which partly relies on shifting risks between parties, is context dependent, and that some kinds of incentive contracts may in some contexts have perverse consequences. For example, consider a firm that not only falls much below its own aspiration levels, but also begins to confront difficulties with sales, and ultimately of paying creditors. In this situation, managers may, because of the context-dependence of risk preference, want to assume more risk than would be sensible to a neutral observer. If they have been equipped with incentive contracts in the form of golden parachutes, their incentives to actually assume excess risks will be strengthened (Shapira, 2000). Incentive contracts that have not been designed with an eye to the context-dependence of risk-preference may therefore misstate the “true” risk-allocation inherent in the contract, given the various states of nature that may be realized.

Example 4: Groups and Organization Design. The use of group-based decision-making is an increasingly important aspect of modern organization. Arguably, the use of groups is partly determined by the phenomenon which social psychologists have pointed to numerous times (e.g., Cooke and Kernaghan, 1987), namely that group decisions differ from individual decisions in an organizational context and that groups can change the behavior of their members. The positive aspect of this is that groups may give rise to distinct synergies, stemming from improved problem-solving. The improved problem-solving that is available to teams/groups has been strongly emphasized in the Total Quality Management movement (Jensen and Wruck, 1994). Thus, adherents of TQM advocate extensive delegation of decision rights to groups to promote a closer co-location of decision rights and specific, local knowledge *and* a faster and more efficient decision-making process.

Although group interaction may thus yield certain types of informational rents, a rich literature in social psychology indicates that groups have biases of their own. The costs associated with such biases may offset potential benefits. For example, an apparently rather robust phenomenon is the “risk-shift phenomenon” in which group compromise leads to the adoption of a most risky common view (Mullins, 1996). This may well be related to what Janis (1972) famously described as “groupthink,” that is, the tendency of the sharing of mental models to lead to incomplete surveys of alternatives and objectives and failures to fully examine the risks of available alternatives, in turn leading to excessive optimism and risk-taking and a suppression of “heretic” ideas.

Organizational economists have known at least since Alchian and Demsetz (1972) that groups/teams may fall victim to free-rider problems, and that this may help explaining aspects of the organization of firms. As the above indicates, free-rider problems are not the only problems of the “disaggregation of corporations” (Zenger and Hesterly, 1997) in smaller teams, an increasingly widespread practice (*idem*). Among the relevant costs of increased disaggregation are the potential costs of promoting groupthink-phenomena within groups/teams (although this may mean that corporation-wide groupthink may be reduced).

Many other similar examples could be constructed based on the many cognitive biases that have not been reported here.¹⁷ Moreover, it would also be possible to examine implications for other aspects of economic organization than internal organization. However, the examples hopefully to indicate the main thrust of the argument: It is possible to tell stories about economic organization that pay more attention to work in cognitive psychology insights, but still does not fundamentally break with explanatory fundamentals of organizational economics. Specifically, the above examples indicate that this kind of research may enrichen the understanding of the sources of organizational inertia and the barriers to organizational change, the analysis of opportunism, the design of incentive contracts, and organizational design. In a wider sense, a thick BR approach means that more determinants of transaction costs, perhaps particularly those that relate more to bounded rationality than to opportunism, are identified.

4. Discussion: Transaction Costs, Efficiency and Social Learning

Where does all this leave us with respect to the presumed efficiency properties of alternative governance structures?¹⁸ One response may be to argue that it merely adds extra dimensions to the comparative contracting approach, refining the understanding of issues such as the remediability criterion (Williamson, 1996, chapter 9), managerial opportunism, and the sources of transaction costs and contractual incompleteness. However, the basic theoretical structure is not affected. That conclusion is, however, too facile. For example, farsighted contracting — at least in the strict sense of involving rational expectations and subjective utility maximization— will not survive confrontation with findings from cognitive psychology, reviving Dow’s (1987) argument that in the presence of BR, agents cannot be expected to choose efficient contracts and governance structures on an *ex ante* basis. The inclusion of behavioral variables that are likely to be endogenous to incentives and managerial actions clearly adds more complexity to the (dynamic) optimization problem, making it even

¹⁷ Examples may include the effect of frequent performance evaluation on the framing of decision situations and the effect of framing on risk preferences. See Payne (1997), Camerer (1998) and Shapira (2000) for inspiring discussions of the implications of cognitive psychology for organizational decision making.

¹⁸ For a particularly thoughtful discussion of the difficulties of giving meaning to efficiency when BR is taken seriously, see Furubotn (1999).

more unlikely that boundedly rational people can choose efficient structures.¹⁹ Moreover, an implication of making room for richer conceptions of BR is (as we have seen) that the assumption of organizational plasticity becomes untenable, making it even more implausible that efficient governance structures can be implemented by boundedly rational designers. On the other hand, this provides the basic condition, namely rigidity of behaviors, for processes of natural selection to operate. However, natural selection explanations neither say much about where the varieties of economic organization come from, nor about how decisions actually made on the level of the firm influence the process of selection.

While the efficiency-from-design argument is implausible and natural selection explanations are typically uninformative about managerial choice, there is another way in which the emphasis on economizing and efficiency perspective characteristic of organizational economics may be defended, even when rich models of BR are taken into consideration, namely social learning models. Arguably, social learning explanations appeal less to economists than intentional or natural selection explanations for the same reason that economists are traditionally reluctant to draw on thick BR models of behavior: The disciplinary distance seems too large (but see Witt, 1996). However, there are reasons to revisit social learning explanations, because they provide mechanisms explaining how hitherto successful (not necessarily efficient in any strict sense) governance structures or contract designs may diffuse (Peteraf and Shanley, 1997).

In the perhaps most cited contribution to field, Bandura (1977) went beyond simple stimulus-response or imitation schemes of individual learning and argued that while agents try to emulate the successful actions and avoid the failures of other agents, they do so through sophisticated processes of interpretation and reflection that lead to the construction of models of behavior that are useful for interpreting concrete situations and acting in these. A particular important factor in relational modeling in social interaction is represented by symbols of all kinds (including shared typifications), which represent not only focal points (in the game theoretic sense), but also repositories of values and beliefs. Symbolic processes thus allow agents to develop models of behavior that are more generally applicable than the models developed from direct learning from observation. For example, relational modeling is facilitated by the existence of symbols because symbols provide already produced categories into which other agents or experiences may be placed.

There are many reasons why the basic point in social learning theories — namely, that individual learning and choice behavior is very strongly conditioned by processes of social comparison — is particularly applicable to an understanding of processes of design of certain governance structures or contract designs. A basic premise is that governance structures and contract designs are chosen by managers, (possibly in some sort of cooperative relation with owners/boards of directors. However, the activities of managers are very much relational and representational (Peteraf and Shanley, 1997), Representing the interests of others (owners, the

¹⁹ Contrary to what Moore (1992) thinks, the inability to perform dynamic programming does not necessarily mean that the analysis breaks down. For example, agents may still be able to choose governance structures or contracts based on their perception that the relevant payoffs exceed some threshold level (as in Simon 1955).

business unit, etc.), typically to others who are also engaged in representational activities, requires great skill in the modeling of those whose interests are represented and of those to whom the representation is directed. The team-based nature and the strong emphasis on mutual consent in managerial work represent further reason for it being inherently representational and relational. Finally, because of considerations of BR, management relies on low cost ways of learning, particularly observing and interpreting the actions of other firms, and relying on intermediaries, such as consulting firms, consulting academics, trade associations, etc. who are essentially brokers in symbolized experiences. Such learning takes place with respect to products, production methods, organization, ways of dealing with public authorities, etc. A highly relevant aspect of learning about organization is that there are no legal barriers to imitative action (in contrast to the case of products); one cannot protect a specific governance structure, internal organization or contract design by means of intellectual property rights.

While it is conceivable that boundedly rational managers may indeed discover successful/profitable governance structures through experimental processes, these are likely to be costly and lengthy, and arguably only successful under stationary or near-stationary conditions (Dow, 1987). Observing and interpreting the outcomes of other firms' experiments with organizational forms are likely to be comparatively low cost ways of gaining access to experience. Thus, the diffusion of governance structures, etc. may take place through such processes of social comparison. This provides some assurance for claiming that types of economic organization that "work" (e.g., seem to lead to increases in profits relative to observed alternatives) will actually be adopted.²⁰

5. Conclusion

The economics of organization has explored only a tiny fraction of the many ramifications of BR. However, the present use of BR is less than satisfactory. BR seems to be neither necessary, nor sufficient in the economics of organization, where its main role increasingly seems to be purely rhetorical. While it is easy to criticize the treatment of BR in the economics of organization, concrete remedies are harder to devise. I have suggested that certain portions of the cognitive psychology literature may be promising. In particular, attention has been drawn to the cognitive biases literature. Many of these biases are well established in experimental research and have rather direct implications for organization and contract design. More specifically, they allow for an empirically grounded understanding of a neglected set of contractual hazards and frictions, and therefore a richer understanding of the determinants of transaction costs. By means of exemplification, I have argued that such an approach has real implications for economic organization, more specifically internal organization. Because of space limitations, implications for issues relating to the boundaries

²⁰ However, as Dow (1987) rightly insisted there can be no presumption that processes of social learning lead to efficient outcomes with any degree of automaticity. First, the symbolic nature of social learning means that certain types of economic organization may be more salient than others. The availability heuristic may reinforce this. The implications are that efficient alternatives may not be imitated, and, hence, not diffuse, and that salient types of economic organization may be applied to transactions for which they are basically not suited. Second, as Dow argued path-dependency may arise, specifically informational cascades may lead to widespread adoption of inefficient alternatives, even in the presence of intelligent learning.

and existence of firms, and wider issues of contract design, have not been developed. Future work will address these issues.

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Danish Research Unit for Industrial Dynamics

The Research Programme

The DRUID-research programme is organised in 3 different research themes:

- *The firm as a learning organisation*
- *Competence building and inter-firm dynamics*
- *The learning economy and the competitiveness of systems of innovation*

In each of the three areas there is one strategic theoretical and one central empirical and policy oriented orientation.

Theme A: The firm as a learning organisation

The theoretical perspective confronts and combines the resource-based view (Penrose, 1959) with recent approaches where the focus is on learning and the dynamic capabilities of the firm (Dosi, Teece and Winter, 1992). The aim of this theoretical work is to develop an analytical understanding of the firm as a learning organisation.

The empirical and policy issues relate to the nexus technology, productivity, organisational change and human resources. More insight in the dynamic interplay between these factors at the level of the firm is crucial to understand international differences in performance at the macro level in terms of economic growth and employment.

Theme B: Competence building and inter-firm dynamics

The theoretical perspective relates to the dynamics of the inter-firm division of labour and the formation of network relationships between firms. An attempt will be made to develop evolutionary models with Schumpeterian innovations as the motor driving a Marshallian evolution of the division of labour.

The empirical and policy issues relate the formation of knowledge-intensive regional and sectoral networks of firms to competitiveness and structural change. Data on the structure of production will be combined with indicators of knowledge and learning. IO-matrixes which include flows of knowledge and new technologies will be developed and supplemented by data from case-studies and questionnaires.

Theme C: The learning economy and the competitiveness of systems of innovation.

The third theme aims at a stronger conceptual and theoretical base for new concepts such as 'systems of innovation' and 'the learning economy' and to link these concepts to the ecological dimension. The focus is on the interaction between institutional and technical change in a specified geographical space. An attempt will be made to synthesise theories of economic development emphasising the role of science based-sectors with those emphasising learning-by-producing and the growing knowledge-intensity of all economic activities.

The main empirical and policy issues are related to changes in the local dimensions of innovation and learning. What remains of the relative autonomy of national systems of innovation? Is there a tendency towards convergence or divergence in the specialisation in trade, production, innovation and in the knowledge base itself when we compare regions and nations?

The Ph.D.-programme

There are at present more than 10 Ph.D.-students working in close connection to the DRUID research programme. DRUID organises regularly specific Ph.D-activities such as workshops, seminars and courses, often in a co-operation with other Danish or international institutes. Also important is the role of DRUID as an environment which stimulates the Ph.D.-students to become creative and effective. This involves several elements:

- access to the international network in the form of visiting fellows and visits at the sister institutions
- participation in research projects
- access to supervision of theses
- access to databases

Each year DRUID welcomes a limited number of foreign Ph.D.-students who want to work on subjects and projects close to the core of the DRUID-research programme.

External projects

DRUID-members are involved in projects with external support. One major project which covers several of the elements of the research programme is DISKO; a comparative analysis of the Danish Innovation System; and there are several projects involving international co-operation within EU's 4th Framework Programme. DRUID is open to host other projects as far as they fall within its research profile. Special attention is given to the communication of research results from such projects to a wide set of social actors and policy makers.

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