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# The New Institutional Economics: Taking Stock, Looking Ahead

OLIVER E. WILLIAMSON<sup>1</sup>

## 1. *Introduction*

I OPEN MY DISCUSSION of the new institutional economics with a confession, an assertion, and a recommendation. The confession is that we are still very ignorant about institutions. The assertion is that the past quarter century has witnessed enormous progress in the study of institutions. The recommendation is that, awaiting a unified theory, we should be accepting of pluralism.

Chief among the causes of ignorance is that institutions are very complex. That neoclassical economics was dismissive of institutions and that much of organization theory lacked scientific ambitions have also been contributing factors. As to progress, that is what most of this paper is about. There being many instructive lenses for studying complex institutions, pluralism is what holds promise for overcoming our ignorance.

Speaking for myself, I subscribe to Jon Elster's view that we work predominantly on partial mechanisms rather than general theories at this stage of de-

velopment (1994, p. 75). In consideration, however, of the "splendid plausibility of error" to which Lord Acton refers,<sup>2</sup> we need to sort the sheep from the goats. That is accomplished by asking each would-be theory to advance refutable implications to which the data are applied.

R. C. O. Matthews, in his presidential address to the Royal Economic Society in 1986, pronounced that "the economics of institutions has become one of the liveliest areas in our discipline" (Matthews 1986, p. 903). Such a pronouncement was a surprise to most of the profession. Hadn't institutional economics long since been relegated to the history of economic thought? Whence the vitality to which Matthews made reference?

Matthews' response was that the new institutional economics (NIE) turned on two propositions. First, "institutions do matter"; and second, "the determinants of institutions are susceptible to analysis by the tools of economic theory" (Matthews 1986, p. 903). The second of these is what distinguishes the NIE, it being the case that institutional economists of all kinds—old and new—are unanimous in the view that institutions matter.

Indeed, although both the older and newer styles of institutional economics

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<sup>2</sup> As quoted in Daniel Boorstin (1998, p. 281).

subscribe to many of the same good ideas, a progressive research program requires more. Kenneth Arrow speaks to the transformation as follows (1987, p. 734):

Why did the older institutionalist school fail so miserably, though it contained such able analysts as Thorstein Veblen, J. R. Commons, and W. C. Mitchell? I now think that . . . [one of the answers is in the] important specific analyses . . . of the New Institutional Economics movement. But it does not consist of giving new answers to the traditional questions of economics—resource allocation and the degree of utilization. Rather, it consists of answering new questions, why economic institutions emerged the way they did and not otherwise; it merges into economic history, but brings sharper [microanalytic] . . . reasoning to bear than had been customary.

There is no question that the NIE has grown in stature and influence over the fourteen years since Matthews' pronouncement. Initial skepticism has gradually given way to respect—it being the case that economists are very pragmatic people. Tell them something different and consequential about phenomena that are of interest to them and demonstrate that the data are corroborative: that will get their attention. The NIE has progressed not by advancing an overarching theory but by uncovering and explicating the microanalytic features to which Arrow refers and by piling block upon block until the cumulative value added cannot be denied.

The NIE, moreover, will not stand still. Even as institutional economics is being incorporated within orthodoxy, new opportunities and challenges await. Both unfinished business and new projects yet to be undertaken await the new millennium.

I begin with a sketch of four levels of social analysis, next turn to some of the good ideas out of which the NIE works, and then examine some of the applications to which the NIE has been put. Concluding remarks follow.

## 2. *Four Levels of Social Analysis*

It will be useful for purposes of perspective to consider the four levels of social analysis that are distinguished in Figure 1.<sup>3</sup> The solid arrows that connect a higher with a lower level signify that the higher level imposes constraints on the level immediately below. The reverse arrows that connect lower with higher levels are dashed and signal feedback. Although, in the fullness of time, the system is fully interconnected, I mainly neglect these feedbacks. The NIE has been concerned principally with levels 2 and 3.

The top level is the social embeddedness level. This is where the norms, customs, mores, traditions, etc. are located. Religion plays a large role at this level. Although Level 1 analysis is undertaken by some economic historians and other social scientists (E. C. Banfield 1958; Robert Putnam, Robert Leonardi, and Raffaella Nanetti 1993; Samuel Huntington 1996; and Victor Nee 1998), Level 1 is taken as given by most institutional economists. Institutions at this level change very slowly—on the order of centuries or millennia—whereupon Douglass North poses the query, “What is it about informal constraints that gives them such a pervasive influence upon the long-run character of economies?” (1991, p. 111).

North does not have an answer to that perplexing question, nor do I. The concept of “embeddedness,” both at the level of society and in the context of ongoing network relations, has been advanced to help explicate these issues (Granovetter 1985). The vast literature on culture (Paul DiMaggio 1994) is also pertinent. Neil Smelser and Richard Swedberg discuss these and related issues in their introduction to the *Handbook*

<sup>3</sup> This framework was first set out in Williamson (1998).

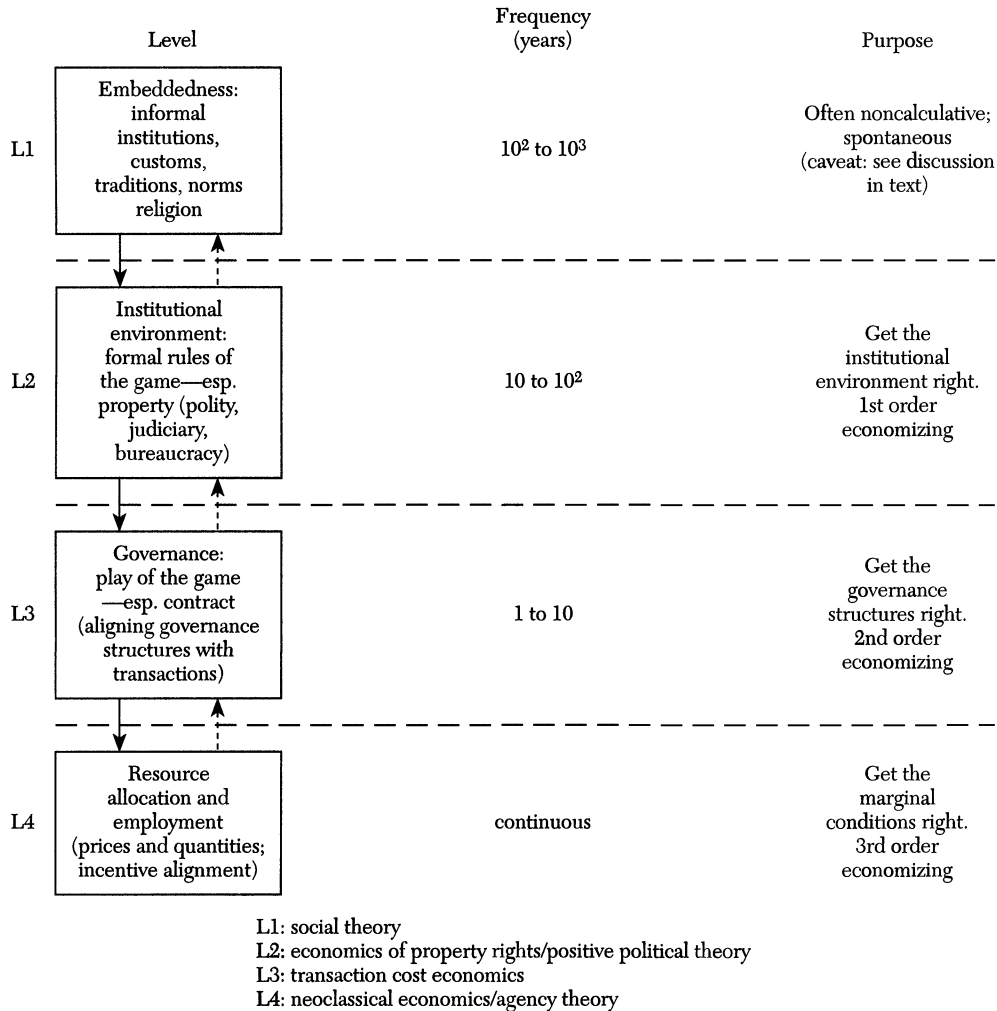


Figure 1. Economics of Institutions

of *Economic Sociology*, where they observe that different kinds of embeddedness—cognitive, cultural, structural, and political—should be distinguished, and conclude that “the concept of embeddedness remains in need of greater theoretical specification” (1994, p. 18).

An identification and explication of the *mechanisms* through which informal institutions arise and are maintained would especially help to understand the slow change in Level 1 institutions. I conjecture in this connection that many of these informal institutions have

mainly spontaneous origins—which is to say that deliberative choice of a calculative kind is minimally implicated. Given these evolutionary origins, they are “adopted” and thereafter display a great deal of inertia—some because they are functional (as with conventions); others take on symbolic value with a coterie of true believers; many are pervasively linked with complementary institutions (formal and informal), etc. Be that as it may, the resulting institutions have a lasting grip on the way a society conducts itself. Insular societies often take

measures to protect themselves against “alien values.”

The second level is referred to as the institutional environment. The structures observed here are partly the product of evolutionary processes, but design opportunities are also posed. Going beyond the “informal constraints (sanctions, taboos, customs, traditions, and codes of conduct)” of a Level 1 kind, we now introduce “formal rules (constitutions, laws, property rights)” (North 1991, p. 97). This opens up the opportunity for first-order economizing: get the formal rules of the game right.

Constrained by the shadow of the past, the design instruments at Level 2 include the executive, legislative, judicial, and bureaucratic functions of government as well as the distribution of powers across different levels of government (federalism). The definition and enforcement of property rights and of contract laws are important features.

Although such first-order choices are unarguably important to the economic productivity of an economy (Nathan Rosenberg and L. E. Birdzell 1986; Ronald Coase 1992; North 1994; Brian Levy and Pablo Spiller 1994; Mancur Olson 1996; Witold Henisz 1998) cumulative change of a progressive kind is very difficult to orchestrate. Massive discontent—civil wars (the Glorious Revolution; see North and Barry Weingast 1989), or occupations (following World War II), perceived threats (the Meiji Revolution), breakdowns (Eastern Europe and the former Soviet Union), a military coup (Chile), or a financial crisis (New Zealand)—will, however, occasionally produce a sharp break from established procedures. Rare windows of opportunity to effect broad reform are thereby opened. Such “defining moments” are nevertheless the exception rather than the rule. At least partly because of our primitive understanding,

the response to such opportunities is often one of “failure.” Absent such a window, major changes in the rules of the game occur on the order of decades or centuries. The European Union, for example, has been “in progress” for fifty years and is still in early stages of development.

What is often referred to as Positive Political Theory (PPT) is concerned with working out the economic and political ramifications of Level 2 features. To be sure, such research also has lessons for the normative design of better polities. Like the NIE of which it is a part, however, PPT is predominantly an exercise in positive analysis. The object is to better understand how things work—warts and all. The research product of PPT scholarship has been nothing less than auspicious, which has been good for both political science and the NIE.

Much of the economics of property rights is of a Level 2 kind. Such research flourished in the 1960s. A strong version of the argument is that “a private-enterprise system cannot function properly unless property rights are created in resources, and, when this is done, someone wishing to use a resource has to pay the owner to obtain it. Chaos disappears; and so does the government except that a legal system to *define* property rights and to *arbitrate* disputes is, of course, necessary” (Coase 1959, p. 12; emphasis added). Once property rights have been defined and their enforcement assured, the government steps aside. Resources are allocated to their highest value as the marvel of the market works its wonders.

This compact statement illustrates both the strength and the weakness of the property rights literature. The great strength of this literature is that it brings property rights to the forefront, where they belong, whereupon novel

property rights reasoning could be brought to bear in informative ways (Armen Alchian 1961, 1965; Coase 1959, 1960; Harold Demsetz 1967). The weakness is that it overplayed its hand. The claim, for example, that the legal system will eliminate chaos upon defining and enforcing property rights assumes that the definition and enforcement of such rights is easy (costless). Plainly, many transactions do not qualify (Coase 1960). Going beyond the rules of the game (property) to include the play of the game (contract) was needed. That is the opening through which the governance of contractual relations walked in during the 1970s.

This brings me to the third level, which is where the institutions of governance are located. Although property remains important, a perfectly functioning legal system for defining contract laws and enforcing contracts is not contemplated. Costless court ordering being a fiction, much of the contract management and dispute settlement action is dealt with directly by the parties—through private ordering. The need to come to terms with contract laws (plural), rather than an all-purpose law of contract (singular), is posed (Clyde Summers 1969; Ian Macneil 1974). The governance of contractual relations becomes the focus of analysis.

John R. Commons prefigured this work with his observation that “the ultimate unit of activity . . . must contain in itself the three principles of conflict, mutuality, and order. This unit is a transaction” (1932, p. 4). Not only does transaction cost economics subscribe to the idea that the transaction is the basic unit of analysis, but governance is an effort to craft *order*, thereby to mitigate *conflict* and realize *mutual gains*.

So conceived, a governance structure obviously reshapes incentives. To focus entirely on ex ante incentive alignment,

however, is a truncated way to study organization—especially if all complex contracts are unavoidably incomplete and if adaptation is the central problem of economic organization (Chester Barnard 1938; Friedrich Hayek 1945). Moving beyond the agency theory tradition of ex ante incentive alignment, transaction cost economics turns its attention—additionally and predominantly—to the ex post stage of contract.

This entails four moves: (1) to name and explicate the principal dimensions with respect to which transactions differ (thereby to uncover differential adaptive needs); (2) to name and explicate the principal attributes for describing governance structures (where each is defined by a distinctive syndrome of related attributes, whence markets, hybrids, firms, regulation, bureaus, non-profits, etc. differ in discrete structural ways); (3) to effect a discriminating match, according to which transactions are aligned with governance structures so as to promote adaptation of autonomous and cooperative kinds; and (4) to ascertain whether the predicted alignments are corroborated by the data.

The canonical problem for dealing with these issues is that of vertical integration, which is the issue posed by Coase in his classic 1937 article on “The Nature of the Firm.” As it turns out, any issue that arises as or can be reformulated as a contracting issue can be examined to advantage in transaction cost economizing terms. A huge number of phenomena turn out to be contractual variations on a common theme. What I refer to as second-order economizing—get the governance structures right—is realized at Level 3. The possible reorganization of transactions among governance structures is re-examined periodically, on the order of a year to a decade, often at contract renewal or equipment renewal intervals.

Such discrete structural analysis of governance is to be distinguished from the fourth level, which is the level at which neoclassical analysis works. Optimality apparatus, often marginal analysis, is employed, and the firm, for these purposes, is typically described as a production function. Adjustments to prices and output occur more or less continuously. Agency theory, which emphasizes *ex ante* incentive alignment and efficient risk bearing, rather than *ex post* governance, nonetheless makes provision for nonneoclassical complications, of which multi-tasking is one (Bengt Holmstrom and Paul Milgrom 1991).

Indeed, a still earlier (zero level) of analysis warrants remark: an evolutionary level in which the mechanisms of the mind take shape (Steven Pinker 1997). The application of these ideas to economics even now is beginning to reshape our understanding of human actors. Our evolutionary psychologist and cognitive science colleagues are vital to the exercise.

Finally, I should call attention to technology. As compared with technological innovation, the study of organizational innovation has been comparatively neglected. The NIE has attempted to rectify that—the idea being that “truly among man’s innovations, the use of organization to accomplish his ends is among both his greatest and his earliest” (Arrow 1971, p. 224). We cannot fail, however, to be awed by the profound importance of technological innovation (Robert Fogel 1999). Inasmuch as these two work in tandem, we need to find ways to treat technical and organizational innovation in a combined manner.

### 3. *Good Ideas*

The new institutional economics had its origins in good critics of orthodoxy

who believed that institutions were both important and susceptible to analysis. Feeling expansive, I would include six Nobel Laureates among the key figures: Kenneth Arrow, Friedrich Hayek, Gunnar Myrdal, Herbert Simon, Ronald Coase, and Douglass North—the last two being the first two presidents of ISNIE. But there are others. Armen Alchian has been an influential figure. So too has been research on organization theory, especially at Carnegie (some of it prefigured by earlier work by Chester Barnard)—where the names of Richard Cyert and James March join that of Simon. Alfred Chandler’s pioneering work in business history was also pathbreaking. Thoughtful contributors from the law, especially contract law, include Karl Llewellyn, Stewart Macaulay, Lon Fuller, and Ian Macneil. John R. Commons also brought original and important ideas to the study of institutional economics. The German Historical School was also concerned with related ideas (Erik Furubotn and Rudolf Richter 1997, pp. 34–35).

Among the key good ideas that I associate with the NIE are these:

*Human Actors.* If “nothing is more fundamental in setting our research agenda and informing our research methods than our view of the nature of the human beings whose behavior we are studying” (Simon 1985, p. 303), then social scientists should be prepared to name the key attributes of human actors. Both the condition of cognition and self-interestedness need to be addressed.

There is close to unanimity within the NIE on the idea of limited cognitive competence—often referred to as bounded rationality. Mind being a scarce resource, cognitive specialization has economizing consequences. Also, given cognitive limits, the complex contracts to which I referred earlier are

*unavoidably incomplete*. But while there is near-unanimity that complete contingent claims contracting is impossible, the appropriate way to model incomplete contracts remains controversial. Lack of agreement on the definition and operational import of bounded rationality is a major obstacle (Ariel Rubinstein 1998; David Kreps 1999).

Contractual incompleteness poses added problems when paired with the condition of opportunism—which manifests itself as adverse selection, moral hazard, shirking, subgoal pursuit, and other forms of strategic behavior. Because human actors will not reliably disclose true conditions upon request or self-fulfill all promises, contract as mere promise, unsupported by credible commitments, will not be self-enforcing.

But for opportunism, the courts would simply ask witnesses to “tell us what you know that is germane to our decision.” That is not, however, the way that testimony is taken. Witnesses are required to take an oath to “tell the truth, the whole truth, and nothing but the truth”: don’t lie; don’t conceal; don’t mislead. Inasmuch, moreover, as oaths are not self-enforcing, penalties for perjury remind witnesses that prevarication has consequences.

Still a third attribute of human actors warrants remark, and that is the capacity for conscious foresight. Indeed, as Richard Dawkins observes, it is the “capacity to simulate the future in imagination . . . [that saves] us from the worst consequences of the blind replicators” (1976, p. 200). Parties to a contract who look ahead, recognize potential hazards, work out the contractual ramifications, and fold these into the ex ante contractual agreement obviously enjoy advantages over those who are myopic or take their chances and knock on wood. The governance of contractual relations—the Commons triple of con-

flict, mutuality, and order to which I referred earlier—is centrally implicated.

*Feasibility*. Students of the NIE eschew hypothetical ideals—which work off of omniscience, benevolence, zero transaction costs, full credibility, and the like—and deal instead with feasible organizational alternatives, all of which are flawed. Coase (1964) and Demsetz (1969) were among the first to take exception with the asymmetric standards that were once used in the “market failure” literature—according to which markets are beset with failures whereas “omniscient, omnipotent, benevolent” governments (Avinash Dixit 1996, p. 8) would reliably administer efficacious remedies. As we all should have recognized (but needed to be told), all feasible forms of organization—government included—are flawed.<sup>4</sup>

What I have referred to as the remediable criterion is intended to rectify this asymmetric state of affairs. This criterion holds that an extant mode of organization for which no superior *feasible* alternative can be described and *implemented* with expected net gains is *presumed* to be efficient.

To be sure, public policy analysis becomes more complicated when analysts can no longer condemn extant modes because they deviate from a hypothetical ideal, full stop. The remediable criterion presses the public policy analyst to display a superior feasible alternative. If, moreover, a proposed feasible alternative cannot be costlessly implemented, then the costs of implementation are appropriately included in the net benefit calculus—which has major ramifications for the path dependency literature. Finally, grounds for rebutting the efficiency presumption need to be addressed—which brings in

<sup>4</sup> Dixit (1996) counsels the older public finance tradition to come to terms with government failures.



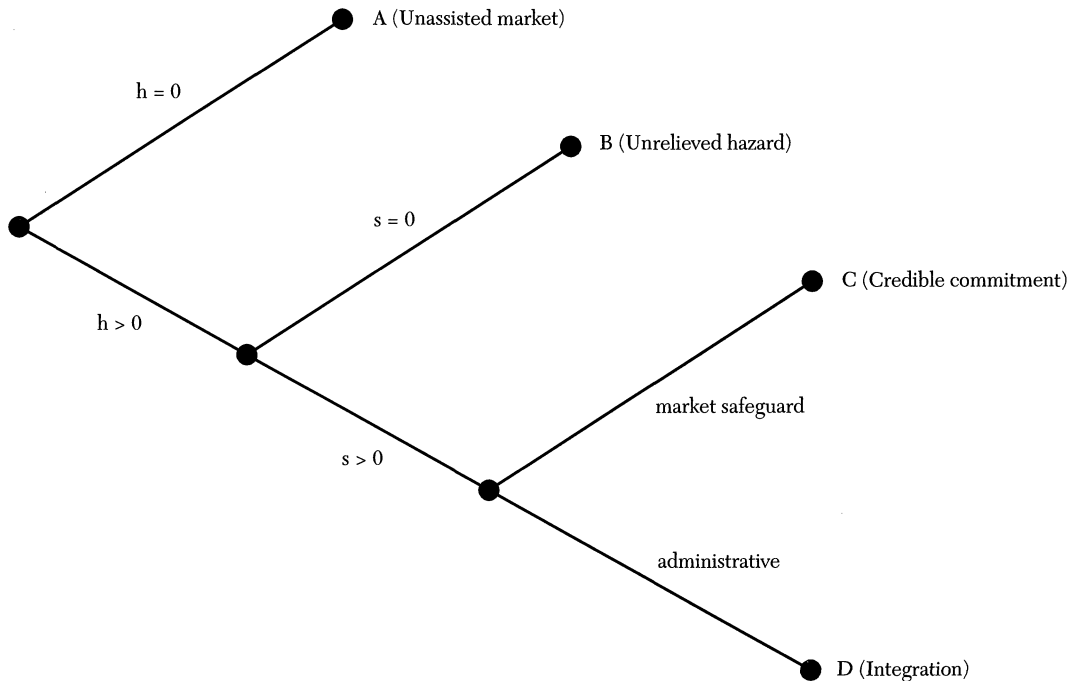


Figure 2. Simple Contracting Schema

politics (Williamson 1996, 1999). Absent rebuttal, the remediableness criterion stands as a reminder of the obvious: it is impossible to do better than one's best.

*Firms and Bureaus.* In addition to the nature of the human beings to which Simon referred, we need also to be self-conscious about the "Nature of the Firm," which was the title of Coase's classic 1937 article from which the NIE draws much of its inspiration. Arrow speaks to the fundamental importance of the theory of the firm, and to long-standing misconceptions thereof, as follows: "Any standard economic theory, not just neo-classical, starts from the existence of firms. Usually, the firm is a point or at any rate a black box. . . . But firms are palpably not points. They have internal structure. This internal structure must arise for some reason" (1999, p. vii).

The need was to get beyond the ana-

lytically convenient (and sometimes adequate) conception of the firm-as-production function (which is a technological construction) to consider the firm as a governance structure (which is an organizational construction) in which internal structure has economic purpose and effect. More generally, the need was to identify and explicate the properties of alternative modes of governance—spot markets, incomplete long term contracts, firms, bureaus, etc.—which differ in discrete structural ways. Because each generic mode of governance possesses distinctive strengths and weaknesses, there is a place for each yet each needs to be kept in its place. The logic of discriminating alignment to which I referred earlier applies.

In a heuristic way, the choice of governance structure moves from market to hierarchy through the sequence of moves shown in Figure 2 (where  $h$

denotes contractual hazards and *s* denotes safeguards).<sup>5</sup> This can be interpreted as a move from simple to complex. We thus begin with autonomous contracting, which is the ideal transaction in both law and economics: “sharp in by clear agreement; sharp out by clear performance” (Macneil 1974, p. 738). This discrete transaction paradigm comes under strain as contractual hazards appear. The inability of courts, for example, to verify what is common knowledge between the parties to an exchange (Williamson 1975, p. 30) could induce a move from interfirm to intrafirm organization. Other sources of contractual hazard include bilateral dependency (by reason of nonredeployable assets), weak property rights (especially intellectual property rights), undisclosed quality, health, and safety hazards, failures of probity, and the like. Such hazards compromise contractual integrity and give rise to contractual impasses, maladaptations, and investment distortions. Here, as elsewhere, inefficiency invites relief. Cost-effective hazard mitigation through added governance ensues.

Moving from less to more complex governance entails introducing added security features, reducing incentive intensity, and incurring added bureaucratic costs. Moving from simple (discrete) contracts to complex (incomplete long term) contracts is thus attended by a whole series of features: the length of the contract increases, penalties to deter breach are introduced, provision is made for added information disclosure and processing, and specialized dispute settlement mechanisms appear.

Additional mechanisms, to include the use of hierarchy to effect coordination and decide disputes by fiat, are

<sup>5</sup> A variant of Figure 2 originally appeared in my paper on “Public and Private Bureaucracies” (1999).

introduced when transactions are removed from the market and placed under unified ownership (the firm). Because added compliance and security features always come at a cost, more complex modes of governance are reserved for those transactions for which contractual hazards are especially difficult.

The public bureau, in this scheme of things, can be thought of as the organization form of last resort: try spot markets, try incomplete long-term contracts, try firms, try regulation, and reserve recourse to public bureaus for when all else fails (comparatively). Note that the common practice of condemning public bureaus because they have lower-powered incentives, more rules and regulations, and greater job security than a counterpart firm completely misses the point. These features have been *deliberately crafted* into the public bureau, thereby to make it better suited to govern some (especially difficult) transactions.<sup>6</sup> Vigilance is nonetheless needed—lest the public bureau be “overused.”

If transaction cost economics works out of variations on a few key themes, then this schema, which was initially devised to help explicate the governance of contractual relations in intermediate product market transactions, should also apply, with variation, to other classes of transactions. It does.

The four nodes are interpreted with reference to intermediate product transactions above. Consider final goods markets and government procurement transactions.

Transactions in final goods markets, where individual consumers are the

<sup>6</sup> This is a recurrent theme not only of the transaction cost economics literature but also of parts of the agency theory literature. See especially Holmstrom (1989) and Holmstrom and Milgrom (1991, 1994), where the benefits of low-powered incentives in firms are featured.

buyers, are similar but different. Node A transactions are generic and competitively organized. Node B transactions are rare. These correspond to “P. T. Barnum”—there’s a sucker born every minute—and other fly-by-night transactions. Node C is the credible commitment node. Branding in combination with reputation effects and product warranties appear. Also, for some natural monopoly transactions, public utility regulation serves credibility purposes. Node D is a nearly empty set. Economies of scale and of specialization are impediments to own-supply by consumers, although collective organization (consumer cooperatives) can be used to manage some transactions. (Many household services can be thought of as own-supply, but few fit comfortably within the schema.)

Government procurement transactions are also similar but different. Node A describes generic transactions to which, often, tedious technical specifications apply. Very few government transactions are of a Node B kind. Credibility mechanisms at Node C include the elaborate machinery of “administered contracting,” as with defense procurement (which transactions, however, are sometimes compromised by the shared interests of the government agency and the private supplier). And Node D is the public bureau, where for probity or political reasons the government chooses to manage the transaction itself.

Other applications of the schema include the employment relation (James Baron and Kreps 1999, ch. 4) and corporate finance (the choice between debt and equity). Some transactions, such as alliances and joint ventures, pose complications of a disequilibrium contracting kind (Williamson 1991) that are beyond the reach of the schema.

*Operationalization.* Many good ideas

are initially expressed as tautologies, which Coase has wryly defined as “a proposition that is clearly right” (1988, p. 19). Because good tautologies expand the mind and are hard to come by, they deserve respect. Lest, however, we slip into the speculations to which Wesley Mitchell once referred<sup>7</sup>—which is a fate that beset the older style institutional economics as well as the American Legal Realism movement—we need to ask what are the mechanisms through which a proposed theory operates and what are the refutable implications.

The effort to operationalize promising ideas has both theoretical and empirical parts. The theoretical often takes the form of a progression from informal to preformal, semi-formal, and fully formal modes of analysis—ideally acquiring value added in the process. Such an effort helps to sort the sheep from the goats. Nicholas Georgescu-Roegen had a felicitous way of putting it: although the “purpose of science is not prediction, but knowledge for its own sake,” prediction is nevertheless “the touchstone of scientific knowledge” (1971, p. 37). Would-be theories for which predictive content is lacking must eventually step aside (be set aside) for those for which the hard work of formalization and empirical testing are undertaken.

*Theory Development.* Formalization is vital to a progressive research agenda, but it sometimes comes at a cost. Thus although Simon once argued that “mathematical translation is itself a substantive contribution to theory . . . because it permits clear and rigorous reasoning about phenomena too complex to be handled in words” (1957, p. 89)

<sup>7</sup>“Speculative systems can be quickly excogitated precisely because they do not require the economist to collect and analyze masses of data, to test hypotheses for conformity to fact, to discard those which do not fit, to invent new ones and test them until, at long last, he has established a factually valid theory” (Mitchell 1945, p. 2).

and subsequently asserted that the “poverty of mathematics is an honest poverty that does not parade imaginary riches before the world” (1957, p. 90), provision also needs to be made for the possibility that core features of the theory are left out or obscured by the translation. There is, after all, such a thing as prematurely formal theory. Kreps speaks to the issues as follows (1999, p. 122):

If *Markets and Hierarchies* has been translated into game theory using notions of information economics, it is a very poor translation . . . In particular, mathematics-based theory still lacks the language needed to capture essential ideas of bounded rationality, which are central to . . . transaction costs and contractual form. Anyone who relies on the translations alone misses large and valuable chunks of the original.

What is referred to as the “property rights theory of the firm,” which had its origins with Sanford Grossman and Oliver Hart (1986) and has subsequently been developed by Hart and John Moore (hence the reference to the GHM model), *relates to* but differs significantly from the transaction cost economics setup (as presented, for example, in Williamson 1985, 1991). It is similar in that it deals with the make-or-buy decision through a setup where contracts are incomplete (by reason of bounded rationality), mere promise cannot be used to overcome noncontractibility (by reason of opportunism), and parties to the contract are bilaterally dependent (by reason of asset specificity). These commonalities notwithstanding, there are also major differences.

Some of these differences are attributable to simplifications that invariably attend formal modelling. Ideally, core features of the verbal argument and the mechanisms through which they work are made more precise in the process of formalization. Such a case can be made

for the property rights theory of the firm, which is a major intellectual achievement that has spawned a growing literature on the formal modelling of incomplete contracts.<sup>8</sup> As Kreps suggests, however, valuable chunks are missing. In the spirit of full disclosure (honest poverty), I focus on these.

The most consequential difference between the TCE and GHM setups is that the former holds that maladaptation in the contract execution interval is the principal source of inefficiency, whereas GHM vaporize *ex post* maladaptation by their assumptions of common knowledge and costless *ex post* bargaining. The upshot is that all of the inefficiency in GHM is concentrated in the *ex ante* investments in human assets (which are conditional on the ownership of physical assets).<sup>9</sup>

This shift from *ex post* maladaptation (the hazards from which vary with the condition of asset specificity and the disturbances to which a transaction is subject) to *ex ante* investment distortions matters. For one thing, GHM makes very limited contact with the data<sup>10</sup> whereas (as discussed below) TCE is an empirical success story. Related (*ex post*) governance and (*ex ante*) investment differences are the following:

<sup>8</sup> The January 1999 issue of the *Review of Economic Studies* is entirely devoted to recent contributions, critiques, responses, and extensions upon the GHM model.

<sup>9</sup> Bounded rationality enters this setup in a very peculiar way: parties who are unable to write complete contracts *ex ante* are nevertheless able to anticipate *ex ante* what decisions will be taken *ex post*, contingent on state realizations (Kreps 1999, pp. 123–25). In effect, the GHM setup is one of *selective* unbounded rationality: “not everything that is logically consistent is credulous” (Kreps 1999, p. 125).

<sup>10</sup> That the data relevant to GHM are so limited and inaccessible explains why there has been “no formal testing of the property rights approach” (Hart 1995, p. 49)—although the “inside contracting system” (John Buttrick 1952) is an approximation to (and its failures could be interpreted as a partial contradiction of) GHM.

- (1) The TCE rendition of the make-or-buy decision between successive stages (A and B) asks whether A and B should be separately owned and operated or if the ownership and operation of these two stages should be unified. If independent, then each stage appropriates its net receipts (high-powered incentives obtain) but maladaptation problems can arise during contract execution. If unified, then the two stages are managed coordinately through hierarchy. (Maladaptation problems are thereby relieved; incentives are lower-powered; and added bureaucratic costs arise.) By contrast, GHM view vertical integration in a directional way: either A buys B or B buys A, and it matters which way this is done. That is because common ownership under GHM *does not imply unified management*. Instead, each stage (in all configurations—A and B are independent; A buys B; B buys A) appropriates its net receipts. This last is a very unusual condition, in that unified ownership is normally thought of as a means by which to effect cooperation.<sup>11</sup>
- (2) TCE maintains that each generic mode of governance—spot market, incomplete long-term contract, firm, bureau, etc.—is defined by a syndrome of attributes to which distinctive strengths and weaknesses accrue. Specifically, TCE holds that alternative modes differ in incentive intensity, administrative controls (to include auditing, accounting, and transfer pricing), access to the courts, and informal organization (to include politick-

ing). GHM assume that incentive intensity, administrative controls, and informal organization are unchanged by ownership and that courts are irrelevant (because of costless renegotiation). None of the physical asset utilization and transfer pricing distortions that I associate with the “impossibility of selective intervention” (Williamson 1985, pp. 135–40) thus occur under the GHM setup.

- (3) TCE examines a wide range of ex post devices for infusing credible commitments into contracts and applies this reasoning to a wide set of transactions. Variations on this theme include hybrid modes of organization (Scott Masten 1996, Part III), exchange agreements and other uses of hostages to support exchange, the organization of work, the organization of labor and human resources more generally, corporate governance, regulation (and deregulation), public bureaus, and project financing. Because GHM is a property rights and property rights only construction (Holmstrom 1999), it relates to some of these issues not at all and others very selectively (Hart 1995; Hart, Andrei Shleifer, and Robert Vishny 1997).

GHM is nonetheless a pathbreaking contribution and has set the formal modelling of incomplete contracting in motion. New formal models of incomplete contracts which are closer in spirit to TCE include the treatment of procurement by Patrick Bajari and Steven Tadelis (1999), which focuses on the incentive and ex post adaptation differences between fixed price and cost plus contracting. Also, the recent paper by Susheng Wang and Tian Zhu (2000) employs the idea that alternative modes

<sup>11</sup> The inside contracting system referred to in note 7 supra is in the spirit of GHM organization.

of governance work out of different contract law regimes (Williamson 1991). And Gene Grossman and Elhanan Helpman (1999) appeal to the added bureaucratic costs of unified as compared with market governance in their assessment of alternative modes for producing differentiated consumer products. More veridical treatments of incomplete contracting are thus in progress and still more are in prospect.<sup>12</sup>

*Empirical.* Some scoff at prediction, evidently in the belief that prediction is easy. Also, since everyone knows that “it is easy to lie with statistics,” what useful purpose is served by empirical testing? My experience is different: prediction is a demanding standard, which is why so many would-be theories remain excogitated speculations; and corroboration is difficult, which explains why few predictions are tested.

Because, however, good theories are rarely fully developed at the outset, the theory and the evidence are often interactive. As Alan Newell observes (1990, p. 14):

Theories cumulate. They are refined and reformulated, corrected and expanded. Thus, we are not living in the world of Popper. . . . [Theories are not] shot down with a falsification bullet. . . . Theories are more like graduate students—once admitted you try hard to avoid flunking them out. . . . Theories are things to be nurtured and changed and built up.

Good but underdeveloped ideas are evidently like good but underdeveloped minds: both are precious things. Because development is costly, promising theories, like promising graduate students, are admitted only if they cross a threshold. Once admitted, theories (and

graduate students) are progressively built up—moving from less formal to more formal stages of development. Finally, as with promising graduate students, we do not hold on to cherished theories indefinitely: some do flunk out. Specifically, theories that remain tautological or yield predictions that are contradicted by the data must make way for theories that yield predictions for which the data are corroborative.

Empirical applications of transaction cost economics got under way in the U.S. in the 1980s and have grown exponentially since: the number of published studies exceeds 500 and involves social scientists in Europe, Japan, India, China, Mexico, South America, Australia, New Zealand, and the list goes on. It could have been otherwise, but the theory and evidence display a remarkable congruity (Scott Masten 1995, p. xi). Recent empirical surveys include Howard Shelanski and Peter Klein (1995), Bruce Lyons (1996), Keith Crocker and Masten (1996), and Aric Rindfleisch and Jan Heide (1997).

Not only has this research been broadly corroborative of the predictions of transaction cost economics, but the importance of risk aversion to commercial contracting has been placed in doubt (Douglas Allen and Dean Lueck 1999). To be sure, transaction cost economics, like everything else, will benefit from more and better empirical work. I have no hesitation, however, in declaring that the NIE is an empirical success story. Paul Joskow concurs: “this empirical work is in much better shape than much of the empirical work in industrial organization generally” (1991, p. 81). Those who have done this modest, slow, molecular, definitive work deserve enormous credit.<sup>13</sup>

<sup>12</sup> For an earlier formal treatment (of a reduced form kind) akin to Bajari and Tadelis, see Michael Riordan and Williamson (1985). The recent paper by Hart and Moore, “On the Design of Hierarchies” (1999a), also makes express provision for organization.

<sup>13</sup> The recurring regularity is this: more complex modes of governance appear as contractual hazards build up—where bilateral dependency, due

4. *Phenomena*

The NIE is predominantly concerned with Levels 2 and 3 of the four levels of social analysis shown in Figure 1. These are the levels of the institutional environment and the institutions of governance, respectively. Between them, they cover a lot of ground.

The formal features of the institutional environment—the laws, polity, judiciary, bureaucracy—are crucial in examining the development of nation states (North and Weingast 1989) and for making intertemporal comparisons within and cross-national comparisons between nation states. Indeed, this last has come to be a growth industry to which many economists who are only slightly associated with the NIE have made contributions. It is nonetheless noteworthy that the NIE has done much of the pioneering work in this area.

To repeat, any issue that arises as or can be posed as a contractual issue can be examined to advantage in transaction cost economizing terms. Examples for which contractual issues are evident at the outset include contracts for intermediate products, for labor, for final goods and services, for the rental or lease or purchase of land, equipment, and buildings, for professional services, for marriage, and the list goes on. Even, moreover, if contractual features are not immediately evident from the outset, many issues can be reformulated so as to disclose their contractual qualities, the choice between debt and equity,

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to asset specificity in any of its forms (physical, human, site-specific, dedicated assets, brand, and temporal), in combination with disturbances that beset contracts during the contract execution interval are responsible for many of these hazards. Working, as it does, out of noncontractible human asset investment distortions, GHM cannot lay claim to these same empirical successes (Michael Whinston 1997; Holmstrom 1999).

the oligopoly problem,<sup>14</sup> and the multinational corporation being examples (Peter Buckley and Mark Casson 1976; Hubert Gatignon and Erin Anderson 1988).

Many public policy issues, moreover, turn jointly on the combined use of Level 2 and Level 3 reasoning. In the area of privatizing telecommunications, for example, Levy and Spiller examine the institutional environments in five countries through a comparative contractual lens in which issues of credible contracting are featured (1994, 1996). The recent study of reforming urban water systems by Claude Menard and Mary Shirley (1999) likewise makes clear that ownership is not determinative but needs to be examined in conjunction with the support, or the lack thereof, of the mechanisms of governance. Again, issues of credible contracting are salient. The same is true of commercial contracting in Vietnam (John McMillan and Christopher Woodruff 1999).

Broad reach notwithstanding, the NIE is not and does not pretend to be an all-purpose construction, as the reform of economies of Eastern Europe and the former Soviet Union illustrate. Thus Coase in his Nobel Prize lecture observed that (1992, p. 714):

The value of including . . . institutional factors in the corpus of mainstream economics is made clear by recent events in Eastern Europe. These ex-communist countries are advised to move to a market economy, and their leaders wish to do so, but without the appropriate institutions no market economy of any significance is possible. If we knew more about our own economy, we would be in a better position to advise them.

Two years later, North, in his Nobel Prize lecture, expressed similar precautions.

<sup>14</sup> Of the various ways in which it can be posed, its contractual nature becomes more evident when it is posed as a problem of reaching and enforcing a cartel agreement (Williamson 1975, Chap. 12).

Thus even if we are confident that “polities significantly shape economic performance because they define and enforce the economic rules,” whereupon “an essential part of development policy is the creation of polities that will create and enforce efficient property rights,” there is the further problem that “we know very little about how to create such polities” (North 1994, p. 366).

Real-time events, however, cannot be put on hold. Hard choices have to be made. Economic reform in Russia is an example.

The team of Maxim Boycko, Andrei Shleifer, and Robert Vishny responded to the perceived need to give shape to the reform with the recommendation that the Russian economy should be privatized quickly and massively. Considerations of both Realpolitik and economic theory were invoked in support of this recommendation.

There being widespread agreement that “political influence over economic life was the fundamental cause of economic inefficiency” [in Russia], Boycko, Shleifer, and Vishny (1995, p. 11) declared that:

. . . the principal objective of reform was . . . to *depoliticize* economic life. . . Privatization fosters depoliticization because it deprives politicians of the opportunity to allocate goods. . . The goal of privatization was to sever the links between enterprise managers and politicians. . . There was no other way to achieve restructuring and efficient operation of firms.

The two strategic actors in this reform program were the official bureaucracy, which was viewed as “the enemy to be fought at all costs” and the stakeholders—managers, employees, and local governments. The Boycko et al. team “consistently and generously recognized stakeholders’ claims, and thus ensured their eventual support of privatization” (Boycko, Shleifer, and Vishny 1995, pp. 13–14).

This political prescription for massive and rapid privatization was reinforced by the economic theory of the firm on which the Boycko et al. team relied. Specifically, they appealed to the aforementioned work by Grossman and Hart (1986), which views ownership as a system of control rights and treats the appropriate assignment of property rights as determinative (Boycko, Shleifer, and Vishny 1995, p. 13). Upon privatizing state-owned enterprises, therefore, effective restructuring by the new stakeholders would presumably follow (op cit, p. 150). In the confidence that the future would take care of itself, the mass privatization program that was begun in the spring of 1992 had purportedly reached a “triumphant completion” in June 1994 (op cit, p. 8), by which date two-thirds of Russian industry was privately owned.

Had the Boycko et al. team consulted the new institutional economics, a more cautious and selective program of privatization with greater attention to implementation would have resulted. Consider first the literature on franchise bidding for natural monopoly, where the property rights approach and the governance approach reach very different conclusions.

The property rights approach to the problem of natural monopoly is to conduct an *ex ante* bidding competition and award the right to serve the market to the group that tenders the best bid (Demsetz 1968; George Stigler 1968; Richard Posner 1972). Very much in the spirit of Boycko et al., the future will take care of itself once the assets have been privatized in this way.

That sanguine view does not withstand scrutiny if serious *ex post* implementation problems are in prospect. Under the governance approach, the award of a monopoly franchise needs to be assessed comparatively. This entails



looking ahead and uncovering ex post contractual hazards, thereafter working out the ramifications for alternative modes of governance (Williamson 1976, pp. 79–91). Because franchise bidding works much better for some natural monopoly industries than others (Williamson 1976, pp. 102–103), the use of franchise bidding will be reserved for those industries where comparative net benefits can be projected—but not otherwise. Privatization, it turns out, is not an all-purpose solution (Victor Goldberg 1976; George Priest 1993).

Although privatizing an entire economy is a much more ambitious undertaking than privatizing a natural monopoly industry, the key lessons nevertheless carry over. Specifically, privatizing needs to go beyond the ex ante award stage to include an examination of possible ex post implementation problems and, in consideration of the *differential hazards*, to proceed selectively.

Recall, moreover, that the NIE operates at two levels. Upon moving from the level of governance to that of the institutional environment, the rules of the game come under review. The Levy and Spiller (1994, 1996) study of privatizing telecommunications in five countries reveals that the decision to privatize and the nature of privatization turn critically on the condition and quality of judicial independence, the division of powers between the executive and legislative branches, the competence of the regulatory bureaucracy, and contractual safeguards. Whether and how to privatize telecommunications should therefore be made conditional on these features.

As Bernard Black, Reinier Kraakman, and Anna Tarassova detail in their paper on “Russian Privatization and Corporate Governance: What Went Wrong” (1999), the “triumphant completion” of privatization in Russia was a premature

verdict. Thus, although privatization was evidently a success for small firms, it was deeply problematic and attended by massive corruption in others. But for undue reliance on ex ante property rights reasoning, some of these problems could have been anticipated by looking ahead and examining the hazards of ex post implementation. Greater appreciation for the shortfalls of the institutional environment in Russia would have led to more cautious pronouncements (Anders Aslund 1995). Whether added respect for the rules of the game (to include an appreciation for the limited efficacy of Russian law enforcement) would have resulted in rule improvements in Russia could be disputed. Arguably, however, the effort to reform Russia would have proceeded in a more modest, slow, molecular, deliberative way.

None of this is to suggest that the NIE could have done it all. The Boycko et al. team made heroic efforts. My claim is much more modest: the NIE is informative and should be included as part of the reform calculus.

### 5. *Concluding Remarks*

The new institutional economics is a boiling cauldron of ideas. Not only are there many institutional research programs in progress, but there are competing ideas within most of them. With reference to history, for example, we see North (1990) and Avner Greif (1999) pursuing complementary but separate agendas. The institutions of embeddedness (Level 1) are an important but underdeveloped part of the story. Within transaction cost economics we distinguish between governance and measurement branches. The attributes of mixed ownership modes (alliances, joint ventures, franchising, and the like) as well as the mechanisms for supporting credible contracting between autonomous

firms are incompletely worked out. Incomplete contracting of semi-formal and fully formal kinds differ in consequential ways, although the gap has been closing. Evolutionary economics of selectionist, population ecology, and ontogenetic kinds are in progress. Path dependency is a real and important condition, but its interpretation is actively disputed. The merits of privatization are real but are not uniform and need to be assessed with reference to both the rules of the game and the play of the game. The firm is variously described in technological, contractual, and competence/ knowledge-based perspectives. How best to describe human actors is still unsettled, although evolutionary psychology holds promise. Politics is judged with reference to a hypothetical ideal by some (North 1990) and in comparative institutional terms by others (Williamson 1999). Efficiency arguments have mainly prevailed over power interpretations because the latter are tautological, but power issues refuse to go away. Bureaucracy remains a poorly understood condition no matter what lens is brought to bear. Private ordering approaches to contract have made progressive headway, but legal rules remain important and their relation to private ordering is incompletely worked out. Positive political theory has made major conceptual advances, but an overarching understanding of politics does not appear imminent. And the list goes on.

The upshot is that, its many accomplishments notwithstanding, there is a vast amount of unfinished business—refinements, extensions, new applications, more good ideas, more empirical testing, more fully formal theory. I conclude that the new institutional economics is the little engine that could. Its best days lie ahead. Who could ask for more?

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