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Journal of institutional and theoretical economics : JITE

Volume 149 / 1993 / Issue 1 / Journal Part / Article

Journal of Institutional and Theoretical Economics (JITE) 149/1 (1993), 73–87
Zeitschrift für die gesamte Staatswissenschaft

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by

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This paper compares the scope and theoretical character of two recent movements in applied economics: the New Institutional Economics, and Law and Economics. It criticizes the antitheoretical strain in the New Institutional Economics (and notes the absence of such a strain in Law and Economics), but it concludes that both movements are valuable and that they substantially overlap, with the differences between them being mainly matters of emphasis and vocabulary. (JEL: B41, K1)

The award of the 1991 Nobel Memorial Prize in Economic Sciences to Ronald Coase on the eve of this tenth anniversary meeting dedicated to the new institutional economics is, and not only for those of superstitious bent, a happy augury for the future of this school of economic thought. Not only is Coase the most illustrious economist associated with your school; he is also one of its strongest supporters. At a previous one of these meetings he said, "Modern institutional economics is economics as it ought to be" (COASE [1984, 231]). That was eight years ago but I do not think he has changed his mind. Coase is also one of the founders of the "new" (that is, post-antitrust) law and economics. He was honored as one of four founders of the movement at the first meeting, in the spring of last year, of the American Law and Economics Association. Apart from the enormously influential role that his article *The Problem of Social Cost* (COASE [1960]) has played in the evolution of economic thinking about law, Coase has been a member of the faculty of the University of Chicago Law School for more than a quarter of a century, was the second and a long-serving editor of the *Journal of Law and Economics,* and was for many years director of the law school's thriving law and economics program.

Coase may therefore be said to stand at the intersection of the new institutional economics and the law and economics movement, and it is natural to suppose therefore that there is considerable overlap between these movements – even, perhaps, that they are the same thing. There is an overlap, but not an

* My thanks to Gary Becker, William Landes, Lawrence Lessig, Kenneth Scott, and Steven Shavell for very helpful comments on a previous draft.

identity, although I think the differences are ultimately rather unimportant because both fields are parts of economics and economics is increasingly a single field, utilizing a common paradigm in Thomas Kuhn's sense, and because many of the apparent differences between the new institutional economics and law and economics are differences merely in emphasis and vocabulary. This paper elaborates these points.

I.

The adjective "new" implies that there was a previous institutional economics, and of course there was,[1] and in fact still is.[2] The best known of the original institutionalists, Thorstein Veblen and John R. Commons, flourished in the early decades of this century. The most famous of their living epigones is John Kenneth Galbraith. Another is Willard Hurst, professor emeritus of law at the University of Wisconsin Law School. Maybe Ian Macneil of Northwestern University Law School can be placed in their camp too. (I shall return to Hurst and Macneil.) The leitmotif of the old institutional economics was the rejection of classical economic theory. I have not read extensively in the literature of the old institutional economics but what I have read confirms Coase's dismissive characterization: "without a theory they had nothing to pass on except a mass of descriptive material waiting for a theory, or a fire" (COASE [1984, 230]). I tried recently to read Willard Hurst's magnum opus, a massive tome on the history of the lumber industry of Wisconsin (HURST [1964]). I didn't get far. The book is a dense mass of description – lucid, intelligent, and I am sure scrupulously accurate, but so wanting in a theoretical framework – in a perceptible *point* – as to be virtually unreadable, almost as if the author had forgotten to arrange his words into sentences.

The old institutional economics rejected classical economic theory, the theory of Adam Smith; the new institutional economics, in some versions anyway, rejects or at least severely questions neoclassical economic theory, as expounded for example by Paul Samuelson. The reasons for this rejection are both methodological and political. In order to facilitate mathematical formulation and exposition, neoclassical economic theory routinely adopts what appears to be, and often are, from both a physical and a psychological standpoint, highly unrealistic assumptions: that individuals and firms are rational maximizers, that information is costless, that demand curves facing firms are infinitely elastic, that inputs and outputs are infinitely divisible, that cost and revenue schedules are mathematically regular, and so forth. The unrealism of the assumptions drives a wedge between economic theory and the economic system

[1] For an excellent summary, see HUTCHISON [1984].

[2] See, for example, HODGSON [1988]. Despite its title (*Economics and Institutions: A Manifesto for a Modern Institutional Economics*), the book is highly critical of what I am calling the new institutional economics.

that the theory is purporting to describe and explain. Since the reality does not fit the theory, it is natural, though surely not inevitable, for theoreticians to want to alter the reality. So – and here is where the political objection to neoclassical theory originates – because conditions in the real world never satisfy the theory's specifications for an efficient allocation of resources (price equal to marginal cost, no externalities, no second-best problems, markets complete, etc.), neoclassical economic theory becomes a recipe for public interventions – antitrust laws, pollution-control laws, compulsory schooling, laws regulating employment, public utility and common carrier regulation, and much else besides. With every deviation from perfect competition labeled "market failure" and such deviations everywhere, it is hard to retain a robust faith in unregulated markets. This is disquieting to economic liberals (now more likely to be called conservatives), and it is no surprise therefore that they dominate the new institutional economics. (There is of course an influential strain in traditional conservatism of suspicion of all theory.)

The new institutional economists, however, are not against economic theory *tout court*. This is the vital difference between them and the old institutionalists. Some of them, like Coase, want to go back to the earlier, simpler, looser, nonmathematical theory of Adam Smith.[3] Being thus unworried about mathematical intractability, they are happy to relax the more austere assumptions of neoclassical theory. Others, like Oliver Williamson, have reservations about specific aspects of the neoclassical theory, such as the assumption of utility maximization[4] or of costless information (and consequent omniscience), but they want to enrich rather than to abandon neoclassical theory. Coase's famous articles on transaction costs are better regarded as correcting errors, induced by collectivist leanings, in neoclassical theory than as jettisoning that theory. Some new institutionalists do not want to change economic theory at all, but merely want to extend it so that it can be applied across the full range of institutions in diverse cultures and epochs.[5]

Rejection of economic formalism, or of a certain version of that formalism, is the negative side of the new institutional economics. The positive side is the study of – institutions. Yet it may be difficult at first to see how an interest in

[3] Coase believes that there will be a day for mathematical economics – but not yet. "Once we begin to uncover the real factors affecting the performance of the economic system, the complicated interrelations between them will clearly necessitate a mathematical treatment, as in the natural sciences, and economists like myself, who write in prose, will take their bow. May this period soon come" (COASE [1991, 12]).

[4] Coase, too, rejects the assumption that people are rational maximizers (COASE [1984, 231], COASE [1988, 4]). At the conference, Coase took sharp issue with my characterization of his methodological views. However, I believe that my characterization is amply supported in his published writings. See POSNER [1992b].

[5] For a notable recent example, see EGGERTSSON [1990]. EGGERTSSON [1990, 6] is careful to distinguish his approach from that of the new institutionalists who question the rational-choice model.

institutions could distinguish the new institutional economics from all but the most abstruse mathematical economics. For below those dizzying heights all economists believe that they are engaged in the study of institutions, the institutions of the economic system broadly or narrowly understood. Theorists of "perfect competition" think they are studying an institution called the price system or the market. But the new institutionalists, like their predecessors the old institutionalists, think of institutions in a grittier sense. They study not "the market," but the concrete institutions that enable markets to work – for example, the rules of the Chicago Board of Trade; or long-term contracts in the uranium industry; or how public utilities set rates; or the terms on which diamonds are offered to dealers; or the common law system of property rights in animals; or the operation of restrictive covenants in a city without zoning laws; or the governance structure of conglomerate corporations; or how problems of trespassing animals are handled by ranchers in a particular county; or the emergence of the nation state; or even the function of the engagement ring in the marriage "market" (BRINIG [1990]). 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.

Having been classified with the new institutional economists (BASU and JONES [1987, 2 n. 4]), I must be very careful in my criticisms. I do not doubt for a moment that there is a place in economics for case studies guided by informal theory – an apt description, in truth, of much of my own work – and therefore for the new institutional economics. But I reject any suggestion that the new institutional economics ought somehow to displace the rest of microeconomics. The view that it should is bound up with a dislike of abstraction, a sense that it falsifies reality. Abstraction does imply a departure from descriptive realism, but, as Milton Friedman and others have long argued, a theory is not necessarily false just because the assumptions on which it rests are unrealistic (i.e., false in detail). The idea that it is rests on a misunderstanding of the purpose of theory, at least of the kind of theory that we find in economics. The purpose is not to describe the phenomena being investigated; it is to add to our useful knowledge, mainly of causal relations, and for that purpose an unrealistic theory may be quite serviceable – may in fact be essential. Suppose the question is whether imposing an excise tax on cigarettes will result in a higher price and a lower output. (The answer, to a person innocent of economic theory, is not obvious; he might think the cigarette producers would swallow the tax whole, leaving price and therefore output unaltered.) One could model the problem by assuming that the cigarette industry is perfectly competitive, that its output is infinitely divisible, that the price of cigarettes is infinitely divisible, that consumers are perfectly informed, that prices can be changed instantaneously, and all the rest, and the model would predict that the excise tax would indeed result in a higher price and a lower output of cigarettes. So if the excise tax were imposed and the effects were as predicted, we would have some reason to think that we had discovered a causal relation, and that by further use of the model we might discover other causal relations.

The statistical counterpart to this observation is that it is possible to have a regression equation in which the coefficients on the independent variables are highly significant statistically even though the amount of variance explained by the equation is small. This would mean that although the regression equation was not a realistic model of the phenomena under study it had identified robust correlations. These correlations might well have causal significance and thus enable prediction and control – the most important goals of science. The abstractness of statistical methodology is in fact a precondition to its identifying causal relations. "Cause" and "effect" are themselves abstractions.

If, returning to the cigarette example, we wanted to know whether a cigarette excise tax would have a greater impact on purchases by educated than by uneducated people, we would need a more refined model. The refined model might for example partition the full price of cigarettes into the nominal price and the perceived health costs, and might assume that the latter, and therefore the full price, would be higher for educated consumers both because the costs of bad health are greater the greater one's income (there is more to lose if one is disabled by poor health) and income and education are positively correlated, and because the better educated have lower costs of information about health (so, among other things, the refined model would be relaxing the standard neoclassical assumption of perfect information). We might infer that the demand for cigarettes by educated consumers was therefore less elastic, and therefore that the excise tax would have a smaller impact on their purchases than on purchases by the uneducated, because for the educated the increase in the nominal price of cigarettes would represent a smaller increase in the full price, the health component of that price being unchanged. And again if the prediction turned out to be correct[6] we would have learned something about economic behavior, even though our model would remain highly unrealistic because it assumed infinitely divisible prices and outputs, etc. A model can be a useful tool of discovery even if it is unrealistic, just as Ptolemy's astronomical theory was a useful tool of navigation (and hence of "discovery" in another sense) even though its basic premise was false. Conversely, a full description, such as we find in Willard Hurst's book about lumber, can be useless.

We should be pragmatic about theory. It is a tool, rather than a glimpse of ultimate truth, and the criterion of a tool is its utility.[7] For all I know there is

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[6] For confirming evidence, see BECKER, GROSSMAN and MURPHY [1991, 240].

[7] I doubt that there would be much research in mathematics if mathematical discoveries seemingly of no utility when made (such as the discovery of non-Euclidean geometry in the nineteenth century) had not time and again been later found to have practical utility.

more truth in Hurst's book than in the entire economic literature on excise taxation. Only I cannot see the utility of Hurst's book.

Not only is it wrong to be against formal theory, it is wrong to suppose that formal economic theory is inherently interventionist. Maybe *bad* formal theory is, but that's a different point. Neoclassical theorists used to be pretty casual about proposing the use of public funds to correct "market failures," because the theorists assumed that public funds could be obtained through income taxation with no important misallocative effects. In a *theoretical* paper – not a case study of the Internal Revenue Service – Milton Friedman showed that there was no reason in theory to expect the misallocative effects of an income tax to be any smaller than those of an excise tax (FRIEDMAN [1976, ch. 3]).

Ronald Coase's great papers on the firm, on the pricing of services produced under conditions of declining average cost, and on social cost may have grown out of his practically oriented education (his undergraduate degree was in commerce, not economics) and out of case studies, such as his study of the Federal Communications Commission, in which the Coase Theorem was first adumbrated. But they are theoretical papers. They correct neoclassical theory, for example by adding transaction costs to the analytic framework (as George Stigler added information costs) and by showing that Pigou was wrong to suppose that the efficient way to correct a negative externality always is to tax or regulate the injurer. Coase has helped us to see that socialists deform economic theory in a socialist direction, requiring correction by economic liberals such as himself. It is a theoretical task.

And while it is right to distrust facile armchair theorizing in economics as in other fields, we should not overlook the *efficiency* of theory. The tortoise doesn't always beat the hare. Milton Friedman is the world's most influential economic liberal. He conducted famous institutional studies of incomes from professional practice and of the history of monetary policy in the United States, but those studies are peripheral to his significance for economic liberalism. That significance rests on his opposition to the progressive income tax, his insistence on the dependence of political liberty on economic liberty, his advocacy of the volunteer army and of a balanced-budget constitutional amendment, his criticisms of Keynesianism, his opposition to professional licensure and to paternalistic legislation such as the narcotics laws, and his proposals for education vouchers and for a negative income tax. It shows no disrespect to Friedman to point out that he has not conducted any detailed, painstaking case studies of the subject matter of his policy positions outside of the fiscal and monetary areas and professional licensure. He arrived at those positions as a matter of theory. If he had followed the case-study route exclusively his range would have been far narrower and economics would be immeasurably poorer as a result.

Here is another example of the fruitfulness of formal economic theory for the study of institutions. The household is an economic institution. It is as important to the economy as the firm is. It is also a nonstandard organizational form of the sort neoclassical theorists were apt to neglect. As they should not have done: the increased productivity of the household in recent decades as a result of the new labor-saving devices (not only kitchen appliances but also TV dinners), improved methods of family planning, better communications, and technological advances in home entertainment has been instrumental in enabling increased female participation in the labor force, which is transforming the economies of the wealthy nations. The leading student of household production, Gary Becker, is a formidable theorist. His basic theoretical tool, which institutionalists old and new tend to deride, is the assumption that man (and woman too, of course – and many a child) is a rational maximizer of his satisfactions. This assumption has guided fruitful empirical work by Becker and others, as well as bringing the household securely within the orbit of economic analysis. I do not think that we should regret the absence of case studies of individual families.

I have been purporting to describe the new institutional economics *en gross* but in fact I have been describing the conception of the field held by Ronald Coase and those who identify closely with him. Oliver Williamson has a somewhat different conception – perhaps different emphasis would be a better word – well summarized in papers that he has given at earlier seminars in this series.[8] His approach is more eclectic than Coase's, both methodologically and politically. Williamson has no objection in principle to formal models or to econometric studies, and indeed rather than hankering for a return to the informal approach of Adam Smith he wants to build a new model of economic behavior that will incorporate not only certain insights implicit in Smith and in the "old institutionalists" but also the insights of modern psychology and organization theory. He wants to apply the enriched model primarily to vertical integration, corporate governance, and long-term contracts. He does not appear to be particularly interested, as Coase is (though not to the exclusion of the subjects that interest Williamson – they are in fact central to Coase's emphasis on transaction costs), in formal markets (such as the securities and commodities exchanges), in property rights, in externalities, and in the critique of public regulation. (Many of Coase's own case studies have been of public agencies such as the British Post Office and the Federal Communications Commission.) Rather, the focus of his interest is on types of contract, broadly understood, that are remote from spot contracts, where the conditions for perfect competition – many buyers and sellers, full knowledge, standardized goods, and so forth – are most likely to be satisfied, or at least approximated. Williamson is interested in other words in contractual settings permeated by asymmetric

[8] WILLIAMSON [1984], [1985 b]. The fullest presentation of Williamson's approach is in his book *The Economic Institutions of Capitalism* (WILLIAMSON [1985 a]). On Williamson's own perception of the differences between his approach and that of Coase, see his interesting review of Coase's book *The Firm, the Market, and the Law* (WILLIAMSON [1989]). For illustrative discussions of Williamson's work, see ALCHIAN and WOODWARD [1988], DUGGER [1983], and ENGLANDER [1988].

information, bilateral monopoly, and uncertainty. Coase having directed the attention of the economics profession to the importance of transaction costs, Williamson seeks to move down the research agenda implicit in Coase's work by exploring the sources of high transaction costs and the contractual and organizational devices that businessmen have devised to overcome such costs. Other votaries of the new institutional economics, such as Douglass North, comparing the state to a long-term contract, try to use transaction-cost economics to enrich the study of political institutions.[9]

In his good-natured eclecticism Williamson provides a moderate alternative to Coase's stark, and many would say eccentric, rejection of the theories and the empirical methods of modern economics. But in the process of moderating Coase, Williamson has gone far toward collapsing the new institutional economics back into mainstream economics – which is fine with me. He believes that what he is doing is novel – that he is rejecting key assumptions of neoclassical economics, such as rational maximization, in favor of new concepts such as "bounded rationality," "asset specificity," "information impactedness," "dimensionalizing," and "opportunism," that he is drawing new disciplines into economics, notably law and organization theory,[10] and that he is redirecting economists' attention to institutions heretofore but little studied by them. But I am uncertain how much of this is new theory and how much of it is the repackaging of old theory in a new vocabulary that is not necessarily an improvement, in point of clarity or precision, over the old one. Does "bounded rationality," for example, mean more than that economic actors have and must (rationally) act on less than full information? 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. They can be – they have been – incorporated into economic theory without requiring an alteration in any fundamental assumptions, such as rational maximization; for rationality is not omniscience.[11] Asset specificity refers to the existence of specialized resources and hence to the problem of bilateral monopoly, and opportunism means taking advantage – sometimes of a temporary monopoly, sometimes of superior information ("asymmetric information"); in other words it is self-interest in settings in which private incentives cannot be relied on to promote

[9] See, e.g., NORTH [1984]. North's work is linked – at the level of subject matter, not method – with the branch of the old institutional economics, illustrated by the work of Gunnar Myrdal, that emphasized institutional obstacles to the development of backward countries. See GUNNARSSON [1991].

[10] "The three interdisciplinary legs of NIE [New Institutional Economics] are law (especially contract law), economics (with a relentless emphasis on economizing), and organization theory (which is responsible for greater behavioral content)" (WILLIAMSON [1985b, 190]).

[11] At the conference, Williamson was unable to give a useful definition of bounded rationality or indicate how the concept differs from that of rationality under positive information costs.

social welfare. These are old themes in economics too. The increasing use of game theory to address these problems comes out of – game theory, rather than out of anything special to the new institutional economics.

Unless I am mistaken, the novelty of Williamson's work is not in identifying new sources of "market failure" and domesticating them for theory; it is not in formulating a theory of transaction costs; it is in inviting economists' attention to a host of underexplored problems [12] 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. Williamson has argued for example that the decentralized organization characteristic of conglomerate corporations economizes on the costs of information, compared to the traditional hierarchical structure, and by doing so expands the span of corporate management's control. And there is now a rich literature showing how firms adapt to the uncertainty, and the resulting bilateral monopoly problems and opportunistic temptations, of long-term contracts.[13]

Well, but how much does this literature actually owe to the ideas of Williamson, or even for that matter of Coase? A clue to the answer lies in Williamson's rather warped view (as it seems to me at least) of "the applied price tradition at Chicago" (WILLIAMSON [1985 b, 189]). He commends the Chicago School for "its insistence that nonstandard forms of organization be examined through the lens of price theory," but criticizes it for encouraging economists to "focus on monopoly features," such as price discrimination, as explanations for these nonstandard forms (tying arrangements, vertical integration, and so forth) (WILLIAMSON [1985 b, 189–190]). The criticism overlooks the central role of information costs, not only in Stigler's pathbreaking essay on the economics of information but also in his theory of cartels – where costs of information are one great theme and opportunism is the other.[14] That is far from the only omission in Williamson's appraisal. The law and economics work at Chicago, work very much in the applied price tradition that Williamson properly associates with Friedman, Stigler, and their followers (among whom I count myself), is preoccupied with problems of uncertainty, bilateral monopoly, and opportunism and with how legal and economic institutions try to solve them.[15] None of this work is cited by Williamson even though law is one leg of the interdisciplinary triad on which the new institutional economics is said by him to rest.

[12] It is greatly to Williamson's credit that he was willing to discuss important issues in economics, for example the behavior of the firm, before the theoretical tools for addressing the issues had been developed. Theory has now caught up.

[13] For a recent contribution to this literature, see CROCKER and MASTEN [1991]. Benjamin Klein, a leader in this branch of the new institutional economics, provides a useful summary in KLEIN [1985].

[14] The relevant essays are in STIGLER [1968].

[15] For examples of papers in which those themes are particularly emphasized, see LANDES and POSNER [1976], [1978], [1985], and POSNER [1977].

Twenty years ago one had a definite sense that economics was a field divided into rival schools – the Harvard School, the Chicago School, the MIT School, the Cambridge School, and so on. That is much less true today. The biggest difference today is between theoretical economics and applied economics. New institutional economics à la Williamson is a form of applied economics not easy to distinguish, save in vocabulary, from the other forms; and the only word in the new vocabulary that I find useful is "opportunism."

II.

Since my topic is the relation between the new institutional economics and the law and economics movement, it is time that I took a closer look at that movement, though I can be brief. The roots of the movement go back very far – at least as far as Bentham, who applied economic theory to the behavior of criminals and to the methods of criminal punishment. Oliver Wendell Holmes, Louis Brandeis, and Learned Hand prepared the ground for the reception of economics into American law. By the 1950s, antitrust law, corporate law, public utility regulation, and federal taxation had been brought under the scrutiny of economics; representative figures are Aaron Director, Henry Simons, Donald Turner, and Henry Manne. In the following decade, papers by Coase, Becker, and Guido Calabresi ushered in the "new" law and economics, which emphasizes the application of economics to the central institutions of the legal system, including property, contracts, torts, criminal law, family law, civil and criminal procedure, damages and other remedies, common law rulemaking (with its emphasis on decision according to precedent), admiralty, restitution, and legislation. The years since 1970 have witnessed an expanding torrent of scholarly writing across the full range of law and economics, old (where a significant area of development has been the economics of bankruptcy) as well as new.[16]

The law and economics movement differs from the new institutional economics in that it has no, or at least very few, aspirations to change economic theory or economists' empirical methodology. Partly because of the nature of its subject matter, and partly because many of its practitioners have little formal training in economics or statistics, there is heavy use of informal theory and of case studies. But there is no opposition to mathematical modeling or econometric methodology; the latest generation of law and economics scholars is deep

[16] For a sense of the contemporary scope of the field, see POSNER [1992a]. On its growing influence on law, see LANDES and POSNER [1992]. In the pioneering generation, Alchian, Demsetz, and Tullock should also be mentioned. Current practitioners in the U.S. include Shavell, Easterbrook, Bebchuk, Priest, Polinsky, Fischel, Epstein, Kaplow, Ellickson, Schwartz, Sykes, Cooter, Kornhauser, Mnookin, White, Romano, McChesney, and many others. There is also a growing number of practitioners of law and economics in Europe and Japan.

into game theory. Nor, although some of the most prominent practitioners of law and economics are economic liberals in the tradition of Coase, Hayek, and Friedman, is there a political cast or agenda to law and economics, except insofar as any form of "bourgeois" economic analysis strikes left-wing radicals as political. The only thing that is distinctive about the movement, viewed as a movement in economics, is its subject matter; only viewed as a movement in law is it methodologically radical.

The subject matter of law and economics overlaps that of the new institutional economics at a number of points. Vertical integration, corporate governance, and long-term contracts are foci of law and economics, just as they are foci of the new institutional economics. Students of those practices whom Oliver Williamson clasps to his bosom, such as Benjamin Klein, Victor Goldberg, and Paul Joskow, are also and equally and rightly considered practitioners of law and economics. Historically oriented new institutionalists such as Douglass North and Thráinn Eggertsson exhibit the same deep interest in property rights as law and economics scholars.

The overlap between the two approaches is not total, and this leads to a difference not in theory but in theoretical emphasis. The new institutional economists' preoccupation with transaction costs leads them away from price theory toward theoretical concepts tailormade as it were for transaction-cost problems. (An exception is the concept of bilateral monopoly, which is traditional.) The broader subject-matter domain of law and economics is conducive to a more eclectic theoretical approach. When the economist or economically minded lawyer studies even so recherché a subject (from a traditional economist's standpoint) as criminal law, he is likely to rely heavily on the tools of price theory, since while criminal punishments are not prices they behave like prices. And so with studies of legal restrictions on the adoption market, or of the traffic in illegal drugs. Human-capital theory also plays a large role in law and economics, and may in fact explain some of the phenomena that particularly interest the institutionalists.[17]

Still, there is much overlap between law and economics and the new institutional economics, and yet when Williamson speaks of law as one of the legs of the interdisciplinary tripod on which the new institutional economics rests he seems not to be referring to the economic analysis of law, which is what "law and economics" means. Organization theory is distinct or at least somewhat distinct from economic theory, so one could see the two theories as separate legs of an interdisciplinary trunk. But it is very difficult to see how the application of economic theory to law could be a third distinct leg. And then as I have said Williamson does not cite the "new" law and economics literature out of Chicago, although it deals directly with the very topics that interest him the most. He seems to think that legal theory somehow distinct from economic theory can contribute to the new institutional economics – can be a separate leg

[17] As argued in BECKER [1990].

of the triad from economics – so naturally one of his favorite law professors is Ian Macneil, the noneconomic theorist of "relational" contracting.[18] Macneil believes that contract law has been too much concerned with spot contracts to the exclusion of contracts embedded in an ongoing relationship between the contracting parties.

This is music to Williamson's ears. But, unfortunately, though all too commonly when one is speaking of legal "theories" that lack a foundation in economics, Macneil's theory of contracts has very little content, so far as I am able to determine. He is in quest of "a *Grundnorm* recognizing the embeddedness of all exchange in relations" (MACNEIL [1985c, 542]). If this means that we must recognize the problems and opportunities that arise when parties have a continuing relation rather than merely meeting as strangers in a spot market, I agree. Such a relation may make contracts self-enforcing, because each party stands to lose if the relation terminates.[19] Conversely, it may create temptations to opportunistic breach – maybe one party's performance precedes the other's – or problems of bilateral monopoly, which can be acute in cases in which one party seeks modification of the contract, because the parties can deal only with each other. These are problems on which economics has a strong grip; so far as I am able to determine, neither Macneil nor any other "legal theorist" of contracts has anything to contribute to their solution.[20] If I am right that the new institutional economics and the law and economics movement are two sides of the same coin, Macneil's irrational hostility to the latter may explain his inability to make concrete contributions to the former. I do not think "irrational" is too strong a description of the following passage, which is representative: "the law-and-economics vision is one of two alternative visions of Hell in America. A realistic law-and-economics vision is simply a slightly milder, right-liberal mainstream Hell... An idealistic law-and-economics vision is a Hell of hedonistic markets governed, along with everything else in life, by private bureaucratic law unmitigated by any more due process than the private bureaucracies find it desirable to allow" (MACNEIL [1985a, 919]).

So the law leg of Williamson's triad collapses; and so far as the organization-theory leg is concerned, it seems to me to add nothing to economics that the literature on information costs had not added years earlier. I am therefore confirmed in my earlier suggestion that the new institutional economics, save

[18] On Williamson's high regard for Macneil, see WILLIAMSON [1985a, 68–73].

[19] As emphasized in the setting of the employment relation by EPSTEIN [1984].

[20] But the reader can judge for himself by reading Professor Macneil's review article, MACNEIL [1985b, esp. 523–524]. In his paper for this conference, WILLIAMSON [1993, 44–46] made some highly questionable statements about literalism in contract interpretation (that it is the bane of contemporary American contract law – the opposite would be closer to the truth) and about remedies in arbitration (that they are rarely monetary – in fact they are usually monetary, except in labor arbitration). I do not think he has found in Professor Macneil a reliable guide to the nature and problems of modern American contract law.

in Coase's version, is just – economics; and I have said that the law and economics movement is just economics too. When the new institutionalists study long-term contracts and corporate governance and vertical integration and property rights and the like, they are doing the same thing that the law and economics scholars do when they study the same subjects. In that area of inquiry, at least, the convergence between the two approaches is complete and I regret that Oliver Williamson's taste for neologisms has made the convergence more difficult to recognize than it should be.

But I do not want to end my survey and critique on so querulous and asseverative a note. What I have stated as assertions about the novelty (or lack thereof) and scope of the new institutional economics should really be viewed as questions, or at least as hypotheses. Although it seems to me that the new institutional economics is a matter very largely of selecting from the tool bag of modern economic theory those tools most apt for the study of the narrow though important range of problems, involving impediments to transactions, on which the new institutionalists have chosen to focus, maybe I am missing something. But there is much that I find puzzling. Let me recur to the issue of the new institutionalists' attitude toward rational utility maximization. Coase rejects this cornerstone of neoclassical theory but at the same time says that his own approach is guided by the assumption that people prefer more to less.[21] 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, but what if I have still another opportunity, worth 5? I will choose that. And so on until I have – maximized my utility. Maybe Coase or Williamson thinks that some cognitive or psychological defect in us prevents us from moving up the ladder of preferred alternatives in the manner that I have described. Maybe they think we have a quirk in the brain that makes us disregard opportunity costs, or fail to disregard sunk costs. But it is unlikely that they think this, or at least that Coase thinks this. The concept of opportunity costs is fundamental to the Coase Theorem, and the distinction between sunk and avoidable costs is fundamental to the public utility pricing literature to which Coase has made distinguished contributions.

All this said, I concede that there may be theoretical or methodological novelties that have simply eluded my understanding. But even if I am right in my suspicion that the new institutional economics is less a new body of theory or methods than a change in emphasis, a body of case studies, and a rejection of collectivist deformations of theory, I consider it a valid and valuable part of modern economics and I hope that it will continue to prosper and to grow.

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[21] "For groups of human beings, in almost all circumstances, a higher (relative) price for anything will lead to a reduction in the amount demanded. This does not only refer to a money price but to price in its widest sense." (COASE [1988, 4])

Zusammenfassung

Dieser Beitrag vergleicht Thematik und theoretischen Charakter zweier junger Strömungen in der Ökonomik: die Neue Institutionenökonomik und „Recht und Ökonomik". Es wird die atheoretische Linie der Neuen Institutionenökonomik kritisiert (und die Abwesenheit einer solchen Richtung auf dem Gebiet der „Recht und Ökonomik" festgestellt). Beide Strömungen sind jedoch wertvoll und weisen erhebliche Überschneidungen auf. Die Unterschiede bestehen im wesentlichen in der Schwerpunktsetzung und der Wortwahl.

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Journal of Institutional and Theoretical Economics (JITE) 149/1 (1993), 88–91
Zeitschrift für die gesamte Staatswissenschaft

The New Institutional Economics Meets Law and Economics

Comment

by

WERNHARD MÖSCHEL

I.

I have no real difficulties with POSNER's [1993] central thesis that new institutional economics and law and economics together form two sides of the same coin. Both are "just economics" (cf. PICOT [1992, 82]). Small differences in research areas and in the methodological approach are not really significant. Those difficulties I do perceive lie rather more in some of the nuances of Posner's contribution. These could give rise to fruitless disputation. I therefore want to submit some points of clarification and I assume that Richard Posner would agree with them.

1. The formulation of general propositions, the object of which is to describe reality, is only possible on the basis of some kind of theory and this must necessarily include some abstraction.[1] A map with a scale of 1 : 1 makes little sense, but that does not go to show that descriptive economic analysis is without merit. In this context, Posner refers to HURST [1964]. Firstly, such activities make possible the empirical verification of explicative theorems. Secondly and more important still, without proper appreciation of facts one cannot begin to make any kind of meaningful inquiry. At the beginning of any realization of knowledge, stands first the θαυμαζειν, that is to say – to marvel at the thing itself. COASE's [1988, 187] article on the development of the British lighthouse system is a good example.

2. On the basis of a given proposition, it is possible with a theory – in so far as the theory is workable – to arrive at a prognosis. But this is only half the picture; the other half comprises of its indicative utility, in other words its claim to truth or reality. Suppose my weather-frog starts to croak whenever the rates in Wall Street climb, this might be a useful theory for me, but if I am going to

[1] As Schumpeter points out, simplifying models are inevitable, if there should be any economies of thought, cf. SCHUMPETER [1955, 15–16].

invest other people's money on the market, then the need for a more discriminative theory seems inevitable.

3. The question as to what is "new" in economic research might be important for people who bestow positions, wealth or academic distinction. These are not our objectives in this seminar. Settling such questions is in any case usually difficult. We have all academically stood on the shoulders of others – in an endless chain. I would emphasize, though, that a number of aspects mark out what is new in research. Newness in research may concern the analytical tools, or the formulation of the proper question. Finally, its newness can be evaluated rather more in terms of the intellectual impact it has on the development of a scientific discipline. What I see as crucial in the new institutional economics theory is not so much the introduction of transaction costs as an assumption to be made in the calculation of decisions, or even the preoccupation with institutions, for old style institutionalists also raised questions about the latter. The core of the theory lies in establishing the connection between both. With this aspect in mind, Coase's now famous 1937 study extended methodological individualism – which also identified with the neoclassical theorists – to its ultimate consequence. In the meantime, I consider the influence of such thinking to have been quite considerable. Perhaps Posner's contrary thesis, that Coase is usually merely cited, rather than having any meaningful influence on others, is then a charming understatement. The qualification of Williamson's institutional economics as "applied economics" only convinces me that nothing is quite so practical as a good theory.

4. I agree with the picture Posner depicts of the law and economics movement. This however is subject to two caveats, which also concern the new institutional economics as well.

- I consider it both legitimate and profitable to analyze such phenomena as slavery, abortion, the death penalty in terms of economic categories and then to make normative recommendations which can be passed on to the legislative. But one needs to be aware that allocative and productive efficiency in this context is not necessarily the sole deciding factor.
- Economic analysis influences law. The same is also true in reverse. Whoever submits conflicts to a rule of law – as opposed to discretionary or political decision making – is only ever free to take that first step. When continued, one must consider the inherently autonomous nature of applying the law further (MÖSCHEL [1983, 49–50]). The legal prerequisites should be so formulated that it is still possible to assess them in a general-abstract manner. In addition the legal prerequisites of the norm should be so formulated that they can be recognized within the administration of justice. An economic research programme needs to take this into account if the results are to remain capable of being integrated in a rule of law system. This particular dimension seems to be neglected in the research agenda, a much heard and proudly proclaimed word.

II.

When I said, to begin with, that I have no difficulties with Posner's central thesis on the unity of economics, this does not rule out a degree of relativizing. Posner's view appears to be applicable if one interprets it in a fundamentalist manner – so to speak – in that the idea of a single and indivisible truth applies for economics as well. However the state of our knowledge in the year 1992 seems to me to make such claims rather fanciful. Posner for his part ends up drawing on numerous schools of thought and a plethora of theoretical starting points: old institutional economics, new institutional economics (be it in Coaseian or Williamsonian mode), the law and economics movement, neoclassical equilibrium theory, game theory, organization theory, the Chicago-, Harvard-, MIT and Cambridge schools. Other theories could be added, such as the market process theory and coordination or evolutionary market theory. The differing theories, quite dependent of the cognitive interest of those who make use of them, each have their own legitimacy. However, even today I do not see an omnipotent metatheory which is capable of embracing all the facets of previous theories and scientific thinking. This brings me to my final point, and it is outside the realms of Posner's paper. Is there a market for economic theories, which could be analyzed with the assistance of these theories themselves?

The aforementioned theories, each with their subdivisions, apply economic methods of analysis to institutions, which influence economic activity in the widest sense. They each seek to explain the coming into being, adaptation and functioning of institutions, and then to offer recommendations for their proper organization. There is, then, to that extent a degree of competition between each theory. This is characterized by specialization and in so far as the theories overlap by product differentiation as well. Hitherto specialization appeared in the context of market development to have been productive. In the case of new institutional economics this specialization also brought, in my opinion, an advancement in understanding of the theory as a whole. It is harder, though, to make an assessment of product differentiation, in other words the application of other terms, other analytical methods within the scope of a relevant "theory market." Is it that new terms are for ever dressing up the same problem; categories such as market failure, external effects, transaction costs, incomplete contracts, information costs, bounded rationality, asset specificity, information impactedness, opportunism, organization costs, incomplete information, asymmetrical information, unprotected and non-specified property rights? Could it be rather more of a strategic behavior, whereby good will in the market can be established merely with the assistance of product differentiation? Is this perhaps the manifestation of a vigorous competition as between ideas and to that extent productive? Or maybe this competition for ideas is even ruinous, in the sense that a breakthrough will never materialize as long as the scientific community is beavering away in too many places at the same time?

In this context the difficult question pertaining to the institutional structure of scientific advancement needs to be asked. However one proceeds – a static equilibrium concept can hardly offer a sense of direction.

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Journal of Institutional and Theoretical Economics (JITE) 149/1 (1993), 92–95
Zeitschrift für die gesamte Staatswissenschaft

The New Institutional Economics Meets Law and Economics

Comment

by

KENNETH E. SCOTT

As a commentator on this paper, I feel like I am in an impossible position, in two directions. As regards the author of the paper, you will note that this is a revised draft. I commented on the first draft, and the author of course took those comments to heart. As a result, the paper must now be perfect – what is there left to object to? Yet I realize I will be derelict in my duties to the conference if I do not try.

As regards the subjects of the paper, you will note that it is largely concerned with the views and contributions of Ronald Coase and Oliver Williamson. Under normal conditions, I would argue that in certain respects Richard Posner has misconceived their positions or misdescribed their views. Unfortunately, there they both are, sitting down the table a ways, and some people with a narrow point of view might feel that they have a comparative advantage over me in correctly representing their own thinking.

So what is an earnest and willing commentator to do? The anwer is obvious – take refuge in methodology. Having no alternative, that is what I shall do.

On this tenth anniversary occasion of a conference devoted to the new institutional economics (NIE), Posner is interested in what is distinctive about it and how it relates to law and economics in particular and neoclassical economics in general. For this purpose, he separates the NIE into two main camps, one associated with Coase and the other exemplified by Williamson.

The Coasean camp is seen by POSNER [1993, 76] as hostile to formal economic theory and econometrics, relying instead on case studies guided by informal theory. With that, Posner launches into a defense of the usefulness of formal theory, even when based on simplifying assumptions that do not accurately map reality, if it leads to verified predictions. It is hard for me to disagree, though not perhaps for Coase, who has noted his preference for insight into causation over testable predictions (COASE [1982]).

But Posner's argument, it seems to me, conflates the formal with the mathematical. Coase, it is true, does not speak in algebra or differential equations, a fact for which some of us lawyers are duly grateful. But that does not keep him from being a formal theorist, any more than it did Knight or Wicksteed.

I find it difficult to regard the Coase of "The Marginal Cost Controversy" (COASE [1946]) as not a practitioner of formal theory, though the article contains not a line of algebra or a single curve.

The basic distinction Posner is making, I believe, is between theoretical and applied economics, and Coase has done work in both. On the applied end, if one does empirical research, it might take the form of individual case studies at one end of the spectrum or the econometric manipulation of large databases at the other end. The choice of a particular technique will reflect the choice of subject, the cost of information, and the personal skills and preferences of the author. Why is the personal selection of one technique in one study to be construed as the "rejection" of all others, in some general sense?

So I think it goes a bit far to see Coase as bluntly rejecting modern economic theory.[1] He strikes me as not so much a school apart as someone very much a part of the neoclassical tradition who is extending it to some previously neglected questions (e.g., the boundaries of the firm) or correcting some previously accepted errors (e.g., Pigou's treatment of externalities as requiring a governmental solution). The introduction of transaction costs into formal analysis should be viewed as an addition to, not a repudiation of, neoclassical economics.

But now I am exactly where I did not wish to be – out on a limb, arguing with Posner by characterizing Coase. Either can saw off the limb, so I had better move hurriedly on.

When it comes to Williamson, Posner has a different point to make, relating the NIE to the law and economics movement rather than to the neoclassical tradition. He sees both law and economics and the NIE à la Williamson as applications of standard economics to particular sets of questions, with the NIE's focus being on a somewhat narrower or less eclectic set, limited to situations where transaction costs are high and parties devise various means of coping.

Posner has no methodological bone to pick with Williamson, who uses both mathematical and non-mathematical analysis. Instead, he criticizes the Williamson camp for not recognizing its affinity with much of the law and economics literature and integrating the two. (That would seem an observation that could cut both ways.)

Indeed, Posner seems to suggest that the contributions of the Williamson camp may lie more in terminology than substance. Does bounded rationality lead us in a different direction than the recognition of information costs? Do asset specificity and opportunism create different problems than those considered under the heading of bilateral monopoly?

So while Coase (but not Williamson) is seen as rejecting the use of neoclassical tools, not only for himself but for others, Williamson (but not Coase) is

[1] As did Posner in his conference draft – not this final version.

depicted as dispensing old wine under new labels. This assessment of the NIE is obviously not of one piece, but more importantly, is it correct?

I have already commented on Posner's perception of Coase, so let me turn to his appreciation of Williamson. POSNER [1993, 80] observes, for example, that "asset specificity" is not a new concept but just a neologism for the old problems of "bilateral monopoly." His point invites examination on two levels. The first is whether both terms have the same referents – do they encompass the same situations?

On the level of terminology, my sympathies are with the Queen of Hearts: everyone is free to give the words they wish to use whatever meaning they prefer, so long as they are clear about it. If the term is new but the content old, the charge is only that of commission of a minor nuisance – the infliction of unnecessary jargon.

But in fact I do not believe the concepts and the literature devoted to them to be the same. The traditional bilateral monopoly literature is concerned to find the optimal terms of a trade between a single buyer and a single seller; there is no particular interest in how they arrived in that position of mutual uniqueness. Asset specificity is interested in precisely that latter question: what *creates* relationships of bilateral monopoly? One of the answers is a significant degree of loss of value if the physical or human capital in question has to be shifted to a new use – a distinctly different issue.

But the second level is the more interesting one: has the treatment of bilateral-monopoly-type questions by Williamson and other new institutional economists drawn attention to them and carried them further? I think the answer is decidedly in the affirmative. Posner, for example, has stressed the high transactions costs (and lost opportunities for gain) often associated with strategic bargaining in bilateral monopoly situations, and the role of legal rules in avoiding them. But Williamson adds emphasis on the wide array of contractual and governance devices invented by parties who foresee vulnerability to expropriation of whatever quasi-rents are embedded in their initial deal.

Posner and Williamson may have similar starting points, in a sense, but they proceed in different directions. The distinction, to oversimplify a bit, is between examining the efficiency of the (common) law in mitigating bilateral monopoly and examining the efficiency of private ordering in anticipating and attenuating its costs.

So to Posner's indictment of Coase and Williamson I am inclined to render at the least a Scottish verdict – not proven. That brings me in conclusion to the Scot (not Scott) for whom he reserves his strongest words – Ian Macneil, he of relational contract fame. Is Macneil irrational? (POSNER [1993, 84]). Certainly he is irascible, and perhaps he is overly fond of the schizoid – a possibility on which I have speculated before.[2] But – no doubt fortunately, from the stand-

[2] See SCOTT [1988].

point of the conference organizers – limitations of time prevent me from extending these comments any further.

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