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by

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Richard Posner is a prolific writer and distinguished jurist. He is frequently asked to speak with wisdom and authority on many issues. Whether he hits the mark or misses varies with his depth of knowledge and understanding of those issues.

I contend that POSNER's [1993] commentary mainly misses. Specifically, Posner (1) has not understood the Coasian message (or does not like what he hears), (2) misconstrues transaction cost economics,[1] (3) misconstrues game theory, (4) has a truncated understanding of bounded rationality, the economics of information, and maximizing, and (5) mischaracterizes empirical research in transaction cost economics. That is disappointing and a needless setback to the combined development of Institutional Economics/Law and Economics. Readers are reminded that Posnerian Law and Economics is not an error-free enterprise.[2]

1. *Ronald Coase and the 1991 Nobel Prize Lecture*

Posner describes Coase as "the most illustrious economist" associated with the New Institutional Economics and as "one of four founders" of Law and

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[1] Transaction cost economics is a more specialized enterprise than is the New Institutional Economics. The same is true of the relation of Posnerian Law and Economics to Law and Economics. Because Law and Economics is a field with which I have had a long and satisfying affiliation, whereas my relationship to Posnerian Law and Economics has frequently been one of caution or dissent, and because Posner's remarks are principally directed at transaction cost economics, which is my predominant concern as well, the title of my response signals a more limited terrain than Posner purports to cover.

[2] Earlier work of Posner's with which I have taken exception includes his involvement in the *Schwinn* case (see WILLIAMSON [1979, 975–985]) and his unduly sanguine assessment of the efficacy of franchise bidding for natural monopoly (see WILLIAMSON [1976]). For a more general critique, see Mitchell POLINSKY's [1974] "Economic Analysis as a Potentially Defective Product: A Buyer's Guide to *Posner's Economic Analysis of Law*." Also see Frank MICHELMAN's [1979] precautions in "A Comment on *Some Uses and Abuses of Economics in Law*."

Economics. Correct as far as it goes, but it does not go far enough. Ronald Coase is both the most illustrious economist associated with the New Institutional Economics *and* one of the two most illustrious scholars among the four founders of Law and Economics. It was Ronald Coase and Guido Calabresi who made the pathbreaking intellectual contributions which ushered in the Law and Economics movement. That is obscured by Posner's "four founders" formulation.

Considering Coase's prominence in both the New Institutional Economics and in Law and Economics, one might have expected Posner to celebrate, along with other conference participants, Ronald Coase's 1991 Nobel Prize in Economics. Instead, Coase comes off as eccentric.

The Royal Swedish Academy's citation to Coase emphasizes the importance and influence of two of his major papers: his 1937 article on "The Nature of the Firm" and his 1960 article on "The Problem of Social Cost." Transaction cost economics relates mainly to the former, while the latter is credited with moving the Law and Economics out of antitrust into the study of law more generally. How does Coase treat these two papers and these two fields in his Nobel Prize lecture?

One possibility is that Coase would give them equal prominence – in which case the lecture might be titled "Law, Economics, and Institutions." The other possibility is that one article/field would be given greater emphasis than the other. Judging from Posner, who regards the New Institutional Economics (and, a fortiori, transaction cost economics) as a narrow enterprise as compared with Law and Economics, Coase ought to have featured his 1960 article and its dramatic impact on Law and Economics – in which event the lecture might have been titled "The Evolution of Law and Economics" and the 1937 paper would be viewed a precursor to the big breakthrough in 1960.

That is not, however, the way that the lecture reads. Not only is the lecture titled "The Institutional Structure of Production," but Coase features the 1937 article and holds that "the important contribution of this article is the explicit introduction of transaction costs into economic analysis" (Coase [1992, 716]). His ambition, moreover, is to see that transaction cost arguments are brought to bear broadly and systematically throughout economics (Coase [1992, 716]).

That ambition is repeated in Coase's treatment of his 1960 article, which is introduced as follows (Coase [1992, 717]):

> I now turn to the other article cited by the Swedish Academy, The Problem of Social Cost, published some thirty years ago. I will not say much about its influence on legal scholarship, which has been immense, but will mainly consider its influence on economics, which has not been immense, although I believe that in time it will be.

The novelty of the 1960 article is that it used the fiction of zero transaction costs (which can be thought of as a strong version of unbounded rationality) as a device to display the deficiencies of much of the prior work on externalities and property rights. This fiction was merely an analytical device, however.[3] It was meant to be used as a "stepping stone on the way to an analysis of an economy with positive transaction costs" (COASE [1992, 717]). Accordingly, the crucial lesson of the 1960 article is the same as that of the 1937 article: "let us study the world of positive transaction costs" (Coase [1992, 717]).

Given the immense influence of his 1960 article on legal scholarship and (in Posner's view) the lesser importance of transaction cost economics, how could Coase have gotten his priorities so wrong? Possibly, in the fullness of time, Coase will come to see the error of his ways. But while I concur that Law and Economics has made great strides, I am also in broad agreement with the central emphasis and unifying theme of Coase's lecture: as Posner might put it, "This is music to Williamson's ears" (POSNER [1993, 84]).

2. *Transaction Cost Economics*

Posner questions the importance of the New Institutional Economics in general and my work on transaction cost economics in particular.[4] The importance of both is for others to decide. A correct rendition of the facts will lead to better decisions.

Transaction cost economics is intendedly responsive to the Coasian imperative: study the world of positive transaction costs.[5] It does this mainly in a comparative institutional way – according to which the factors which influence the efficacy of alternative modes of organization (governance) are examined in relation to the attributes of transactions. This is a *microanalytic enterprise* (ARROW [1987, 734]). It requires that the details of governance structures and transactions be examined at a higher level of resolution than economists had hitherto thought to be necessary.

The difference between the firm-as-production function and the firm-as-governance structure, which goes unmentioned by Posner, is one illustration. Although the former is the familiar and for many purposes the appropriate

[3] Indeed, one could argue that Coase was not the first to employ it. The idea that costless bargaining leads to efficiency was employed by VON NEUMANN and MORGENSTERN [1944] and even earlier by EDGEWORTH [1881]. Transaction cost is a larger concept than bargaining costs, however, and, as presented by Coase, invites comparative institutional analysis – or at least that has been my experience, shared with many others.

[4] Transaction cost economics is a larger enterprise than that described here. A decade ago I was aware of almost all of the work in transaction cost economics – often before it was published. That is no longer true today. My discussion here deals with the issues raised by Posner.

[5] Although COASE [1992] sees real merit in this exercise, he is not persuaded that asset specificity is as important as I and others have treated it (COASE [1988, 12–16]). See WILLIAMSON [1988, 70] and KLEIN [1988] for responses. Also, COASE [1988, 38] has emphasized the importance of management costs. A partial response is WILLIAMSON [1991 a].

construction, there are other purposes for which the firm-as-governance structure is more instructive (TIROLE [1988, 17–34]; HOLMSTROM and TIROLE [1989]; KREPS [1990 a]; [1990 b]). Transaction cost economics has been instrumental in bringing about this shift in emphasis.[6]

Relatedly, transaction cost economics maintains that all complex contracts are unavoidably incomplete. Accordingly, all of the relevant contracting action cannot be concentrated on the ex ante incentive alignment but some of it spills over into ex post governance. To be sure, the analysis of incomplete contracting is formidably difficult (GROSSMAN and HART [1986]). Increasingly, however, many are persuaded of the need to address the problems of organization on the terms that transaction cost economics prescribes. Implicitly, if not explicitly, asset specificity, bounded rationality, and opportunism have become key features.

Also, transaction cost economics combines the insights and arguments of both Friedrich HAYEK [1945] and Chester BARNARD [1938] that adaptation is the central problem of economic organization. But whereas HAYEK [1945] emphasized spontaneous coordination, effected through the price mechanism, BARNARD [1938] emphasized intentional coordination, effected through hierarchy. The apparent opposition of these two arguments notwithstanding, transaction cost economics avers that adaptations of both kinds are needed in a high performance economy and joins both in a common framework. Three different contract law regimes – classical, neoclassical, and forbearance (which support, respectfully, market, hybrid, and hierarchical modes of governance) – are features in this framework (WILLIAMSON [1991 a]).

Finally, transaction cost economics traces some of the enduring puzzles of economic organization to intertemporal process transformations that went unnoticed by orthodoxy. That neglect may be because others shared Posner's view that "organization-theory ... [has] nothing to add to economics that the literature on information costs had not added much earlier" (Posner [1993, 84] and/or because others, like Posner, do not want to struggle with the deep puzzles of organization. Information economics did not, however, discover (indeed, generally ignores) the "unintended consequences" that sometimes attend efforts to exercise control (MERTON [1936]; GOULDNER [1954]; MARCH and SIMON [1958]), which consequences turn out to be vital to an understanding of the employment relationship. And both the Fundamental Transformation and the impossibility of selective intervention – jargon, to be sure, but important nonetheless – were discerned and explicated by taking the twin conditions of bounded rationality and opportunism seriously.

[6] David Kreps succinctly distinguishes between the neoclassical and transaction cost economics conception of the firm as follows: "The firm is like individual agents in textbook economics. Agents have utility functions, firms have a profit motive; agents have consumption sets, firms have production possibility sets. But in transaction cost economics, firms are more like markets – both [firm and market] are [governance structures] within which individuals can transact" (KREPS [1990 b, 96]).

Posner nevertheless asserts that there is no novelty in Williamson's work "in identifying new sources of 'market failure' and domesticating them for theory" (POSNER [1993, 81). There are several responses. First, this is a symmetrical test: What are the new sources of market failure that Posner and his co-authors have discovered? Second, why the preoccupation with market failure? Surely Posner has heard, if not learned, the Coasian message that all forms of organization are subject to failure. But my third and main response is that I have been alert to several types of comparative failures. These include (1) first mover advantages, (2) serious problems with common knowledge assumptions, (3) the Fundamental Transformation, and (4) the impossibility of selective intervention.

My initial discussion of first mover advantages was in relation to dominant firm industries. The argument was that even if early and late entrants had identical intertemporal cost profiles, early entrants would enjoy competitive advantages over later entrants if learning curves were negatively sloped (WILLIAMSON [1975, 216–217]). The argument relates to earlier discussions by Carl KAYSEN and Donald TURNER [1959, 73–75] and can be thought of as a precursor to later work in which experience-curve pricing is used to deter entry (ENCAOUA, GEROSKI, and JACQUEMIN [1986]). Being preliminary, my treatment does not qualify as "domestication."

My examination of vertical integration as a means by which to raise the cost of capital incurred by new entrants is both more novel and more complete (WILLIAMSON [1974, 656–659]; [1975, 110–113]; [1979, 960–966]). Posner once described this project as follows (POSNER [1979, 945]):

> Because Stigler's definition of a barrier to entry, as a cost that differentially affects new entrants compared to firms already in the market, is now generally accepted, the search is for costs having this characteristic. The most sophisticated quester, Oliver Williamson, has found one: the uncertainty of the new entrant's prospects may force him to pay a higher premium to obtain capital than existing firms must pay. This is a legitimate point.

It is furthermore noteworthy that the 1982 Merger Guidelines of the U.S. Department of Justice expressly adopt this reasoning – which may qualify as domestication.

With reference to common knowledge, my argument is this: common knowledge, or best efforts to realize common knowledge, may not suffice. Three cases can be distinguished: one party has more of the relevant information than the other; each party has different (some better, some worse) information than the other; and both have the same information.

Consider the first case and assume that the less well informed party (say B) asks to be apprised of the information possessed by the more well informed party (A). That may be difficult because (1) it is costly for A to transmit his information to B (possibly because B is unsophisticated and cannot understand it), (2) A may be unable to articulate the nuances because he lacks the words or does not fully understand his own intuitions, or (3) B suspects that A will make only a partial and biased disclosure. Conditions (1) and (2) are manifestations of bounded rationality. Condition (3) reflects additional concerns with opportunism. Were it not for these behavioral features, ex ante information asymmetry would be supplanted by information equivalence.

Case two is merely a complex variant of case one. So consider case three, where both parties are identically informed. Assume, however, that it is very costly to apprise an objective third party, such as the court, as to the true state of affairs. Then if the true state of affairs is Z but A says X and B says Y, an uninformed court does not know if either or both are lying.

To be sure, clever stratagems can sometimes be employed – as with King Solomon's pronouncement that the disputed baby will be divided, whereupon the true mother is revealed. But such stratagems are rare (and, furthermore, cannot be repeated if reputation effects work well). *Three-way* common knowledge – to include the arbiter as well as the immediate parties – is a much more demanding condition, yet three-way common knowledge may be needed to avoid contractual complications.

All of these cases, and some of the organizational ramifications that accrue thereto, are discussed in *Markets and Hierarchies* (see especially pages 31–37). At the time that was written (1975), complications of these kinds were little recognized. That has changed in the years since.

The Fundamental Transformation explains why the problem of bilateral dependence – previously treated as a very special condition of *pre-existing* bilateral monopoly – is actually a very widespread and troublesome condition. The argument is this: competition among large numbers of qualified suppliers at the outset is frequently *transformed* into a small numbers exchange relation during contract execution and at contract renewal intervals. That will obtain if investments in transaction-specific assets are made by or accrue to initial winning bidders during contract execution. Initial winners of bidding competitions in such circumstances will enjoy advantages over nonwinners, on which account parity is upset and a condition of bilateral dependency sets in. The discovery and explication of the Fundamental Transformation is very much a transaction cost economics exercise (WILLIAMSON [1971], [1975], [1985]; KLEIN, CRAWFORD, and ALCHIAN [1978]). It is ignored by students of comparative economic organization only at their peril. Posner has yet to acknowledge those difficulties, even though he has been one of its victims. Many others have seen merit in this formulation, some of whom initially held otherwise (WILLIAMSON [1985, 53]).

Also, the impossibility of selective intervention is my answer to one of the truly deep puzzles of economic organization: Why can't a large firm do everything that a collection of small firms can do and more? Were it that an acquiring firm could *credibly* promise to respect autonomy save for those cases where expected net gains to intervention can be projected, then interfirm contracts could always be profitably supplanted by merger. That is because the combined entity could always do as well as autonomous trading (through replication) and would sometimes do better (through selective intervention). Reformulating and unpacking that puzzle is a project with which transaction cost economics has been expressly concerned.

That puzzle may not interest Posner. It was nevertheless posed by Coase in 1937 (and even earlier by Frank Knight) and resisted successive attempts to deal with it for almost fifty years (WILLIAMSON [1985, 132–135]). The literature on high- and low-powered incentives is closely connected with (and much of it is an outgrowth of) the work on selective intervention (more precisely, the impossibility thereof). This has ramifications beyond those of firm and market to include issues of Eastern European economic reform.

What explains Posner's dismissive claims? One possibility is that Posner does not read or does not understand the literature. Be that as it may, transaction cost economics has identified hitherto neglected sources of "organizational failure" (of which "market failure" is a special case); and many students of economic organization agree on the need to come to terms with these conditions.[7]

3. *Institutional Economics and Game Theory*

3.1 We are all Institutionalists now

Abstruse mathematical economics aside, POSNER [1993, 76] avers that "all economists believe that they are engaged in the study of institutions." But when did this come about? And what does this mean?

It was not so long ago that economics was defined as the study of price and output – in which event institutions are implicated very minimally. Consider Coase's interpretation of Lionel ROBBINS' [1932] influential views on *The Nature and Significance of Economic Science*: "in Robbins' view, an economist does not interest himself in the internal arrangements within organizations but only in what happens on the market ... [M]icroeconomics is largely a study of the determination of prices and output" (COASE [1992, 714]). Those views had a lasting hold on the profession. Kenneth Arrow made note of, and was among the first expressly to take exception with, those views in his more "institutional" approach to the medical-care market in 1963:

> The first step ... in the analysis of the medical-care market is the comparison between the actual market and the competitive model. The methodology of this comparison has

[7] Yet another variety of organizational failure that I have discussed relates to the "economics of atmosphere" (WILLIAMSON [1975, 37–39, 256]; [1992 a, 31–33]). The main applications are to employment relations and externalities and the question is this: Ought all tasks/externalities to be metered at the margin which, taken separately, can be metered with net gains? The answer is that if functional separability does not imply attitudinal separability, then the conventional metering prescription will lead to an excess of metering, hence to avoidable losses.

been a recurrent subject of controversy in economics for over a century. Recently, M. Friedman has vigorously argued that the competitive or any other model should be tested solely by its ability to predict. In the context of competition, he comes close to arguing that prices and output are the only relevant data ...

In this paper, the institutional organization and the observable mores of the medical profession are included among the data. (ARROW [1963, 943–944])

A growing interest in institutions, expecially in the support institutions for contract – where contract is broadly construed to include internal organization, has gradually taken hold. My conference paper in this volume (WILLIAMSON [1993]) traces some of those developments. If we have now reached the stage where we are all institutionalists now, let's celebrate.[8] If some concur that transaction cost economics has helped to promote this transformation, I am grateful.

Posner, however, dissents. He observes that Williamson favors "new concepts such as 'bounded rationality', 'asset specificity', 'information impactedness', 'dimensionalizing' and 'opportunism'" and believes "that he is drawing new disciplines into economics, notably law and organization theory" (POSNER [1993, 80]). Posner will have none of that. Bounded rationality, in his view, has no content beyond the proposition that information is costly, hence should be dealt with by the type of information economics done by George Stigler (POSNER [1993, 80]). Posner further avers that the problems posed by asset specificity, opportunism, and asymmetric information are not only old themes in economics but "The increasing use of game theory to address these problems comes out of – game theory, rather than anything special to the new institutional economics" (POSNER [1993, 81]). Also, as previously remarked, POSNER [1993, 84] believes that "organization-theory ... [adds] nothing to economics that the literature on information costs had not added years earlier".

I examine bounded rationality and information economics in section 4. Consider here the relation of transaction cost economics to game theory.

3.2 Game Theory

I could understand – which is not to say that I would agree with – Posner's claim that game theory is a self-contained way to address institutional economics issues if Posner's name were Fudenberg, Hart, Holmstrom, Kreps, Laffont, Milgrom, Roberts, Tirole, etc. But Posner's name is, well, Posner. To my

[8] I would hesitate, however, to make this claim. The firm, after all, is an institution. But to describe the firm as a production firm, as many economists do (any for many purposes is the appropriate construction), suppresses its governance structure properties. Also, the Stigler-Becker approach to politics has been described by Terry Moe as one that not only ignores the new economics of organization but that "proudly ignore[s] institutions altogether" (MOE [1990, 129]). Growing agreement that institutions matter and are susceptible to analysis (MATTHEWS [1986, 903]) notwithstanding, we have not yet reached unanimity.

knowledge, he has made no fundamental contributions to and makes very little express use of game theory. Since "theory dropping" is problematic, let us examine the data.

Game theorists, like most economists, are much less doctrinaire than Posner. Partly that is because they are aware that game theory is not self-contained but is only a tool. There is a prior need to pose the problems. If transaction cost economics poses the problems, so be it. If game theorists come upon these independently, so be it.

A number of applied game theorists have found it instructive to work out of a transaction cost economics formulation of the issues.[9] That is obvious from the pathbreaking paper by Sanford GROSSMAN and Oliver HART [1986] on vertical integration and from Jean TIROLE's [1988] chapter on the "Theory of the Firm" in his text, *The Theory of Industrial Organization*. Also, the Bengt HOLMSTROM and Jean TIROLE [1989] chapter on "The Theory of the Firm" in the *Handbook of Industrial Organization* relies substantially on transaction cost economics. The Oliver HART and Bengt HOLMSTROM [1987] chapter on the "Theory of Contracts" in *Advances in Economic Theory* does likewise. Although Drew FUDENBERG, Paul MILGROM, and Bengt HOLMSTROM [1990] take exception with my arguments about incomplete contracting, mistakenly I think (WILLIAMSON [1991 b]), they expressly relate to them. The same is true of the Paul MILGROM and John ROBERTS [1992] book on *Economics, Organization, and Management*.

David KREPS [1992], however, is the person who has taken the deepest interest in bringing game theory to bear on transaction cost economics issues. His paper on "(How) Can Game Theory Contribute to a Unified Theory of Organization?" begins with the following statement: "almost any theory of organization which is addressed by game theory will do more for game theory than game theory will do for it" (KREPS [1992, 1]). Not only is the relation between organization theory and game theory a two way street, but game theory is the principal beneficiary! Kreps's paper deals with "relational contracts" (KREPS [1992, 2]), which POSNER regards with contempt [1993, 84], with special emphasis on the employment relation, as originally formulated by Herbert SIMON [1951] and subsequently developed by transaction cost economics (KREPS [1992, 8–46]). Among the (transaction cost economics) issues with which Kreps is concerned are "problems of governance brought on

[9] A mathematical economist colleague once told me – in all seriousness, I believe – "you write mathematics." That is too high a compliment, but I was pleased.

[10] Posner objects to the term information impactedness. My discussion of common knowledge (in section 2, above) is pertinent. As discussed there, information asymmetry does not pose a problem if and when that condition can be relieved at negligible cost. Information impactedness refers to those information asymmetry conditions where it is costly, perhaps impossible, to effect redress.

Because this caveat has come to be recognized and because information impactedness is an awkward term, I have been chary about using the term more recently. (There are by contractual incompleteness" (KREPS [1992, 20]), the need to buttress firm reputation effects by safeguards (KREPS [1992, 22]), impacted information (KREPS [1992, 23, 47]),[10] the bounded rationality of managers (KREPS [1992, 24]), the added credibility of governance based on procedures (KREPS [1992, 28]), the importance of real time coordination (KREPS [1992, 29]), the idea that organizational forms need to be thought of as clusters of internally consistent attributes (KREPS [1992, 32–33, 41]), the hazards of opportunism (KREPS [1992, 37]), and social embeddedness (KREPS [1992, 44–46]). Kreps concludes on the same note on which he started: "An important part of organizational analysis, it seems clear, will be a study of relational contracting" (KREPS [1992, 47]).

3.3 Relational Contracting

Ian Macneil is a distinguished legal scholar. I have found his work in contract law – particularly his 1974 and 1978 articles – especially illuminating. To be sure, others – including Karl LLEWELLYN [1931], Stewart MACAULAY [1963], Lawrence FRIEDMAN [1965], Clyde SUMMERS [1969], Marc GALANTER [1981], and many labor law specialists – have observed that the legal rules (and, even more, the economic)[11] conception of contract is unsuited to many purposes and can impair our understanding of economic organization. What we need to do is to think about contract laws (plural) rather than contract law (singular). Although Posner may regard that as unimportant, obvious, or wrong-headed (it is impossible to infer from his remarks about Macneil), I have found the more expansive view of contract developed by Macneil and others helpful in my efforts to understand economic organization. Armen Alchian (who called my attention to Macaulay) and Victor Goldberg (who recommended that I read Macneil) have also found this richer description of contracting useful. If anything that deepens our understanding of complex economic organization is to be valued,[12] then we should celebrate rather than denigrate this line of work.

27 references to information impactedness in *Markets and Hierarchies* and only 3 in *The Economic Institutions of Capitalism.*) Some colleagues nevertheless continue to use it, and some game theorists are among the users. If in the end it vanishes, I will have no regrets. The important thing is that we engage rather than elide the real issues.

[11] It was analytically convenient for economists to buy into the notion that efficacious rules of law regarding contract disputes are in place and are applied by the courts in an informed, nuanced, and low-cost way. That is tantamount to a zero transaction cost construction. The need for economists to study comparative economic organization is thereby relieved.

[12] This eclectic view was first presented to me when I was a graduate student at Carnegie. I had written a paper on "Selling Expense as a Barrier to Entry" (subsequently published as WILLIAMSON [1963]) for Herbert Simon's course on Mathematical Social Science. The paper used very orthodox economic techniques of constrained maximization, which were at variance with the main thrust of the course. That was not a concern for Simon, who received it with the remark that anything that deepens our understanding, rather than use of a "sanctified methodology," was his test.

That I have benefitted from Macneil does not require, however, that I follow Macneil everywhere he goes. Indeed, Macneil's description of the richness of contract goes beyond that which I have found useful (WILLIAMSON [1979, 236]). It is more convenient for Posner, however, if he implicates me in all that Macneil says and writes. In the spirit of advocacy scholarship, that is what he does (POSNER [1993, 84]).

Although Posner may be discomfited, I am happy to side with him in this argument that Macneil has gone too far in his criticism of economics. More generally, the point is this: I have learned from Macneil, but Macneil and I have our differences. (The same is true, for that matter, of my relation to all of the "four teachers" to whom I dedicated *The Economic Institutions of Capitalism* (Kenneth Arrow, Alfred Chandler, Jr., Ronald Coase, and Herbert Simon).) [13]

4. *Bounded Rationality, Information Economics, and Maximizing*

Posner queries whether bounded rationality means more than "that economic actors have and must (rationally) act on less than full information" and he observes that the "costs of information, both acquisition and processing costs, have been a part of mainline economic theory since Stigler's famous article more than thirty years ago" (POSNER [1993, 80]). I examine bounded rationality first and then turn to the economics of information and maximizing.

4.1 Bounded Rationality

I am persuaded that social science research is influenced by the attributes that we ascribe to economic actors (SIMON [1985, 303]) and I, like many others, have urged that the assumption of hyperrationality should sometimes be supplanted by bounded rationality. Bounded rationality is not a term of my own making. It originates with Herbert Simon, who has defined bounded rationality as behavior that is "*intendedly* rational, but only *limitedly* so" (SIMON [1957a, xxiv]). I offered that definition (in my paper and in my oral remarks at the conference), but Posner complains that the definition is not "useful" (POSNER [1993, 80, n. 11]).

Posner believes that bounded rationality means only that information is costly to acquire and process. But it also refers to the impossibility of thinking through complex but well-structured problems – such as describing the complete decision tree for chess (SIMON [1972]). It can also be used to describe "misconceptions" – such as myopia or the inability to correctly process low probability events. The confusion that sometimes arises when there is a need to deal instantly with, rather than to reflect upon, unanticipated contingencies

[13] I come from a small school (Carnegie) that believes that ideas should be contested and does not have a reputation for protecting its own.

(WILLIAMSON, [1992a, 14, 24–25]) is another manifestation. Another relates to the limitations of language (POLANYI [1962]). KREPS [1990c, 156] interprets the ramifications of bounded rationality for modelling in the following way: "in building a model, one must balance the tractability and cost of analysis of the model with the amount of detail put in, a trade-off that itself is not amenable to analysis; the balance must be struck according to intuition (i.e., heuristically)."

The idea of mind as a scarce resource (SIMON [1978, 12]) is an especially productive way to think about bounded rationality. It immediately suggests the need to economize on bounded rationality. All complex contracts – for capacity constraint reasons but also intentionally – will be incomplete. Contractual incompleteness, in conjunction with asset specificity and opportunism (WILLIAMSON [1975, chapter 2], [1985, 30–32, 61–67]), leads directly into the study of ex post governance.

Information cost economics (of the kind to which Posner favorably refers) tends to work out of the firm-as-production-function rather than firm-as-governance structure setup. The former does not invite attention to the microanalytics of transactions and governance. Also and related, the economics of information did not discover the Fundamental Transformation or the impossibility of selective intervention. Those are bounded rationality constructions.

4.2 *Information Economics*

The information economics to which Posner calls special attention is the work of George Stigler. I agree that this was and is an important line of research. It contributed to significantly Stigler's Nobel Prize award. But I would make three further points: (1) Simon had already covered many of the pertinent issues in his 1955 article in the *Quarterly Journal of Economics*; (2) contrary to Posner, I have made favorable reference to and use of Stigler's work; and (3) transaction cost economics is especially indebted to a strand of information economics ignored by Posner, namely, that originated by Arrow. Consider each.

4.2.1 *Search*

Stigler opens his famous 1961 article on "The Economics of Information" with the observation that "One should hardly have to tell academicians that information is a valuable resource ..., yet it occupies a slum dwelling in the town of economics. Mostly it is ignored: the best technology is assumed to be known; the relationship of commodities to consumer preferences is a datum" (STIGLER [1968, 171]). Simon's 1955 article, "A Behavioral Model of Rational Choice," opens similarly: "the task is to replace the global rationality of economic man with a kind of rational behavior that is compatible with the access to information and computational capacities that are actually possessed by ... man" (SIMON [1957b, 241]).

The search for information is crucial to the theories of rational choice developed by both Stigler and Simon. In Stigler's case, the search criterion is that of expected net gain, which involves optimization. The search procedure described by Simon involves "the replacement of the goal of *maximizing* with the goal of *satisficing*, of finding a course of action that is 'good enough'" (SIMON [1957 b, 204–205]; emphasis in original). Satisficing is cumbersome and one must ask whether the added cost (of nonstandard and more complicated modes of analysis) justifies the benefit (realism of cognitive assumptions). Judging from the influence of these two articles (Stigler, massive; Simon, limited) and from the negative verdict on the utility of the satisficing approach more generally (AUMANN [1985]), the answer would appear to be negative. The jury may still be out on this, however; interest in satisficing has by no means vanished and may be increasing.

Be that as it may, I would observe that neither Stigler nor Posner (who makes a great to do about citations, or the lack thereof) cite Simon – although Simon's article was published in a major economics journal, was reprinted in an influential book of essays, and is described by SIMON [1985, 293] as his "chief epistle to the economists." I do not take that as a mark of disrespect; it is simply a fact.

4.2.2 References to Stigler

Posner complains that I make no reference to Stigler's work on the economics of information. That is not correct. Chapter 12 of *Markets and Hierarchies* [1975] deals with oligopoly, with special emphasis on interfirm versus intrafirm organization. The chapter opens with a discussion of economic and legal antecedents. The economists on whom I rely are William FELLNER [1949], on the efficacy of tacit collusion, and George STIGLER's use of statistical inference techniques to examine oligopoly (STIGLER [1964]). The legal antecedents are Donald TURNER [1962] and Richard POSNER [1969].

Maybe Posner would have me rely on him and on Stigler more extensively. That is a judgment call; arguments can be made both ways. What I find astonishing, however, is that Posner makes only a single (rather negative) reference to Fellner in his 1969 article, and Stigler makes no reference to Fellner in his entire 1968 book. Fellner, in my judgment, deserves better.

Also, I am not the only transgressor. The famous article, "Production, Information Costs, and Economic Organization" by Armen ALCHIAN and Harold DEMSETZ [1972], features information costs in its title but makes no reference whatsoever to STIGLER [1961]. I do not believe that this omission is because they were unaware of STIGLER [1961] or withheld citation as a grudge. Rather, the information costs of concern to Alchian and Demsetz were *very different from* the search costs emphasized by Stigler. Information costs being an expansive term and Stigler's use of it being limited, not all users owe intellectual debts to Stigler.

4.2.3 Arrow

Search and statistical inference techniques do not exhaust information economics. The ways in which information influences economic organization include (1) the "fundamental paradox" of information: "its value for the purchaser is not known until he has the information, but then he has in effect acquired it without cost" (ARROW [1971, 152]); (2) information asymmetry: "the critical impact of information on the optimal allocation of risk bearing is not merely its presence or absence but its inequality among economic agents" (ARROW [1969, 55]); (3) appropriability: "It really calls for some explanation, why the firm that has developed the knowledge cannot demand a greater share of the resulting profits" (ARROW [1962, 355]); and (4) the distinction between hidden action and hidden information problems (moral hazard and adverse selection, respectively) (ARROW [1985, 38–48]). These more microanalytic concerns with information (as well as related concerns about tacit information examined by Michael POLANYI [1962]) are more central to transaction cost economics than are search costs. That is neither here nor there. That is simply a fact.

4.3 Maximizing

Posner chides me, but especially Coase, for rejecting "rational utility maximization," which approach he favors and illustrates with a numerical example (POSNER [1993, 85]):

> So if I am offered a choice between 3 and 2 I will prefer 3. But what if I have another opportunity, worth 4? Then I will prefer it, because I prefer more to less. Well, what if I have still another opportunity, worth 5? I will choose that. And so on until I have – maximized my utility.

To infer utility maximization from the consistent choice of more over less (where the options are costless and unambiguous) is akin to inferring the capacity to play competitive chess by observing that a child has mastered tic-tac-toe. Many Law and Economics scholars, myself included,[14] are embarrassed to have utility maximization so described.

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[14] Posner makes no reference to my involvement in Law and Economics (POSNER [1993, 82, n. 16]). That is consistent with the decision by Posner and Landes to exclude me from their list of economists whose citations are counted in their statistical assessment of the influence of the economic analysis of law (LANDES and POSNER [1992, 9, n. 15]), which decision permitted them to reach the following result: "Two economists (Landes and Goetz) account for more than 45 percent of the law-journal citations in this sample. Both have co-authored many articles with law professors (Landes with Posner and Goetz with Scott). This suggests – though weakly, because of the tininess of the sample – that a law and economics article has a greater impact when it contains a large dollop of legal culture than the typical law and economics article written by an economist" (LANDES and POSNER [1992, 13]). One of the problems with tiny samples is that they are not robust to excluded observations.

For one thing, it is elementary that gradient methods will normally converge to a local optimum at best. (Posner might protest that of course he will leap from a peak of 10 to a peak of 25 if given the chance. But why did he painstakingly climb the peak to 10 when he could have been at 25? And if "information economics" is the answer, then we need a more complete description.) To infer utility maximization from the observation that consumers prefer more (positively valued) goods to less does not, moreover, scale up to large groups of agents who are operating in various degrees of cooperation and conflict and are confronted with the need to make multiple decisions in the face of uncertainty. Indeed, the meaning of utility maximization is obscure in these circumstances.

To be sure, simplification in science is vital. But there is a need to distinguish between naïve simplifications and fruitful simplifications. The contrast with Coase, who uses the fiction of zero transaction costs to lay bare the core issues, whereupon positive transaction costs are introduced and are used to assess the efficacy of alternative modes of governance, could not be more stark. Posnerian Law and Economics – invoke the costs that suit your purposes – and Coasian Law and Economics – continuously study the world of positive transaction costs – are really very different.[15]

5. Empirical Research in Transaction Cost Economics

POSNER [1993, 76] characterizes the empirical research program in the New Institutional Economics as follows:

> Intensive scrutiny of particular institutions implies in turn an emphasis on the case study, relying heavily on histories and ethnographies, judicial opinions and other court records, newspaper and magazine accounts, and even interviews, rather than on studies of aggregated data, using statistical methodology. So the impression is of a field that is skeptical not only about formal economic theory but also about econometrics.

The last of these statements is unwarranted. To be sure, transaction cost economics is a more microanalytic exercise than received microtheory, on which account focused case and industry studies have been illuminating. I count Alfred D. Chandler, Jr. among my "four teachers" (even though I never had him in class) precisely because his studies of microanalytic business history uncovered such important features of the industrial landscape. However, *most* of the empirical work in transaction cost economics has employed standard econometric techniques (see the surveys by Paul JOSKOW [1988], [1991], Howard SHELANSKI [1991], and Scott MASTEN [1992]). I would estimate that the number of such studies is over 200, and I know that it is growing.

[15] Selective attention to the costs that suit his purposes helps to explain why Posnerian Law and Economics is market-favoring in the extreme. Contrast POSNER [1972] with WILLIAMSON [1976].

Although there are differences between this work and much of the empirical work in applied microeconomics, the differences are not in the use or nonuse of econometric methods. Rather, because empirical transaction cost economists concur with the proposition that the "task of linking concepts with observations demands a great deal of detailed knowledge of the realities of economic life" (KOOPMANS [1957, 145]), and because the action resides in the details, the main difference is that the people who have tested transaction cost economics propositions have gone out and collected the relevant microanalytic data – often by examining contracts and getting knowledgeable engineers and managers to discuss practices and fill out questionnaires. That is a daunting exercise, but science progresses by collecting its own data rather than by using the data at hand (KUHN [1971]). COASE [1992, 714, 718–719] makes repeated reference to the need for additional empirical work in his Nobel Prize lecture. That advice is even now being heeded.

6. *Concluding Remarks*

Posner follows his remarks that game theory owes nothing to the New Institutional Economics with the following characterization of my work: "Unless I am mistaken, the novelty of Williamson's work is ... in *inviting* economists' attention to a host of unexplored problems and in contributing to their solution by exploring the ways in which businessmen overcome transaction costs by a variety of devices in a variety of settings" (POSNER [1993, 81]; emphasis added).

It is, of course, flattering to imagine that other economists are so deferential to my views that they have responded to my "invitation" to explore neglected problems. But that is also implausible.

Economists, in my experience, are very tough-minded social scientists who are unmoved by laundry lists of "interesting problems." Moreover, few of the problems on which I have worked are original to me; most have a very long literature. Tell economists something that they didn't previously know about phenomena of interest to them and demonstrate that the data line up: that will get their attention. *Fruitful reformulations of problems* are what count.

I did not, for example, discover the vertical integration problem. Rather, I concluded, after an extensive reading of the literature, that Coase was correct in ascribing the decision to integrate (or not) to differential transaction costs. I therefore set about to unpack the puzzle of markets and hierarchies in a more complete way. Similarly, I did not discover the problem of corporate governance. That had been in the literature since the separation of ownership and control problem was posed by Adolph Berle and Gardiner Means in 1932. What I did was systematically apply the "simple contractual schema," which is a transaction cost economics construction, to examine that issue. Similarly, I did not discover the employment relation, pose the puzzle of debt and equity, advance the (mistaken) hypothesis that franchise bidding for natural monopoly was reliably efficacious, discover the modern corporation (M-form, conglomerate, multinational, Japanese, etc.), originate the notion of credible commitment, and the list goes on. Listing researchable problems requires a good nose, but that is easy as compared with addressing these problems in ways that lead to different, and especially better and deeper, understandings.

The transaction cost economics project – which draws on law, economics, and organization and demonstrates, once again, that the whole can be larger than the sum of the parts – is by no means played out, moreover. Although Posner may be unable to relate to the Coasian ambition, the comparative institutional analysis of the world of positive transaction costs is a worthy challenge. It has made more headway in the theory of the firm than in the theory of consumer behavior, in antitrust than in regulation, in studying capitalism than in socialism, with respect to markets than bureaucracies, in more developed as compared with less developed countries, in organization theory than in political science, in business than in law.

If in the end transaction cost economics, as we know it currently, gets subsumed within a unified science of economic organization, that is no cause for regrets. That is progress.[16]

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[16] Indeed, that is already happening. Many of those who are currently engaged in the study of governance problems in which incomplete contracting and (implicitly if not explicitly) transaction cost issues are featured would not describe themselves as institutional economists. Labels are not the issue. Studying real phenomena in operationally engaging ways is what the exercise is all about.

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