

# The Rhetorical Dimensions of Bounded Rationality: Herbert A. Simon and Organizational Economics

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## **Abstract**

I discuss the rhetorical dimensions of bounded rationality in two different, yet related, contexts, namely in the practice of organizational economists and in Herbert Simon's key attempts to persuade economists to take bounded rationality seriously, his Ely lecture and his Nobel Prize lecture. I discuss various reasons why Simon failed to convince his contemporaries, among other things, the absence of clear definitions of bounded rationality and heuristics for incorporating it into economic models. Simon's failures in these respects help explaining the very modest, and mainly "rhetorical," use of bounded rationality in the works of organizational economists.

**Keywords:** Herbert Simon, bounded rationality, organizational economics, rhetoric.

**JEL classification:** B41, D23, L2, M1

# I. Introduction<sup>1</sup>

In this chapter I make a couple of connected arguments about the status of bounded rationality (henceforth, “BR”) in modern economics and the role of Herbert Simon with respect to understanding this status. Following McCloskey (1983), emphasis is placed on the rhetorical aspects, the attempts to persuade, of scientific development in economics. Following Mirjam-Sent’s (1997) fine study of Thomas Sargent’s (rhetorical) appeal to BR, economists’ actual use of BR is examined. The economists whose use of BR I consider are economists of organization. This choice is far from arbitrary: In his key papers directed at an economics audience (Simon 1978, 1979), Simon made several explicit references to the emerging economics of organization and he himself contributed fundamentally to the neighboring field of organization theory. His examples of BR and its implications usually involved the business firm. Indeed, he sometimes took the notion of “administrative man” to be *synonymous* with a boundedly rational agent. Perhaps not surprisingly, then, the economics of organization was probably the first sub-field of economics where BR was systematically invoked, and it is perhaps still today the sub-field it is still being invoked with the highest frequency.<sup>2</sup>

More specifically, it will, first, be argued that the use of BR arguments in the economics of organization is primarily rhetorical in the somewhat pre-McCloskeyan (McCloskey 1983) sense of a dressing up a theory with arguments that are essentially empty in an explanatory sense, but are nevertheless made because they help to persuade. Specifically, BR is invoked in the rhetorical practice of organizational economists, because it represents a way of conveying the intuition of another, much more central point; however, it is not invoked because it itself is in any way central. Specifically, BR is used in order to explain in a loose, background way the notion of contractual incompleteness. Thus, if people do not have the wits to imagine and

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<sup>1</sup> The comments of Mie Augier are gratefully acknowledged.

<sup>2</sup> Game theorists may refer to BR more frequently, but game theory is hardly a distinct sub-field in economics; rather, a workshop that supplies machine tools to the various fields.

make contractual provision for a number of contingencies, they will leave the contract incomplete, giving rise to the kind of externality problems that drive the modern economics of organization. However, asymmetric information (a well-defined concept) can do, in these models, what BR (a concept with a multitude of different connotations, but arguably no clear definition) supposedly does, and can do so more “cleanly” (Section II, “*Economists’ Use of Bounded Rationality: the Case of Organizational Economics*”). This raises the question why BR continues to be invoked, a question that leads to the second argument.

Second, I argue, admittedly in a more speculative vein, that there are strong reasons to suspect that the status as well as the rhetorical practice of Herbert Simon play a decisive role for understanding the way in which BR is presently used. The fact that a Nobel Prize was bestowed upon Simon for his work on BR, and that it was the brainchild of one of history’s more impressive polymaths, should make it hard for almost anyone to simply dismiss it.<sup>3</sup> Arguably, however, the rhetorical practice of Simon made it hard for most economists to *not* in the end dismiss it, if perhaps only discreetly, namely by tacitly refusing to incorporate it in their theoretical work. Here, rhetorics is understood in a more authentic manner as the “... art of discovering good reasons, finding what really warrants assent, because any reasonable person ought to be persuaded” (Booth 1974: xiv; quoted by McCloskey 1983: 482). Thus, although Simon certainly wrote as if “any reasonable person ought to be persuaded” by his arguments, he did not, I argue, give the “good reasons,” those that would “really warrant assent,” for BR. He failed to persuade. Most importantly, in his most “rhetorical” papers, directly aimed at persuading economists, Simon never provided any precise definition of what BR really *is*. Moreover, he never really gave good heuristic advice on to incorporate BR into economic models, and simply noted that how exactly notions of BR developed in the

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<sup>3</sup> Giovanni Dosi (2002) reports that a few years ago, he (and a couple of co-authors) had a paper on industrial dynamics rejected from *Econometrica*, based on a review report, the main conclusion of which was that the paper would bring “... back the discussion of industrial change to the Dark Ages of Herbert Simon.” Such bluntness is, however, rare.

theory of computational complexity — his favorite example of successful modeling of BR — would be incorporated into economic models “remain[s] to be seen” (1978: 12). These failures form part of the explanation of why most economists, including economists of organization, so far have not fundamentally taken BR seriously (Section III, “*Simon Lecturing Economists on Rationality*”).

Some implications and wider implications, notably with respect to the future of BR in the economics of organization, are finally briefly discussed. A conclusion is that there is reason to be more optimistic on behalf of the use of BR in economic reasoning, since there are now better critical and constructive foundations for BR than when Simon tried to make economists take BR seriously, as partly signaled by the publication of the present book (Section IV, “*Discussion*”).

## II. Economists’ Use of Bounded Rationality: The Case of Organizational Economics

### **Bounded Rationality in Organizational Economics**

In his autobiography, Simon (1991a: 270-271) recounts increasingly violent disagreements with mainstream economists, leading him to abandon, for a period, economics in favor of psychology and computation science. “By the time I returned to a concern with economics in the 1970s,” he observed, “the war was open and declared.” Indeed, many of Simon’s writings from that period (Simon 1976, 1978, 1979) are so sharply formulated that it seems quite likely that at least Simon himself felt that he was part of a war.<sup>4</sup> (It is actually harder to find specific and concerted critique, at least in print, of Simon and BR, such as one would expect of a genuine war).

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<sup>4</sup> Simon also published in the 1970s on issues that are not directly related to BR, for example, Ijiri and Simon (1974) and Seskind and Simon (1973).

However, at exactly the time when the supposed war was going on, an important part of economics, namely the theory of the firm, seemed to be increasingly influenced by considerations of BR. New, 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 further back). These, still flourishing, approaches all seemed to start from bounded rationality,<sup>5</sup> exactly as Simon would like them to. And the explicit 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; McLeod 2000). References to the need to draw more on psychological research for understanding the workings of organization are quite common now, even among the economics profession's foremost symbol manipulators (e.g., Holmström and Tirole 1989; Lazear 1991).

Thus, a newcomer to the field may, by glancing at contemporary organizational economics, easily get the impression that Simon's lessons have been absorbed, and that organizational economists have acknowledged the need to place BR centerstage in their theorizing. This is not the case. If anything, the use (or invocation) of BR may have declined.<sup>6</sup> To some extent this is because the mainstream economics of

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<sup>5</sup> Albeit rather different conceptions of BR. Thus, BR in team theory is in actuality maximizing with costly communication, BR in transaction cost theory is the factor that explains why contracts are incomplete, and BR in evolutionary theory is a matter of search.

<sup>6</sup> An impressionistic example: In Williamson's work, bounded rationality looms larger in *Markets and Hierarchies* (1975) than in *The Mechanisms of Governance* (1996).

organization has developed into a highly formal and axiomatic enterprise, and BR 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 many economists of organization (particularly contract theorists) certainly act as if it is. Oliver Hart (1990: 700-1) arguably sums up the attitudes 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.

In fact, some parts of the economics of organization, particularly contract theory, bear little substantial imprint of BR.<sup>7</sup> This is not surprising: Contract theory is based entirely on information economics and game theory, which at least in their standard, “toolbox versions” have no room for BR at all. Still, even contract theorists occasionally invoke BR, usually to explain in a loose way why some contingencies may be left out of a contract.

### **Bounded Rationality and Transaction Cost Economics**

It is sometimes argued that transaction cost economics provides considerably more room for BR than contract theory (e.g., Brousseau and Fares 2001). There is something to this claim; for example, Williamson (1975) does invoke BR in connection with, for example, explaining the M-form, and other aspects of organizational structure. He puts much emphasis on the need for adaptation that arises in a world of uncertainty and bounded rationality. Governance mechanisms in

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<sup>7</sup> For example, rational expectations are central in most contract theory models. Applying the notion (any notion, really) of BR to expectations formation would imply that much contracting would produce unintended consequences, producing a need for *ex post* governance; however, this is not explored in contract theory.

Williamson's work are more than efficient (i.e., second-best) *ex ante* allocations of property rights (as in Grossman and Hart 1986; Hart and Moore 1990); they are mechanisms for *ex post* adaptation and conflict resolution. Williamson's works are replete with references to Simon.

Still, however, Williamson refrains from being explicit about how to model BR on the level of the individual agent. He is quite explicit here, noting 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: 46). However, in transaction cost economics, "heuristic problem solving" is not central. Instead, transaction cost economics " ... is principally concerned ... with the economizing consequences of assigning transactions to governance structures in a discriminating way."<sup>8</sup> In other words, Williamson is interested in making use of bounded rationality for the purpose of developing a theory of discriminating alignment rather than for the purposes of explaining administrative *behavior*, as in Simon (1947). He is not interested in BR as a "decision process response." 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: 128), as well as most other mainstream economists of organization who invoke BR, adopt the same procedure.

Thus, BR enters organizational economics reasoning in a loose background sort of way, in which it lends credence to exogenously imposing constraints on the feasible contracting space, but is not modeled itself. It supplies the rhetorical function of lending intuitive support to the notion of incomplete contracts. A Simonian information processing argument is sometimes invoked in order to be more concrete about how BR produces incomplete contracting (Hart 1990: 698;

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<sup>8</sup> Thus, Simon may 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 1991b: 27). See also Simon (1997).



Schwartz 1992: 80): 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. This is indeed a BR argument. However, agents are supposed to deal with this manifestation of BR in a substantively rational manner, as numerous critics have pointed out since Dow (1987). As he observed, this approach provokes a lurking suspicion of a basic inconsistency, for whereas BR is loosely invoked as a background assumption (yet still a necessary one), there is no hesitation to appeal to substantive rationality when the choice between governance structures must be explained. I discuss this next.

### **The Irrelevance of Bounded Rationality: The Incomplete Contract Controversy**

A recent theoretical debate on the coherence and foundations of incomplete contract theory — called the “incomplete contract controversy” (Tirole 1999) — is pertinent to the issues under consideration here. The debate concerns whether satisfactory foundations for incomplete contracts are offered in the works of Hart and associates (e.g., Grossman and Hart 1986; Hart and Moore 1990). The main critics are Eric Maskin and Jean Tirole (Maskin and Tirole 1999; Tirole 1999). At the core of this debate is the explanatory tension between invoking transaction costs — which may be understood as a consequence of BR — on the one hand and postulating farsighted and substantively rational contracting on the other hand (i.e., the parties to a contract can foresee the utilities from the relation). Whereas Dow (1987) interpreted this as an inconsistency in transaction cost economics, Maskin and Tirole show that there is no formal inconsistency here, and that on this point the incomplete contracts literature has got it right. However, the main thrust of their argument is that the use of transaction costs (i.e., BR) in models of incomplete contracting does not provide additional explanatory insight relative to models that make no use of these (i.e., complete contracting models).



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 emphasis in transaction cost economics on asset specificity as a key determinant of the scope of the firm, using modeling conventions and insights already developed in (complete contracting) agency theory and its basis in mechanism design theory. However, whereas Williamson (1996) puts much emphasis on inefficient *ex post* bargaining, the incomplete contracting approach assumes that *ex post* bargaining is efficient. Thus, what drives these models are misaligned *ex ante* incentives, particularly with respect to investment in vertical buyer-supplier relationships. The problem is to motivate what may cause such misalignment. 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 or enforce contract terms — all of which may derive from BR<sup>9</sup> — constrain the set of feasible contracting outcomes relative to the complete contracting benchmark. If this is *not* the case, transaction costs (BR) do not suffice to establish the possibility of inefficient investment patterns. Therefore, they do not suffice to establish a role for ownership, and in turn for a theory of the boundaries of the firm.<sup>10</sup>

The Maskin and Tirole argument builds on the key assumption in the incomplete contract approach that although valuations may not be verifiable, they may still be observable by the parties (Hart and Moore 1990). This implies that trade can 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* (where bargaining is efficient) all the trades that were not described *ex ante*. A further crucial step in the argument is the typical contract theory assumption that parties allocate property rights and choose investments so that their expected utilities are

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<sup>9</sup> In the last case (non-verifiability), it is the enforcing party, such as a judge, that is boundedly rational.

<sup>10</sup> That is, within the particular set-ups adopted in contract theory.

maximized, knowing (at least probabilistically) how payoffs relate to allocations of property rights and levels of investment (i.e., they can perform “dynamic programming”). Given this, Maskin and Tirole (1999) provide sufficient conditions under which the undescribability of contingencies does *not* restrict the payoffs that can be achieved. In other words, there are no differences in the allocations that can be achieved under incomplete contracting and complete contracting; no real economic content is achieved by adding considerations of BR/undescribability of contingencies/transaction costs. This is their “irrelevance of transaction costs” theorem. We might as well call it the “irrelevance of bounded rationality theorem.”

### **Bounded Rationality and Mainstream Modeling**

BR, it has been argued, is very much a background assumption that is introduced in order to help explaining in an “intuitive” way incomplete contracting, a key ingredient in understanding the efficient boundaries of the firm, and efficient alignment more generally. BR is never explicitly modeled on the level of the individual agent. It is never fundamentally taken seriously in the way economists take arguments serious, namely by modeling them. After Maskin and Tirole (1999), there would indeed seem to be little reason to take BR seriously at all; its use can at best be “rhetorical.”<sup>11</sup>

However, one may argue that it is exactly the very “thin” way in which BR is treated in organizational economics that allows for the Maskin and Tirole argument, which purportedly demonstrates the complete irrelevance of BR (Kreps 1996; Foss 2001). After all, a main notion in incomplete contract theory and transaction cost economics that a very thin Simon may join hands with a rather corpulent Savage, as it were, and it is not really surprising if the very thin Simon turns out to matter very little indeed for the explanatory weight of the whole construct.

Still, the puzzle remains why organizational economists have chosen to work with models in which BR occupies at best a small corner — of a mainly rhetorical

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<sup>11</sup> In contract theory courses at American universities, BR is often introduced (and finished) in the following manner: “We may use BR as a shorthand for anything that makes a contract incomplete.”

nature — of the model, the rest of the space being taken up by common priors, dynamic programming, etc. Not only was Simon critical of, for example, dynamic programming (Simon 1978), his work was taken up with rather concrete manifestations of BR often explored in detailed computational models (e.g., Newell and Simon 1972). Simon himself published prolifically on firms and other organizations (e.g., Simon 1949, 1951, 1991, 1997; March and Simon 1958). Given all this, how can it be that organizational economists have been reluctant to be serious about BR, in the sense of actually modeling BR?

The presumably most obvious reason is because of the well-known difficulties of aligning BR with the basic machinery of neoclassical microeconomics and game theory (Conlisk 1996; Rabin 1998; Camerer 1998b). Thus, fundamental notions and modeling principles, such as subjective expected utility, common priors, rational expectations/dynamic programming, backward induction, etc., are not too easily aligned with fundamental findings of cognitive psychology (such as gain-loss asymmetries, role-biased expectations, etc.). Moreover, from the mainstream economist point of view, there is a huge price to be paid in terms of analytical tractability and clarity to the extent that one wishes to factor findings from cognitive psychology into economic models. This would seem to be consistent with BR becoming increasingly squeezed out of the economics of organization, as the latter has become increasingly formalist, the Maskin and Tirole argument being the culmination of the squeeze-out operation. However, this may not be the entire story. In the following, I argue that Simon himself is partly responsible for the way in which economists have used (rather, not used) BR.

### III. Simon Lecturing Economists on Rationality

In a series of papers, Simon (1976, 1978, 1979) made a sustained attempt to convince economists to take BR seriously. Two of these are particularly noteworthy in the present context, namely his Richard T. Ely lecture, “Rationality as Process and as a

Product of Thought" (Simon 1978) and the Nobel lecture, "Rational Decision Making in Business Organizations" (Simon 1979). Needless to say, an Ely lecture, held at the annual meeting of the American Economic Association, and a Nobel even more, represent excellent platforms for persuasion exercises. It is therefore worth looking a bit into these papers.

### **The Ely and the Nobel Lectures**

The lectures differ primarily in the dimensions of depth and broadness, the Nobel being more survey-oriented and less analytically engaging. The similarities are, however, much more striking than the differences. First, they cover much the same themes. Second, both lectures are very strongly rhetorical in a number of senses. Thus, they employ a host of familiar rhetorical devices, such as metaphorical reasoning (particularly the Nobel), analogies, reference to authority, and, yes, quotations from *Alice in Wonderland*. Moreover, the lectures are rhetorical in the sense that they are very much taken up with "... probing what men believe they ought to believe, rather than prove what is true according to abstract methods" (Booth 1974: xiii; cited in McCloskey 1983: 482). An aspect of this "probing" is that the lectures are highly polemical, with much strongcritique of economists such as Fritz Machlup, Milton Friedman, Edward Mason and other defenders of the neoclassical theory of the firm. Also, there is indeed no attempt to "prove what is true according to abstract methods"; quite the contrary, "abstract methods" (axiomatic, non-empirical, etc.) methods are criticized and a rather sturdy elementary empiricism is promoted, one that involves "straightforward 'anthropological' field study" (1978: ). General equilibrium theory is condemned as mere intellectual puzzle-solving, it being noted that "[p]erhaps some of these intellectual mountains have been climbed simply because hey were there" (p.493-4).

All these aspects are signaled at the beginning of both lectures. Thus, they begin with the traditional opening gambit for talks of this kind, namely invoking authorities. The relevant authorities are Richard Ely and Alfred Marshall. Citing and quoting them is motivated by their taking a different, and more "realistic," view

of economics from the abstract one associated with Lionel Robbins of how to best allocate scarce means among competing ends, an understanding that is, of course, closely related to the notion of maximizing. The richer and more realistic view of behavior in Marshall and Ely leads directly into the rationality theme.

Particularly in the Ely Lecture, Simon handles this in a strikingly rhetorical manner. He begins by arguing that indeed most of human behavior has a rational element; however, maximizing rationality may not adequately describe this rational element. He then shows that economists in fact do use weaker forms of rationality, particularly in connection with institutional issues, where economists are as methodologically functionalist as sociologists and anthropologists (transaction cost economics is mentioned here). In general, Simon continues, it will not do to separate the various human and social sciences on whether they ascribe to rationality to human beings or not; they *all* do — Freudian psychology and sociological social exchange theory are provocatively mentioned as examples —, and economics differs only by having adopted a particular, narrow conception of rationality. In particular, economics is not concerned with the process of choice — such as what are effective procedures for searching for solutions; only with the results. However, if attention is not in unlimited supply, it is necessary to account for the allocation of attention in search processes. A theory of this is a theory of procedural rationality. However, Simon admits, he is not aware “... that there has been any systematic development of a theory of information and communication that treats attention rather than information as the scarce resource” (1978: 13).

The Nobel Lecture is slightly more specific about concrete manifestations of BR than the Ely Lecture. Here, as in many other places in the two lectures Simon employs the mode of argumentation of beginning by criticizing a mainstream position and then arguing that the behavioral alternative is superior. Thus, he begins by strongly criticizing the methodological notion (endorsed in economics by Machlup and Friedman) that theories can only be tested with respect to their predictions of aggregate phenomena. This is a methodologically unsound idea;

instead, attention should indeed be directed towards the soundness of basic assumptions, not the least with respect to behavior. Moreover, it is insufficient to help discriminating between mainstream and behavioral perspectives, as some of the central mainstream predictions, notably negatively sloping demand curves and first-degree homogeneity of production functions – might be as well explained by a behavioral theory. This leads into a lengthy discussion of “normative decision theory,” much of which, quite appropriately for a Nobel Lecture, surveys Simon’s own work. Later in the lecture there are other references to “advances in the behavioral theory,” notably the works of Tversky and Kahneman, Simon’s own work on the psychology of problem solving, theories of organizational decision-making, and various theories of firm organization, such as the work of Nelson and Winter, Cyert and March, Degroot, Radner, Leibenstein, Kornai, Williamson, and, rather surprising, Baumol’s managerialist theory of the firm (which is entirely based on maximization).

### **Perspectives on the Lectures**

In both lectures Simon makes critical and constructive arguments and observations. His main critical targets are the von Neumann/Morgenstern/Savage model, game theory, information economics, oligopoly theory, rational expectations theory on the level of theory and instrumentalism on the level of methodology. These are treated rather harshly. Thus, of subjective expected utility theory we are told “... it is hard to take SEU seriously as a theory of actual human behavior in the face of uncertainty” (Simon 1978: 9), and of game theory, we are told that it “... is embarrassing in the wealth of alternative solutions it offers” (1978: 10). Not only do these involve excesses of rationality, they also may not lead to determinate solutions (oligopoly theory is mentioned a number of times as an illustration; see also Simon 1976). Simon also offers a number of observations on how bounded rationality has a bearing on economic organization, as well as brief surveys of specific work on boundedly rational behavior.

With the substantial benefit of hindsight we can discern a number of reasons why the rhetorics of Simon's two lectures failed to convince contemporary (and succeeding) economists. Most obviously, Simon's *oeuvre* suffered from unusually bad timing (hardly his fault, of course). Thus, the end of the 1970s, the time when Simon gave his two key lectures, is the beginning of the information economics-and-game-theory revolution. Path-breaking work by Arrow, Mirrlees, Stiglitz, Ross, Myersson, Wilson and others was between 5 and 10 years old. The first statements of contract theory were about 5 years old; the revelation principle was almost contemporaneous. The first really convincing applications of non-cooperative game theory – incidentally to the part of economics, oligopoly theory, that Simon characterized as “the permanent and ineradicable scandal of economic theory” (1976: 140) — by Spence, Dixit and others were being worked out at about the same time. Rational expectations were moving from being strictly associated with Lucas and Sargent (and with specific policy positions), to becoming a generally acceptable modeling tool. Etc. Thus, Simon was fighting a battle that even he, in that particular historical context, was bound to lose.

However, even if the historical context had been more favorable to Simon's arguments, there are still a number of fundamental problems with how Simon tried to convince his economist audiences of the soundness of his ideas. These have to do with the lack of definitions of BR, and the lack of modeling heuristics in Simon (1978, 1979). Consider these in turn.

A fundamental problem which many discussions of BR have pointed to is that the concept is defined negatively rather than positively: BR tends to be seen as all those aspects of decision-making that substantive rationality is *not*. The problems with this are, first, that BR only assumes a real existence when viewed against its substantively rational counterpart, and, second, that the set of candidates for boundedly rational behaviors is without bounds. Of course, the problem is inherent in the name of the concept itself, and Simon may have committed a fundamental labeling blunder here. This may explain why he, from about the mid-1970s used the



notion of “procedural rationality” rather than BR, and why he, in fact, uses the concept very little in Simon (1978, 1979). In a discussion of his earlier work, Simon (1979: 502) mentions that

In *Administrative Behavior*, bounded rationality is largely characterized as a residual category – rationality is bounded when it falls short of omniscience. ... There was needed a more positive and formal characterization of the mechanisms of choice under conditions of bounded rationality.

The theory of satisficing search is, of course, one such characterization, and it, as well as other instances of behavioral decision theory, is discussed in Simon (1978, 1979). It has often been argued that a basic problem with satisficing search is that there is virtually nothing in the theory itself about the merits of alternative search procedures, and certainly not in economics. Simon explicitly argues that in order to understand the relative advantages of different procedures, it is necessary to step outside of economics, and consider, for example, work on integer programming. However, his comments on the subject are extremely vague, and he chooses to “... leave the topics of computational complexity and heuristic search with these sketchy remarks. What implications these developments in the theory of procedural rationality will have for economics ... remain to be seen” (1978: 12).

Thus, Simon essentially admits that the theoretically developed basis for theorizing on satisficing is virtually non-existent, and, at any rate, will likely emerge outside of economics. The conclusion for a listener to these lectures *anno* 1978 or 1979 can only be that work based on satisficing search must make use of rules for search and postulate aspiration levels that are essentially arbitrary, and at best justified by loose empirical considerations of a dubious sociological nature. In other words, Simon’s alternative program would not seem to present any non-arbitrary modeling heuristics.

It is not surprising, then, that Simon’s economist audience, being first severely criticized for their methodical practice and listening to a wholesale condemnation of

what they likely saw as hot new ideas, then being instructed to take seriously a fundamentally undefined notion, and finally being exposed to examples and applications of procedural rationality that came close to what they would consider sociological reasoning, was not persuaded, and that the use of BR in economics remains, at best, rhetorical in a pre-McCloskeyan sense of the word.

Could Simon have done it differently, and perhaps more successfully? A fundamental problem is that his message was in many ways so radically counter to most economists' ingrained habits of thought, and that they no doubt must have seen him as someone who refused to play by the rules of the game. Arguably, there was little Simon could have done, and would have wanted to do, about this. However, he could have done *something*, such as cutting down on the polemical elements, which arguably take up a disproportionate amount of space in the lectures. More importantly, he could have done more to present economists with precise behavioral models and the computational models associated with these (Newell and Simon 1972) in order to more convincingly present the case of a genuine behavioral alternative to mainstream modeling of behavior. It is indeed striking that when Simon talks to economists about BR, he is much less specific than when he addresses audiences in artificial intelligence and psychology about the same subject. Perhaps he felt that what was necessary was conveying the big idea and not go into formal detail (which might not have been appropriate in an Ely or Nobel Lecture anyway). That may have been a mistake, for most modern economists like to be told about big ideas in a formal manner (the rational expectations revolution and the new growth theory comes to mind; Lucas 1972 and Romer 1986 are quite formal pieces, indeed).

## IV. Discussion

### **Simon's Influence on Organizational Economists**

There can be little doubt that Simon has had a strong influence on many of those economists who have directed their analytical efforts towards firms and

organizations. Williamson, Winter and Radner immediately come to mind.<sup>12</sup> When various economics approaches to firms and organizations began to emerge in the beginning of the 1970s, Simon's fundamental work on the subject was already two to three decades old (Simon 1945, 1951; March and Simon 1958). For some of the pioneers it was natural to look to Simon's work for inspiration, perhaps particularly those who stressed the routinized nature of firm behavior (i.e., Nelson and Winter 1973). Bounded rationality seemed to link up directly with the notion of routines, since these may be interpreted as firm-level equivalents to individual behavioral rules that are adopted to reduce complexity under bounded rationality. To others the contribution of Simon's thought lies elsewhere and is, in a sense, less direct. Thus, what Williamson appears to have gained from Simon is, first of all, a rationale for incomplete contracting, and, second, the notion that institutional choice is discrete (see Simon 1978). Although he is, of course, familiar with Simon's work in economics from the 1950s which is taken up with actually modeling boundedly rational behavior (e.g., Simon 1955), it is the Simon of the 1978 lecture — the interdisciplinary Simon who goes into institutional choice as a discrete one at considerable length<sup>13</sup> — that seems to loom largest in Williamson's thinking. As mentioned earlier he explicitly dissociates himself from understanding economizing with BR in terms of, for example, heuristic problem solving.

It is notable that those economists that have been able to utilize aspects of Simon's thought in their work on economic organization were graduate students in the 1950s and early 1960s when Simon's influence was perhaps more readily felt, and when he was around for interaction. In contrast, the works of those theorists of economic organization who began to publish from the mid-1970s are arguably much less, and usually not at all, influenced by Simon. One reason why this is so is simply that they were busy assimilating, applying and extending the new information

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<sup>12</sup> For a brief essay on the Simon-Williamson relation, see Augier and March (2001).

<sup>13</sup> Incidentally, Simon (1978) presents this theme as somehow intimately connected with BR, and mentions that discrete institutional choice is at variance with marginalism. While discreteness in choice indeed may require other tools (notions of complementarity and the underlying mathematical lattice theory) than conventional marginalist ones, it is entirely consistent with mainstream economics.

economics and game theory tools when Simon presented his fundamental ideas to economists, and that Simon's vision was simply too far from what they were up to. A part of that explanation, however, is that the Simon papers they were likely to know would be his two end-of-the-1970s papers. And, as has been argued, there is very little in these papers that may instruct economists wanting to build a BR research program in the economics of organization about the exact nature of BR and how to go about doing it. His later papers are equally barren in this respect. Thus, one of his last papers on organizational issues (Simon 1991) surprisingly does not go into BR *at all*, but mostly takes issue with various themes in organizational economics, notably the assumption of opportunism. This has arguably contributed to the absence of a distinct BR research program in the economics of organization.

### **The Future of Bounded Rationality in the Theory of Economic Organization**

So, what will happen to Simon's Grand Theme of BR in the theory of economic organization? Will it gradually disappear, as contract theory takes over the whole field, everybody realizing that what some theorists try to say using BR may be said more elegantly with notions of asymmetric information, drawing on standard methodology? Although this may have been a reasonable prediction, say, 10 years ago, there are reasons to think that the situation is different now.

First of all, the evidence from psychology and experimental economics about the relative failures of expected utility theory is now so large that it cannot be ignored. Although various findings that are contradictory to EU theory were certainly well known at the time Simon gave this lectures, the amount of findings today is many times larger (Camerer 1998), and the scientific quality of the relevant experimental methods is superior. There is, in other words, a much better *critical* foundation for BR.

Second, there is now a much improved *positive* foundation for theories of BR. Economists do not have to look at (at least to them) esoteric branches of

computational theory or AI.<sup>14</sup> In the fields of psychology and decision theory, perhaps closer neighbors to economics than AI, theorists and experimentalists have been at work since the end of the 1970s trying to align some aspects of BR with EU theory or develop distinct alternatives to EU theory (see Camerer 1998 for a fine survey and discussion). While economists may still be uncertain or ignorant about alternatives to EU theory — one wonders how many economists are familiar with cumulative prospect theory with rank-dependent weights which Camerer (1998: 166) singles out as the best alternative to EU theory in the light of the evidence — at least they may be increasingly *alert* to the new alternatives.

Third, within mainstream contract theory, the Maskin and Tirole paper (Maskin and Tirole 1999) is not taken to be the last word about BR (and Maskin and Tirole do not appear to think of it in this way, either). Rather, it may be taken as a contribution that demonstrates the inherent limitations of a certain class of models (Kreps 1996), and points to the need to overcome these limitations, possibly by means of sophisticated treatments of BR (see Segal 1999). However, it is far from clear how to incorporate BR in contract theory. A main problem is that BR threatens to drastically complicate the link between current actions (e.g., investments) and anticipations of future payoffs, because BR is hard to square with the rational expectations of these payoffs. There is no BR theory of expectations (as far as I know) and although notions of satisficing may perhaps be invoked, we are up against the usual problem that virtually anything can be postulated to be a reasonable aspiration level. Still, there are other uses for BR within contract theory, such as understanding the limitations of hold-up (Carmichael and McLeod 1999) and providing more refined understanding of why contracts are incomplete than merely postulating this by *fiat* (Mookerjee 1998; Segal 1999; McLeod 2000).

Fourth, writers associated with transaction cost economics have increasingly begun to look into the BR component of the approach. Thus, Williamson (1998)

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<sup>14</sup> However, it should be mentioned that various computational approaches have been very usefully applied in the context of the theory of the firm by a number of evolutionary economists. See, for example, Marengo et al. (2000). Also, game theorists have made much use of AI notions of BR (see discussion and references in Lipman 1995).

himself has put forward a possible modeling strategy for how to incorporate richer notions of BR in transaction cost economics. 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. The implication is that the efficiency questions of the economics of organization may usefully be reformulated, relying on more elaborate models of BR, so that "... organization can and should be regarded as an instrument for utilizing varying cognitive and behavioral propensities to best advantage" (1998: 12). A limitation of Williamson's (1998) paper (if not of the program he sketches) 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, Foss (2001) sketches various ways in which findings from the bias and heuristics literature may be utilized in transaction cost economics. The main idea is to interpret these findings as potential sources of transaction cost problems (e.g., contribution biases may increase bargaining costs) and argue that such problems influence the choice of governance structure.

These ideas have been criticized by Loasby (2002) who argues that they continue an unfortunate tendency in most of organizational economics to only look at organization as something that exist in order to avoid the negative aspects and consequences of human behavior and not to stimulate the positive ones. There is certainly something to this critique. Transaction cost economics needs to address how governance choice influences alternative methods of search and learning. A recent, very interesting attempt at building a theory of this is Nickerson and Zenger (2002). Relying on complexity theory, they argue that Simonian heuristic search, which is usually necessary for problems that involve many, highly interdependent knowledge sets, is likely to require to substantial knowledge sharing and ongoing

interaction of knowledge sets. However, this exposes those who control the relevant knowledge sets to various knowledge exchange hazards (e.g., the Arrowian paradox of information). Because firm organization makes such hazards less severe and also better enables the building of a specialized language in terms of which the relevant communication may take place, complex problem-solving requiring heuristic search will be organized inside firms. Simpler problems, which may be decomposed into subproblems, and correspondingly simpler search, may be well organized by market governance. Thus, Nickerson and Zenger elegantly combine key ideas from Simon (notably Simon 1962) with key transaction cost economics ideas in a theory of how governance structures both avoid the negative and promote the positive.

## V. Conclusions

This paper has treated BR as a mainly rhetorical part of the practice of organizational economists. Thus, BR is invoked in a loose, intuitive manner to explain more central concepts, notably incomplete contracting, but it is not itself treated in much detail; it is not defined and modeled in any precise manner. I also argued that this practice may be related to the fact that Simon never really explained, at least to his economist audiences, what BR is, how it may be modeled so that it may be of use to economists, and how exactly BR impact on those issues that interests economists of organization (i.e., existence, boundaries and internal organization of firms; contract design). Thus, although the study of economists' rhetorical practice may be helpful for illuminating aspects of scientific development, it has also been suggested that there are limits to the role of rhetoric in scientific development. At least, rhetoric in the sense of mere eloquence is not sufficient, because the successes of persuasion attempts are highly context-dependent. Thus, although Simon in his two major attempts to win economists over to the behavioral side (Simon 1978, 1979), exhibited rather considerable eloquence he failed to persuade, arguably because the context was so unfavorable to his arguments. However, that context has changed. Had



Simon given his Ely and Nobel in, say, 2001 and 2002 he would have found considerably more receptive audiences than he did in 1978 and 1979.

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