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A Nobel Prize for Governance and Institutions: Oliver Williamson and Elinor Ostrom

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ABSTRACT *This paper reviews the 2009 Nobel Prize in Economics jointly awarded to Oliver Williamson for his work on governance in organizations and the boundaries of the firm, and to Elinor Ostrom for her work on the governance of common pool resources. We review the careers and the research contributions of Williamson and Ostrom to the theory and analysis of economic institutions of governance. Both winners of this Prize for 'economic governance' are thoroughly deserved, yet like the Hayek–Myrdal Prize of 1974 their respective approaches, methods and findings are almost diametrically opposed. Williamson offers a top-down contracts-based solution to the incentive problems of opportunism in corporate governance, whereas Ostrom offers a bottom-up communication-based solution to the governance opportunities of community resources. We offer some critical comments on Williamson's analytic work and discussion of the potential for further application of Ostrom's case-study based experimental methodology. We conclude with a suggested third nominee to make better sense of how these two great scholars' works fit together, namely George Richardson.*

1. Introduction

The *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* (often referred to as the Nobel Prize in Economics) was jointly awarded in 2009 to Oliver Williamson and Elinor Ostrom for the study of institutions of governance: Williamson for work on institutions to overcome opportunism in firms with asset specificity; Ostrom for work on institutions of governance of common pool resources. This paper reviews their contributions to the New Institutional Economics of governance from a pluralistic economic perspective. As well as explaining what they did and why they won the prize, we offer some critical analysis of implications for economy theory and research. Specifically, we argue that Ostrom's method is the more general and that this award should be seen as a boost for the behaviourally founded, evolutionary-institutional

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approach to economic analysis as a branch of complex systems theory. Like Vernon Smith, Thomas Schelling, Friedrich Hayek and Herbert Simon before her, Ostrom's approach offers an interdisciplinary and methodologically complex path forward in the study of evolving institutions of economic coordination.

Governance entails using rules/institutions to influence, if not completely control, the behaviour of other agents. Economic governance systems are necessary to ensure that those who make agreements to buy or sell goods or flows of services, or about how particular common pool resources should be used, achieve the outcomes that have been agreed. Economic governance is inefficient if: sellers fail to receive timely payment or have to offer more than they agreed in order to extract payment; outputs are not delivered to buyers in the amounts and at the times that were agreed; or common pool resources are depleted despite the existence of agreements designed to prevent this from happening. The literature on economic governance therefore focuses on how people discover, establish and enforce particular rules/institutions that support production and exchange.

Markets are one class of governance structure but, as Coase (1937, 1960) pointed out, other classes are possible and in some contexts may be more efficient. Coase won the 1991 Nobel Prize for his seminal work on governance, which recognized the costs of using markets (transaction costs) and thereby explained why firms could be preferred as governance structures: when prediction is difficult, the direction of employees by managers as contingencies arise may be cheaper than formal contracts for the supply of goods or services as a means of determining what is produced and by whom. Unlike Goodhart (1975, p. 4, n. 6) and Loasby (1976, p. 65), Williamson (1975, 1985) did not recognize the implication in Coase's analysis that the firm is an institution that, like money, provides a means for deferring commitment in the face of uncertainty. Instead, he used the transaction cost notion to explain the boundaries of the firm in relation to the properties of the particular assets held by the firm and the individual opportunities for exploitation that arise. As regards common property, the Coasian solution for its governance is private property rights. Ostrom's (1990) work on the emergence of rules/institutions for the governance of common property showed that if local knowledge, monitoring and punishment can be applied to common property, the Coasian solution of privatization may not be the most efficient one—a point previously advanced by Friedrich Hayek (Nobel Prize 1974), that is also an implication of the work of Reinhard Selten (Nobel Prize 1994) on equilibria in repeated games.

The economic problem Ostrom and Williamson address is resource governance involving many people, with the coordination problems and opportunities this presents. But their new theories of economic governance offer very different perspectives. Williamson's theory is a top-down contract-based analysis that presumes people ultimately cannot trust one another. He explains the modern firm as an evolved hierarchical structure of contracts as a solution to this problem of opportunism in the context of asset specificity. His is a Hobbesian solution. Ostrom's theory is a bottom-up analysis that presumes people can form collective agreements at a local level and that the possibility of local enforcement of these agreements by the same people who formed them enables members of

a community to trust each other. For Ostrom, the problem of governance in shared resources is not trust and control, but rather the efficacy of communication and institutions of community formation. Ostrom uses case studies and experiments to show how the governance problems of collective resources can be resolved through *ad hoc* bottom-up institutions of rule formation. Hers is a Lockean or Burkean solution. The polar extremes of these governance solutions to the coordination problem of using shared resources rather signals that this is an area of work in progress, so this is probably not going to be the last Nobel Prize to be awarded in this domain of institutions of governance.

The present paper is structured as follows. Section 2 reviews Williamson's contribution to the problems of firm governance via contractual mechanisms in the context of asset specificity and principal–agent opportunism. Section 3 examines Ostrom's contributions to the emergence of institutions through the interaction of user-communities to arrive at functioning governance institutions associated with shared (or common pool) resources. Section 4 concludes by considering the respective models of agency and roles of trust in the formation of governance institutions, along with a suggestion for how their different views might be reconciled via the work of George Richardson and several others.

2. Oliver Williamson: Economic Strategies to Limit Guileful Behaviour

2.1. Contracting for Output versus Hiring and Managing Employees

Williamson's work focuses on the set of contractual arrangements through which economic activities are organized. It is through these arrangements that the legal boundaries of firms are defined. His work covers contractual relationships between firms, bureaucracies and independent agents that result from doing deals in markets, and relationships inside organizations that are shaped not merely by the contractual terms under which employees join them but also by hierarchical reporting arrangements. The term 'relationships' indicates that the parties involved in transactions deal with each other for significant periods of time during which there is potential for unexpected and/or feared events to occur and prospects of significant loss if the transaction does not unfold in the hoped-for manner. This is very much a real-world perspective: one-off transactions in which both parties can instantly see their payoffs and little is at stake are uncommon. Because of the potential losses from a misjudged transaction, would-be transactors need to be able to work out whether or not it is safe to agree to a particular kind of deal.

Dealing with another party opens up scope to benefit from specialization, but if that seems altogether too risky there remains the option of trying to undertake the activity in-house. Unless the advantages of specialization and the division of labour are to be sacrificed, it is impossible to avoid the question of whether it is wiser to contract with others to have them supply labour services under one's direction, or to contract for the supply of particular forms of output. Either way, markets are used: the labour market, if supply is 'internalized'; the product market, if supply is 'outsourced'. Williamson's (1975, 1985) key

contribution is to provide a theory of the circumstances in which contracting for goods and services will be seen as problematic and consequently be internalized unless potential trading partners can make 'credible commitments' that dispel concerns about the scope for transactions to fail. He also shows how organizational structures can be a key means to reduce the risks associated with internalizing production.

Williamson's analysis can be viewed as using ingredients from contributions that have already been recognized via three Nobel awards but he weaves them together in a novel way with inputs of his own. From the 1991 recipient Ronald Coase, he takes the basic idea that internalization of activities can be preferable to contracting for them in markets due to markets being costly to use. However, Williamson attempts to go beyond Coase by theorizing about the circumstances in which internalization will be the preferred strategy. Like the 2001 winners, Akerlof, Spence and Stiglitz, he recognizes that the dispersed nature of information can be a major cause of market malfunction. But whereas they focused on particular technical notions, such as adverse selection and moral hazard, Williamson offers a much broader, less truncated view of what drives behaviour when there is asymmetric access to information: he proposes the notion of 'opportunism' in the face of 'information impactedness'. Although his focus on devious uses of information advantages overlaps with the 2001 Nobel Laureates, it is better seen as having its roots outside economics, in Chester Barnard's (1938) analysis of the challenge that managers face in winning authority from their subordinates, and the notion of 'sub-goal pursuit' in contributions to organization theory such as that of March & Simon (1958). Moreover, whereas the 2001 prize signified the importance of unavailable information, Williamson also seeks to recognize the impact of cognitive shortcomings by drawing on the work of the 1978 winner, Herbert Simon, and his notion of bounded rationality.

The essence of Williamson's theory of the conditions under which market failure is likely to be anticipated and lead to internalization involves the simultaneous presence of four conditions, namely, bounded rationality, opportunism, small numbers of alternative trading partners, and asset specificity. The logic linking them together is as follows.

In a world of bounded rationality, transactions can become problematic if disputes arise about what the state of the world actually is or whether what was promised for delivery is actually being delivered. If human decision makers had unlimited capacities to gather and process information and to formulate and solve problems, they could devise and negotiate complex contracts that left no room for surprising outcomes: they would be able to cover all possible contingencies and nothing that occurred would be unexpected. In the real world, however, contracts will tend to be incomplete because transactors fail to anticipate some eventualities and attempt to avoid incurring the costs of trying to think of possible eventualities and negotiate over them. The finite cognitive capacities that Simon sought to encapsulate via his bounded rationality term thus take the problem of market transacting out of the neoclassical realm of decision-making in the face of a complete list of risks and into the realm of what Donald Rumsfeld famously called 'unknown unknowns': in the face of unexpected events, one party to a transaction may do something surprising, inflicting unexpected costs on the

other transactor. For example, if a customer's business is doing unexpectedly well, a supplier might threaten to hold up supplies in order to achieve an *ex post* improvement over the terms of the existing supply contract. Not only this, but a decision-maker's capacity to monitor what is going on is limited by finite attentive capacity: multiple workers are hard to supervise simultaneously, while even a single subordinate may be able to employ sleight of hand (or tacit knowledge) to do things that escape a supervisor's notice. Hence, even if particular situations have been anticipated, it may be difficult to judge whether they have actually eventuated.

Bounded rationality would not be a problem for the working of contractual relationships if parties to a transaction could rely upon their trading partners not to make claims that are hard to verify, involving guileful use of any information advantages they possess—in Williamson's words, if trading partners could be relied upon not to act with opportunism in the presence of information impactedness. In the absence of opportunism, gaps in contracts would be dealt with in good faith, with both sides trying to ensure that they agreed something that was fair and reasonable rather than trying to maximize their own returns regardless of the costs they inflicted on the other party. Williamson (1985, p. 64) makes no claim that all economic actors are prone to behave with opportunism, merely that some people are opportunistic some of the time. The trouble is that opportunists are by nature hard to identify *ex ante* so the wise transactor treats all possible trading partners as potential opportunists unless there is a good reason to regard them as likely to have an incentive to refrain from using information advantages as a cover for self-serving behaviour.

One general deterrent to would-be opportunists is the ability of the other transactor to switch to alternative trading partners in the event that opportunistic behaviour is detected, hence Williamson's 'small numbers' condition for market failure being expected. However, between his key 1975 and 1985 books Williamson came to realize that a transactor who was dealing with a monopolistic supplier or monopsonistic customer would have little reason to worry about falling foul of the trading partner's opportunistic tendencies if they could easily redeploy their assets to the production of other outputs. In other words, aside from the possibility of a general lack of demand for output due to a recession, it is 'asset specificity' that is the key determinant of business risks. If money has been sunk into particular assets that have few alternative uses, the chances of recovering it will be limited if the assets cannot earn the expected return in their intended use. By contrast, if an asset can be used for many different purposes, it does not matter if there are only a few trading partners in respect of any one of its uses as there is scope for playing off many different categories of transactors against each other. It should be noted here that care is required when labelling an asset as 'specific'. For example, a large photocopier machine may only be able to produce photocopies but it is not specific to a particular kind of business and it can be readily put in a truck and moved, so it does not present a case of asset specificity. Therefore, we should not be surprised to observe that photocopiers are frequently leased rather than owned by users. By contrast, a blast furnace is only useful in steel production and is highly immobile, as are other parts of a steel mill, so asset specificity applies. Steel production also inherently presents a small numbers

problem due to energy costs requiring it to be technologically integrated: there will be major costs of re-heating steel that goes cold due to a dispute somewhere along the production chain. Steel production thus seems to have the ingredients that promote the vertical integration that is observed in practice despite the theoretical possibility of writing contracts for the delivery of molten steel or red-hot steel slabs.

Where a transactor is worried about potential for contractual failure, a potential trading partner may be able to create a situation in which it would not be in their interest to behave with opportunism. As well as examining the reputation damage that would arise if opportunism were discovered and publicized, Williamson (1985) considers the role of hostages being used to make transactions work: a firm can offer a hostage by making a highly specialized investment whose returns would be ruined if it were seen to be engaging in opportunism. Rather than seeing transactors as calculating whether or not they can trust each other, he prefers not to use the term 'trust' at all and to focus on the perceived credibility of commitments that are put forward as counterweights to temptations towards opportunism.

It is the importance that Williamson assigns to opportunism that makes his approach to transaction cost economics different from Coase (1937) and takes the economics of organization into the territory of business and workplace ethics. Such a dark view of the nature of some business and workplace behaviour is something we might prefer to avoid by arguing that transaction costs can arise purely due to bounded rationality and hence that some internalization choices have nothing to do with fears of opportunism. But it turns out that Williamson was right to assign such a key role to fears of opportunism.

Internalization seems to provide a simple, flexible way for boundedly rational managers to deal with surprises. If activities are internalized, a manager decides what should be done and then simply gives workers a new set of directions for what they need to do within their loosely specified contracts of employment. There is no need to seek bids from rival suppliers and negotiate terms for new contracts to cover the new situation. However, an appeal to the flexibility and simplicity of running an internalized supply chain begs the question of why, rather than vertically integrating, firms may not be willing to agree to a supply contract that is as loosely specified as a typical employment contract. Such a supply contract might set ranges of outputs, prices and delivery rates and permit the customer to vary requirements and leave it to the supplier to decide how to respond given the agreed schedule of acceptable combination. It might also permit the supplier to vary prices, within the agreed boundaries, as the latter's own input costs varied or production difficulties were encountered. If firms opt for vertical integration rather than using such contracts to obtain supplies from other firms we might suspect this is due to fear that agreeing to a loosely specified contract is rather like signing 'blank cheques': such deals could turn out to be an expensive mistake due to the other party succumbing to temptations to engage in opportunistic behaviour. Without fears of opportunism, fuzzy contracts will be close substitutes for internalization in terms of coordination costs and have the advantage of requiring less finance to be raised by a firm in order to deliver a particular volume of end-stage output. Only if one has no worries about the possibility of

opportunism is it safe to sign a 'blank cheque'; otherwise, loosely specified contracts will only be acceptable if they offer superior prospects for monitoring and auditing behaviour, as may be the case with employees in one's own organization.

2.2. Works and Career

Williamson was born in 1932 and was awarded his first degree in 1955. As he explains in a very useful autobiographical account in his book *The Mechanisms of Governance* (1996, Ch. 14), he graduated in engineering and management from a combined programme offered by Ripon College and MIT's Sloan School of Management. After graduating, he worked for two years as a project engineer for the US Government in Washington, DC. This was a formative experience since it gave him the opportunity to witness the workings of a large bureaucracy and the behaviour of research and development sections of large firms. His second degree was an MBA from Stanford University, where he began his PhD. At that time, Stanford's business PhD programme was not strong so he began to focus on economics and had the good fortune to receive critical encouragement for his early efforts at original thinking from his micro-economics teacher, Melvin Reder.

While Reder's reaction to his work convinced him he was capable of research, it was his officemate, Charles Bonini, who recommended that Williamson should transfer his PhD studies to the Graduate School of Industrial Administration at the Carnegie Institute of Technology (now Carnegie-Mellon University), from where Bonini had recently arrived. Williamson took Bonini's advice and was awarded his Carnegie PhD in 1963. He regards his time at Carnegie as the most important event in his intellectual development, with much of his thinking being inspired by James March's 'playful remark' that 'managers maximize slack' (Williamson, 1996, pp. 24, 351). However, although his key mentors at Carnegie included Cyert, March and Simon, the pioneers of a behavioural approach to the theory of the firm, Williamson (1996, p. 351) comments that '[W]hile I was greatly attracted to behavioral economics, I was never entirely persuaded. Even granting that "satisficing" is more descriptively accurate than "maximizing", satisficing is also a cumbersome concept and difficult to model.' As a result, rather than moving toward simulation methods, he heeded advice from economists working in Carnegie's 'other strand', particularly Allan Meltzer and John Muth, to opt for a conventional formal modelling approach in order to think problems through thoroughly and avoid myopic conclusions.

Williamson's first academic position was as an assistant professor in economics at the University of California, Berkeley (1963–1965). Because of his management background, he was assigned the teaching of industrial organization, the field in which he was to make his major impact. From Berkeley, he moved to the University of Pennsylvania, achieving promotion from Associate Professor to Professor in 1968. His rapid rise is easy to understand: he made his mark swiftly, with multiple articles in each of the *American Economic Review*, *Journal of Political Economy* and *Quarterly Journal of Economics* between 1962 and 1968. He had also contributed periods of external service with a number of bodies. Of these, the most significant for his subsequent direction as an economist

was his role at Special Economic Assistant to the Assistant Attorney General for Antitrust at the US Department of Justice (1966–1967), for it was a role that involved working with lawyers. Without really noticing what he was doing, he later came increasingly to try to reformulate into comparative institutional terms any economic problem that he encountered, a tendency that was pointed out to him by Michael Spence (Williamson, 1996, pp. 362–363).

Despite subsequently making major service contributions—including two stints as chair of department and major editorial roles with the *Journal of Law and Economics* (co-editor 1983–2003), the *Journal of Economic Behavior and Organization* (associate editor, 1979–2002) and the *Bell Journal of Economics* (associate editor/editor 1974–1977, 1979–1981)—Williamson consistently published at a prodigious rate. As well as his enviable list of journal articles, his publishing tally includes five books, six edited books and two book collections of his own articles. However, the most striking thing about his publications list, and a significant indicator of his impact, is the sheer number of his articles that have been reprinted, many of which have been reprinted four or more times. Of the latter, the standout paper is his (1979) article 'Transaction costs economics: the governance of contractual relations', which so far has been reprinted in 14 collections.

Williamson served a quarter of a century at the University of Pennsylvania before moving to Yale in 1983 as Gordon B. Tweedy Professor of Economics of Law and Organization. In 1988, however, he returned to where he had held his first appointment, the University of California, Berkeley. Initially this was as Visiting Professor of Economics and Transamerica Professor of Business Administration but later that year he took up his current position as Edgar F. Kaiser Professor Emeritus of Business, Economics and Law. He has travelled the world as an invited speaker (including a keynote address to the 7th Joseph Schumpeter Society Conference in Vienna in 1996) and to receive honorary doctorates (ten, so far).

The range of reading that Williamson draws upon in his work is remarkable: not merely does he take inspiration from a range of disciplines but within economics he refers to contributors as diverse as Hayek, Commons and Marx. At times, however, one gets a sense that careful product differentiating is going on in his writing, for some central themes in obviously related precursor ideas somehow fail to get prominent treatment in his key works. For example, both his 1975 and 1985 books include works by Harvey Leibenstein in their bibliographies but readers who seek to find discussions of X-inefficiency will find neither the term nor Leibenstein's name in the indexes of either book. This is despite the fact that Leibenstein (1966, p. 407) was talking about the significance of incomplete job contracts in relation to workers' choices of effort levels long before Williamson was developing his transaction cost economics. Similarly, despite referring to the work of two of his mentors, Richard Cyert and James March in both of these books, Williamson's readers will struggle to find him considering opportunism in relation to organizational slack or satisficing. Neither book includes index entries for these central themes from the behavioural theory of the firm even though they point to potential for the distribution of returns to be adjusted or productivity to be increased when organizations come under pressure.

Despite this, via its central assumption of bounded rationality, transaction cost economics clearly *is* descended from the work on the behavioural theory of the firm that Williamson came to know about at first hand from Simon, Cyert and March during his doctoral studies. Williamson (1996, p. 27) sees the difference between the two areas as being that whereas the behavioural theory of the firm brought economics and organization theory together, transaction cost theory brings together economics, organization theory *and law* via its focus on alternative contracting modes. However, from Williamson's doctoral work onwards, the relationship between his work and the behavioural approach to the firm has been rather pragmatic. In his doctoral dissertation (Williamson, 1964) and his contribution to Cyert & March ([1963] 1992) he modelled managers as trading off a variety of goals, rather than maximizing profits, in terms of a standard model of utility maximization. (Cyert & March, significantly, did not include Williamson's contribution in the revised, 1992 edition of their book.) When he moved on to develop transaction cost economics, Williamson allowed bounded rationality to be the source of contractual incompleteness but did not characterize managers and workers as satisficing agents who use rules to cope with the complexities of the business environment or life within business organizations. Williamson's failure to align his work with the satisficing approach strained his relationship with Simon for many years (see Augier & March, 2008), but it also left the way clear for mainstream economists to pick up his ideas and characterize managers and workers as if they are rational optimizers when choosing between different contractual possibilities and how to behave with respect to contracts to which they have agreed.

2.3. Impact and Prospects

The impact of Williamson's work is unquestionable and is evident in the citation counts for his key works. (As of mid-2010, *The Economic Institutions of Capitalism* achieved around 20,000 hits on Google Scholar and *Markets and Hierarchies* achieved around 14,000.) Many of these citations reflect the popularity of his work with researchers in business schools even though he has clearly alienated some management scholars (notably Donaldson, 1995) with his consistent presumption that some managers, if given the chance and no incentives to do otherwise, will pursue a 'pet project' and the perks of their jobs, or otherwise engage in self-serving behaviour, rather than acting professionally in the interests of shareholders.

Once we go beyond counting citations, assessments of Williamson's impact are made tricky not merely by overlaps between his transaction cost perspective and that of Coase but also because his analysis can appear superficially similar to the principal–agent approach to economic organization (for example, Jensen & Meckling, 1976). An agent who has a conflict of interest may indeed be tempted to engage in opportunistic behaviour unless presented with a contract that gives incentives to act in the principal's interests. However, the principal–agent approach follows standard constrained optimization thinking whereas Williamson's analysis clashes with it to some degree: bounded rationality is problematic for a closed model of choice between contracts or analysis in terms

of games with well-defined rules, while asset specificity is at odds with axioms of continuity in mainstream production theory. Williamson's approach has thus been less suited to becoming part of the core of microeconomics training in graduate schools than has more formal work on agency and contract theory that sticks firmly to traditional assumptions. Despite satisfying mainstream tastes by appearing to be deterministic (in that internalization is predicted when his four conditions hold), Williamson's model of the conditions for market failure can be read in a much more subjectivist manner that emphasizes scope for considerable guesswork by transactors regarding the surprises their trading partners might inflict upon them, what their own or their trading partner's opportunity costs really are, or how a supposed 'credible commitment' will be or should be viewed. From the latter perspective, the value of Williamson's analysis lies less in its predictive capabilities than in its use as a tool for developing skills in thinking about what can go wrong with particular kinds of transactions and coming up with potentially less disaster-prone methods of doing business.

Williamson's work was timely for debates about microeconomic reform, the downsizing of public sector activities, and the impact of globalization (since, by enabling firms to source globally rather than nationally, the freeing up of international trade may relax 'small numbers' conditions in sectors that previously favoured vertical integration). But it is hard to assess his impact on policymaking. For example, if we start examining the influences on those who have had a major role in choices of institutional arrangements in the public sector, it can be hard to separate the impact of Williamson from agency theorists and other contributors to the economics of organization (for example, see Horn, 1995). When we observe firms engaging in organizational restructuring to create product-based or area-based profit centres, this may look consistent with Williamson's view of the power of an M-form structure as a device to attenuate opportunism and create a better-functioning internal capital market. However, rather than being ploys to 'divide and rule', such changes might simply be methods for reducing coordination costs and senior executive overload: organization by class of product may be preferred simply as a means of facilitating efficient use of scarce cognitive resources. The latter would be more in line with Chandler's (1962) pioneering analysis of the rise of the multidivisional form of business organization, which, like Coase's analysis of the nature of the firm, did not centre on the possibility of opportunism. Moreover, consulting firms such as McKinsey & Company were driving the spread of the M-form approach to organizational structure during the 1960s (see Channon, 1973) before Williamson published his analysis of the phenomenon.

The prospects for developing Williamson's way of viewing economic organization are similarly tricky to assess. Although Williamson (1996, p. 373) claims that his transaction cost framework has been supported by hundreds of empirical studies (a point he reiterated in his Nobel lecture, with many more studies by then notched up), he rarely ventures into the grey area between firms and markets as organizational forms that involve cooperation or authority relationships between businesses. When he does consider 'hybrid forms' (Williamson, 1996, Ch. 4), the examples he has in mind are essentially based around formal contracts, such as joint ventures and franchise arrangements (Williamson, 1996, pp. 107–108).

When he writes about cooperation (for example, Williamson, 1975, pp. 104, 128–129), it tends to be about cooperation between workers, or between workers and bosses, within firms, rather than inter-firm cooperation.

In reality, it is common for firms to engage with each other in complex long-term relationships that resemble relationships between workers and employers. In these cases, which Blois (1972) labels as instances of 'vertical quasi-integration', it is as though one firm is an employee of another. Much business is also done via implicit contracts, as with the use of customary re-buy arrangements instead of putting each new contract out to tender. Given this, it is strange that Williamson has had little to say about such arrangements and does not refer to Blois in his key books. Indeed, there are no index entries for 'implicit contracts' or 'goodwill' in Williamson's 1975 and 1985 books and, astonishingly, despite 'relational contracting' appearing in the subtitle of *The Economic Institutions of Capitalism*, it is barely covered there. Relational contracts are referred to in the introduction (Williamson, 1985, pp. 15–16) but the only section that is actually devoted to them runs to merely two pages (pp. 71–73). He characterizes relational contracting as a governance mode that arises to deal with 'transactions of a recurring and nonstandard kind' (p. 73) and in which the reference point that transactors use for judging how things are going may evolve away from what they originally agreed. Goodwill is mentioned when he discusses the case of Toyota's long-term relationships with a multitude of subcontractors (Williamson, 1985, pp. 120–123). What he describes here is, implicitly, a case of vertical quasi-integration in which Toyota and its subcontractors share similar long-term interests and where Toyota-specific assets used by the subcontractors are owned by Toyota, so the small numbers problem the subcontractors would otherwise face is removed. However, instead of advising his readers that this is symptomatic of a need to devote substantial attention to these kinds of relational contracting, he seeks to portray the Toyota case as rather unusual because trading in Japan is 'less hazardous' than in the United States because there are 'cultural and institutional checks on opportunism' (Williamson, 1985, p. 122).

It is as though Williamson automatically sees contractual incompleteness as likely to be a problem in dealings between firms rather than as something that they might sometimes welcome as a means towards dealing easily with contingencies. His attitude is unfortunate, as there is much potential for extending his work on integration to include quasi-integration, especially by linking it to his frequent references to the role of reputation. A firm may certainly get a valuable reputation if it is known for refraining from opportunism in markets where contracts are highly detailed, but there will surely be even greater benefits of displaying forbearance over long periods of dealing with other firms via vague contracts and through challenging and unexpected situations.

There are also issues in relation to the small numbers and asset specificity conditions in Williamson's theory. He largely downplays the power of potential competition as a deterrent to opportunism in markets where there are only small numbers of actual competitors. He acknowledges (Williamson, 1985, p. 35, n. 26) that the theory of contestable markets (Baumol *et al.*, 1982) stresses the disciplinary power of potential for cross entry but he rejects this line of thinking because, contrary to his own, it downplays asset specificity. His assessment of

the difference between his analysis and that of Baumol *et al.* is correct; but he fails to acknowledge the broader implications for his work of rejecting their position and thereby emphasizing potential for small numbers of alternative transactors to open up markets to opportunism. In appealing to asset specificity to deny the likely significance of potential entry as a counter to small numbers of actual competitors, Williamson is implicitly downplaying the potential role that economies of scope may play in the decisions that firms make to diversify from one market to another. The trouble is that without economies of scope ('synergy' in the jargon of business strategists) it is hard to make sense of the existence of multi-product firms. If Williamson wishes to emphasize the significance of asset specificity to explain the existence of vertically integrated firms, he has to keep away from trying to extend his analysis of the institutions of modern capitalism to the phenomenon of the diversified corporation. This is precisely what he has done: as Kay (1992, 1993) has pointed out, Williamson's analysis of the boundaries of the firm is restricted to vertically related activities only, despite the ubiquity of horizontal linkages between the activities of diversified firms. Links between a firm's different products are also a problem for internal organization: Williamson's analysis of multidivisional structure presumes that a firm is a decomposable system of readily separable profit centres, which frequently is not the case (see Kay, 1997, pp. 254–267).

The key role that Williamson came to ascribe to asset specificity with his 1985 opus is thus unfortunate, even though it is an issue whose theoretical significance should not be disregarded. His earlier (1975) focus on the trio of bounded rationality, opportunism and small numbers provides an excellent starting point for analysing why some firms choose to diversify and others use contracts to trade economies of scale and scope. Such trading is commonplace: for example, in the automobile industry, rival manufacturers supply each other with engines, engage in 'platform sharing', serve as contract assemblers of each other's products and license out technologies. These trading relationships will only work if opportunism can be kept at bay and they will be unlikely to be contracted if the firms are nervous about opportunism or anticipate incurring enormous transaction costs to design contracts to guard against it. Deals of this kind will be harder to do the less specificity there is in the asset at the centre of the deal, because it will be harder to dream up a complete set of possible ways in which the party that is buying access to it might use it and what the terms of use in these cases may be. Thus, for example, if a new kind of technology is made the subject of a technology transfer license, the licensor runs the risk that, armed with the knowledge contained in the licence, the licensee may design a variant that enables it to escape paying further royalties. In such situations, as Kay (1993, 1997) recognizes, firms may prefer to integrate research, development and production and to set up offshore subsidiaries in countries protected by trade barriers, rather than to license production to other parties.

Williamson's focus on the division of information between parties to transactions has rather come at the expense of considering differences in transactors' know-how. The latter have been the focus of evolutionary economists and proponents of the 'resource-based view of the firm' that has grown out of the work of Penrose (1959) and Richardson (1972) (see the collection of classic contributions

edited by Foss, 1997). In Williamson's work, contracts may produce disappointing results due to guileful behaviour, whereas in the resource-based view even the best will in the world may not guarantee satisfactory outcomes if the necessary competence to undertake the activities is missing. Outcomes that are marred by incompetence are not always a matter of a supplier (or employee) opportunistically misrepresenting their capabilities at the time the contract was agreed, for tasks may turn out to be unexpectedly challenging. The way forward in understanding the boundaries of the firm probably lies with an integration of both of these perspectives: if a firm lacks the capabilities that a potentially opportunistic external supplier possesses, it may be wise to risk being let down by the latter rather than make a mess of trying to undertake the same activity in-house.

3. Elinor Ostrom: The Opportunity of the Commons

3.1. How Real People actually Govern Common Property

Elinor (Lin) Ostrom shared the 2009 Nobel Prize for her analysis of governance in common pool resources. Such resources include watersheds, forests, grazing pastures and irrigation systems, all prime issues in developing nations. Recently, Ostrom's work has extended to analysis of the global knowledge commons (Ostrom & Hess, 2006), an issue of increasing importance to developed nations. A common pool resource combines the rivalrous or subtractable aspects of private property with the excludability or free-rider issues of public property. It is a 'third type' of property, non-excludable but rival.[1] In the Pigovian and Coasian canon, this sort of property cannot work because it is incentive incompatible. It either needs to be made fully public (nationalized and regulated) or fully private (property rights attached and sent to market). But Ostrom has explained how and why this mulatto property of common pool resources can and often does work by emphasizing the role of a repeatedly interacting community of users and the local rules they develop and enforce. She has advanced this as a general analytic framework of institutional analysis and development, a framework associated with the Bloomington School of institutional analysis (Aligica & Boettke, 2009) that is based about emergent polycentric governance of community rules and norms (Ostrom, 2005).

Ostrom was not a favourite to win the Nobel Prize. Indeed, the announcement caused harrumphing in many a corridor of high-theory in economics. She wasn't even an economist, but a political scientist! Most news reports and blogs led with a 'first woman' angle. Ostrom's contribution to economics has not been a particular piece of theory but that of an entirely new architecture of analysis: applied micro-institutional analysis of community-based governance of particular resources. 'Governance' means rules for collective action, and the 'particular resources' are those managed as common pool resources (CPRs), whether from necessity or preference.[2] Her work is micro-institutional in that it applies microeconomic

[1]A fourth type is Buchanan's (1965) club goods; these are excludable but non-rival.
[2]This theme occurs in Williamson's work as the problem of asset specificity.

concepts and methods—rationality, game theory, and experiments—to institutional contexts (rules for governance). It is applied as based on fieldwork with analysis directed toward solving specific problems. But the most challenging dimension of her work (for economists at least) is the emphasis it affords to notions of community.

An interesting aspect of Ostrom's assumptions and approach are her agents, who are recognizably human in that they are willing to enter into local agreements about the use of local shared resources, but are also willing to monitor and punish others who take advantage of this trusting situation, even at personal cost to themselves. Hers are rational and self-interested agents, but they are not isolated untrusting egoists with high discount rates and no communication abilities. Rather, they are your neighbours, people like you with whom you might meet to discuss and endeavour to agree upon matters of local concern. In this sort of world (notably, the opposite of Williamson's perspective) opportunistic free-riding is possible, yet it is difficult and costly because of the existence of a community that affords monitoring and administers sanctions. A community is thus an emergent concept in Ostrom's framework as a group of people with a shared interest in the sustainable use of a resource. It means both communication and commitment between people who can successfully interact to arrive at governance rules concerning the resource, a collective action that thereby places the resource in a common pool, thus enriching the community through the institutional emergence of a sustainably governable asset.

Ostrom's work reinforces not only Hayek's (1945) view of local knowledge, but also Jane Jacobs' (1961) view of local monitoring and enforcement. She explains:

> Instead of presuming that optimal institutional solutions can be designed easily and imposed at low cost by external authorities, I argue that 'getting the institutions right' is a difficult, time-consuming, conflict-invoking process. It is a process that requires reliable information about time and place variables as well as a broad repertoire of culturally acceptable rules. (Ostrom, 1990, p. 14)

The upshot is almost homespun—where a functioning community does exist, the opportunities for resource governance are extended to include common pool property. Further, this is often a superior solution to public ownership or regulation (because of government failure), or private ownership (because of market failure). Functional communities and the local/micro institutions they can create thus underpin the effectiveness of localized common pool resource management.

The cornerstone of Ostrom's oeuvre is a devastating rebuttal to Garrett Hardin's (1968) 'tragedy of the commons' metaphor. Decades of fieldwork coupled with statistical, experimental and theoretical analysis have led Ostrom to conclude that, for the most part, the tragedy of the commons simply is not so; Ostrom's work emphasizes instead the latent opportunities of the commons. Many communities involved in CPRs do manage to work out effective governance solutions. The standard responses to Hardin's tragedy, namely public regulation or privatization, are not the only solutions. A third option is local governance. 'What is missing from the policy analyst's toolkit', she explains (Ostrom, 1990, p. 24) 'is

an adequately specified theory of collective action whereby a group of principals can organize themselves voluntarily to retain the residuals of their own efforts.'

Ostrom has analysed how systems of rules can be created that self-organize at the level of local common property to achieve effective governance outcomes not based on extremes of public or private ownership. Bottom-up local rule formation and enforcement can resolve social dilemmas associated with collective property when coordinated about functioning communities of users and the rules they create and enforce. The treatment of rules in economics, as institutions, is routinely exogenous. Rule evolution through local interaction and feedback is not a standard assumption or point of inquiry and analysis. But Ostrom shows that local mechanisms of interaction manifest in emergent rules are actually far more important than previously supposed. Local governance over CPRs can work well in the absence of higher governance. 'It is ordinary persons and citizens who craft and sustain the workability of the institutions of everyday life' (Ostrom, 2000a, p. 505). This offers a bottom-up self-organizing view of emergent institutions as local rule formations to coordinate economic resources.

3.2. Works and Career

Elinor Ostrom was born into the great depression in Los Angeles in 1933. After studying political science at UCLA (Ostrom, 1965), her early work focused on public sector problems from a localized economic perspective, specifically the allocation of policing resources (e.g. Ostrom & Whitaker, 1973). Along the way she met Vincent Ostrom and together they formed a powerhouse intellectual coupling that settled at the Department of Political Science at the University of Indiana in Bloomington, Illinois. With Vincent, she co-founded the *Workshop in Political Theory and Analysis* in 1973, an institution that remains the very model of an interdisciplinary social science research centre. In 2006, she co-founded the *Centre for the Study of Institutional Diversity* at Arizona State University. Elinor Ostrom has won most every award to be had in political science, policy studies and public choice. She has received a plethora of honorary degrees and has been awarded over 30 major research grants (particularly NSF and MacArthur grants). Her list of professional association activities, advisory boards, consulting work and editorial board memberships is prodigious. Hers has been a bold, ambitious and astoundingly successful academic career built on a clear scientific vision and a resolute determination to uncover the surprising truth about the economic coordination of common property.

Ostrom is, by all accounts of those who know her, and even by the off-hand stories of those who have only attended a lecture or participated in her legions of field-workers, an intellectual dynamo and thoroughly engaging and inspiring personality. Her rigorously charismatic bearing and dedicated scientific pursuit has inspirationally changed the life of many a young researcher. But she started off hard, born into poverty, with a bad stutter, and seeking to pursue a career that was then mostly closed to women. She fought through it. Her post-doctoral work in municipal policing went against the grain of all received expert wisdom. She explained why that wisdom was wrong. Her work on common pool property went against the logic of all good economic theory. She explained,

ingeniously by co-opting new theory (in repeated games), but on the back of painstakingly exhaustive field work, just as she had done with the policing question, why that was wrong too. She won the Nobel Prize for that. Awesome is a much overused word, but those who know her tend to use that word a lot.

Ostrom has so far published over 270 academic papers and book chapters, many jointly authored and over a wide scholarly domain. She has edited 21 books and written 11 monographs (most jointly authored), one directly cited in the Nobel Prize (Ostrom, 1990). She is prolific, and the hub of a vast web of citation networks. Like Williamson, Ostrom is highly cited within but especially beyond economics. She describes her work as 'a behavioural approach to the rational choice theory of collective action' (Ostrom 1998, 2000a) and her method as 'new institutionalist' (Ostrom 1990, p. 29). A behavioural approach to rational choice may seem like an oxymoron, but what she means is that in the context of common pool resources smart, rational people need to interact as a community to arrive at rules (or institutions) that depend upon the behavioural characteristics of others and that work with the grain of behavioural propensities, which include, surprisingly, a propensity to monitor and a willingness to punish, both of which we would not expect from a purely rational egoist agent. Ostrom's findings thus draw upon behavioural propensities to explain how rational individuals can arrive at emergent governance institutions that standard economic theory suggest should be impossible or at best unstable. Yet her extensive fieldwork and later experimental work shows that this self-organizing outcome is actually surprisingly common in practice. Her method encompasses fieldwork, experiments and theory, ranging over political science, economics, political economy, institutional analysis, public choice, sociology and game theory. She is multi-methodological and multidisciplinary.

Ostrom's early work in the 1960s and 1970s examined the institutional arrangements of municipal police departments in metropolitan areas. At the time, the consensus was that the overlay of small, local police departments was wasteful and inefficient and would be better served by consolidation and centralization. But Ostrom's comparative studies showed that this was not true. Small local police departments provided better service and value for money than larger consolidated administrations. Why? Because policing experiences diseconomies of scale: larger organizations increase the distance between police and the communities they serve, breaking down the networks of local enforcement, knowledge and community trust upon which effective police work depends. This argued for a *polycentric* approach of many small and partially overlapping police districts. She outlined a new economic analysis of community organization of public goods and urban policy analysis by emphasizing the choice theoretic basis underlying these public services (e.g. Ostrom and Whitaker 1973; Ostrom *et al.* 1977).

Through the 1980s, Ostrom's work developed in the direction of common pool resources (including watersheds, irrigation systems, forests, pastures and fisheries). By collecting and analysing a great many instances of common pool resources from around the world, she sought to reveal the universal rules of successful common property systems. But the problem was that she couldn't find any. Private property, communal property and government property all worked in some

cases and failed in others. But what Ostrom did find were some common architectural principles that seemed to describe successful common pool resource use. These emphasized the role of communities of users in developing, agreeing and enforcing rules, and were typically cobbled together to reflect local circumstances and characteristics of the specific resource. The result of these studies on the effectiveness of user-level governance was gathered in her 1990 book *Governing the Commons*.

Along this path, a significant breakthrough came in the late 1980s when, on sabbatical at Reinhard Selten's institute at the University of Bonn, Ostrom connected her surveys of CPR case studies to non-cooperative repeated game theory (associated with the work of Robert Aumann; see also Benhabib & Radner, 1992; Greif, 2006). This positioned Ostrom's work firmly in the research program of New Institutional Economics, and furnished the micro-institutional analysis of the conditions under which cooperation can emerge in CPRs (Ostrom *et al.*, 1994). It provided a theoretical account and analytic framework of how cooperation can emerge through repeated interactions about the exploitation of a common resource. This was her scientific breakthrough.

Ostrom then extracted design principles that connected her extensive case studies to theoretical and experimental conjectures. Some principles were formalized common sense: recognizing the importance of rules to clearly specify who held what rights and entitlements with respect to the resource; clear and workable rules for conflict resolution; and equity principles connecting responsibilities to expected benefits. But a further aspect of the turn to repeated game theory was an elucidation of the central importance of user participation in the formation of rules and sanctions, as well as user enactment of monitoring and punishment (Ostrom, 2005). In recognizing these distinct mechanisms, Ostrom's research program (along with many colleagues and other researchers; see Nowak, 2006) turned toward laboratory-based experimental endeavours to isolate and examine the institutional mechanisms that contributed to resolving social dilemmas in CPR governance (Ostrom, 2000b). In particular, the role of communication and the intrinsic motivations for punishment, and notably selective punishment, were revealed to be central to understanding how effective CPR governance can emerge and function.

3.3. Consequences and Prospects

When rational economic agents can interact as a community (*à la* repeated game theory) about a common scarce resource in which they have a stake, effective but *ad hoc* governance institutions can result. The upshot is that the public–private (or left–right) debate misses a third option of bottom-up community self-organization through effective emergent and negotiated governance of a CPR. Ostrom has studied and explained how such governance can work, and also how common such actual governance is. *Pace* Williamson, in the domain of common property, community self-organization routinely trumps free-rider opportunism. Following Hardin's (1968) tragedy of the commons, along with much received economic theory, it has been conventional wisdom to assume that CPRs cannot work and should ultimately and properly revert to either private ownership (Coase, 1960)

or public regulation (Pigou, 1920).[3] Yet Ostrom showed that such communal property can and does work without requiring either of these top-down extremes if (and only if) the local communities of users can function to create effective rules of governance. Her life's work has been devoted to uncovering exactly what these 'if' conditions are.

In economic theory, what Ostrom has shown is that there is a world of difference between one-shot games in an anonymous Hobbesian society and repeated games in a successfully interacting community mutually concerned with and interacting about a common resource. In the latter circumstance, common property can actually flourish as an optimal solution, although it critically depends upon the prevailing social norms and connectivity as well as the context of particular resource variables. CPR systems can, in theory and practice, be both robust and efficient. But Ostrom's deeper point is that the pathways to that state of a commons are particular, even counter-intuitive. Town-hall meetings and their proliferating digital equivalents thus become crucibles of community formation and strengthening in the creation of these rules. But these must then be internalized in order to express monitoring and punishment. A surprising finding, for example, is that this works best through intrinsic motivations. Another is that the connective forces holding this together have a distinctly network character, forming through reticulations of stabilized interactions. This evolutionary framework connecting resources, behaviour, rules and community norms (or constitutional rules) to institutions is the overarching methodology of Ostrom's framework of Institutional Analysis and Development.

With CPRs there is no general solution, no master equation; only interested people trying to muddle through. Her profound discovery is that this often works. Centralized governance through experts and bureaucrats is less required than commonly supposed. Ostrom's meticulous work covering thousands of case studies of CPRs has shown that they often do work tolerably well, in being superior to purely privately owned or publicly regulated outcomes, but only when user-communities can themselves envisage and develop localized mechanisms for decision-making and rule-enforcement (Ostrom, 1990). Like Hayek (1945), Ostrom emphasized the importance of local knowledge of time and place. But she has extended that to local governance of monitoring and enforcement. For a patient and involved community of users in a 'repeated game', the commons need not be tragic.

Ostrom's work thus addresses the deeper issue of who follows which (economic) rules and why? Under what circumstances can isolated, self-interested individuals come together about a shared social dilemma associated with a rivalrous but non-excludable resource to form an emergent community of governance? The two standard answers to this problem both disavow the concept of self-organizing communities. The Hobbesian solution, in the *Leviathan*, is to acknowledge the human stain of selfishness and free-riding, and govern with centralized force. This is the regulatory force of public ownership as a solution

[3]Ostrom's Nobel Prize lecture makes pointed reference to this implicit presumption as a still-dominant policy metaphor.

to the tragedy of the commons. The Lockean solution is to suppose that common property is inherently incentive incompatible and only fully privatized ownership can work. Governance is either by state or markets; everything between offers an inefficient and flawed compromise. But Ostrom suggests a third model of emergent localized polycentric governance of complex economic systems. Like Jane Jacobs and modern complexity theorists, her work emphasizes the possibility (and under certain conditions, probability) of localized emergent order (Boettke & Coyne, 2005). But she also emphasizes that these rules of governance can themselves become building blocks for higher-order systems (Kauffman, 1993; Potts, 2000). For Ostrom, functional communities are more incentive compatible than commonly understood and also more powerful than commonly realized.

Several contemporary applications of Ostrom's framework are notable. One is the global knowledge commons (Ostrom & Hess, 2006) and the growing importance of open-source production and innovation. Since the development of the world-wide-web, an enormous new commons has emerged in global knowledge (for example, Wikipedia), which has been differentially successful as a function of the efficacy of the institutions that have emerged from the community of users to govern this new commons. Another instance concerns the rise of open-source communities of practice, first in open-source software but increasingly in open-source approaches to innovation (e.g. von Hippel, 2006). This connects cultural and media studies, law and economics (e.g. creative commons licensing), evolutionary and innovation economics and business strategy. Ostrom's work is likely to resonate here for decades to come. Yet another instance is global atmospheric pollution and climate change, a kind of inverse commons problem of governance of a 'negative resource'. Both are bigger problems than governance of local subtractive resources, but Ostrom teaches that solutions to big resource governance problems (e.g. global knowledge, global environment) do sometimes and occasionally best emerge from the messy and seemingly disorganized process of bottom-up percolation of communities and institutions. Her lesson: trust less in government and markets, and more in people.

4. Conclusions

The 2009 Nobel Prize to Oliver Williamson and Elinor Ostrom was a 'governance and institutions' prize. They both showed why institutions of governance matter to the building blocks of economic organization—Williamson with firms as organizations; Ostrom with institutions as organization. Both break the standard rules of how to be an economist: Williamson draws on other disciplines, while Ostrom is a model of pluralism, happy to use both experiments and multiple case studies from fieldwork. Despite this, however, the 2009 award is rather redolent of the 1974 prize to Gunnar Myrdal and Friedrich Hayek, also awarded for saying opposite things about the same problem.

Williamson says governance is a problem because people are self-interested: asset specificity matters and organizational rules that overcome self-interest can solve this problem. Ostrom says that governance is a solution because common resource (*née* asset) specificity creates incentives for community organization: self-interest can be coordinated when appropriately focused. For Williamson,

most people are good, but some are self-serving and will go unpunished unless institutions are designed about this latter group's governance. Ostrom, however, believes that most people are willing to punish in order to achieve cooperation (her agents are socially smarter than his agents): institutions can be designed about emergent governance. Trust plays a major role in Ostrom's view of how common pool resource problems are handled, whereas Williamson is reluctant to consider people as having natural tendencies to trust; instead of seeing trust in a calculative sense, he prefers to focus on the credibility of commitments that trading partners are prepared to make. Williamson sees top-down processes as the means by which opportunism is limited in organizations, whereas Ostrom sees bottom-up processes as preventing the 'tragedy of the commons'. These are very different perspectives about the exploitation by governance of a common resource.

Pluralists may feel comfortable about the 2009 Nobel award going to both these scholars, for despite their contradictory ways of looking at the world they both seem to have achieved important insights in their respect contexts. However, it may be better to try to consider what potential there is for building bridges between the two contributions rather than confining them to different contexts. We believe that the way to do this is to see firms and the demand side of markets as common pool resources. A firm whose employees treat it as something they can exploit ruthlessly in a non-cooperative manner will not stay in business for long. Likewise, if firms see a population of customers as a pool to be fished with no regard for the longer term, the use of devious baits to capture revenue without offering fair value will drive customers away. By focusing so much on opportunism by individuals, Williamson fails to consider the potential for cooperative behaviour to render unnecessary internalized control by a top-down management system.

At the level of the firm, Williamson failed to spot the implications of a key theme in Barnard (1938), despite frequently proclaiming his admiration for Barnard's work and editing a volume (Williamson, 1995) in honour of it: executive authority is granted by subordinates to managers; it does not arise by virtue of rank. If an executive has good leadership skills, there is no need for cunningly devised incentive systems and oppressive monitoring to try to stop workers from pursuing sub-goals with opportunism. The key thing the leader needs to do is help the workers to see that the best way to serve their own interests in the long run is to do what serves the interests of the firm in the long run—a core ingredient in the 'Japanese way' of doing business. The leader cannot force subordinates to buy into this idea, but a consensus may emerge that the leader's exhortations should be followed rather than ignored. This is why leadership modules have a vital role in MBA programmes and it is very much the perspective that Simon (1997) adopted: he could see beyond tendencies towards sub-goal pursuit, instead viewing a well-managed organization as one in which members pick up the organization's value system and mission and therefore generally do not see it as something to be milked for their own ends. Moreover, boundedly rational workers may be unable to devote attention to devising opportunistic strategies because they are kept busy on deliverables that their bosses can monitor.

At the level of the market, a common pool resource perspective is implied in the work of George Richardson ([1960] 1990, 1972). He sees the need for 'imperfections' and institutions to ensure that markets work efficiently. Markets that are overly easy to enter will not be healthy in the long run, for an overpopulation of suppliers will have trouble making normal profits. Such difficulties will promote opportunistic behaviour as a means of hanging on in the immediate future, with adverse long-run consequences for demand. Richardson saw communication and cooperation as means by which customers and suppliers can engage in mutually beneficial transactions in the long term. Firms that take a long-term view will form trade associations and use them to develop and maintain standards of supply. Excessive entry may be limited, while membership provides a means to signal to potential customers that a firm is not a 'fly by night' operation. By grouping together in industrial districts, firms can more readily gather intelligence about the practices of their rivals, which may be of interest to customers. Firms that wish to keep their customers in the long run also have incentives to deal with contractual problems in good faith, as do firms that want to keep their suppliers.

From this perspective, the division of labour between firms is seen as being due to differences in capabilities, and the internalization of activities is something that occurs to reduce coordination problems between activities that require different capabilities, rather than because of fears of opportunism. If entrepreneurs are reluctant to get involved in activities that are beyond their range of experience and if market institutions and long-run incentives seem to guard against rogue traders being present, outsourcing will be the preferred strategy. In such situations, opportunism will mainly drive firms to choose vertical integration *ex post*, where Richardson-style processes have unexpectedly failed. Moreover, to the extent that firms are concerned about the potential for devious moves by the businesses on which they depend, they can safeguard their positions via partial shareholdings and interlocking directorships; full integration is not intrinsic to such a situation.

Williamson is well aware of Richardson's contributions: not only does he refer to them (see Williamson, 1975, pp. 78, 108; 1985, p. 83), he even provides an endorsement on the dust-jacket of the second edition of Richardson's 1960 book, calling it 'an early and important contribution'. Yet he makes limited use of Richardson's theoretical analysis despite accepting that the latter's examples show that 'activity in the middle range [i.e. long-established linkages and goodwill] is extensive' (Williamson, 1985, p. 83). We do not wish to suggest that the Richardson view always holds but that a much richer perspective on the functioning of the economic system is obtained by recognizing the significance of both opportunism and cooperation.

A consistent, though probably more baffling, salutation by the Nobel committee might therefore have been to award the 2009 Nobel prize on economic governance and the institutions that underpin it to Elinor Ostrom, Oliver Williamson *and* George Richardson, with significant citation to Geoffrey Hodgson, Brian Loasby, and Deirdre McCloskey, among others: Richardson ([1960] 1990, 1972), for his theory of how market coordination actually works through associations of cooperation and a complex web of relationships; Hodgson (1988), for presenting this as a theory of institutional evolution; and Loasby (1999) and McCloskey

(2006) for grounding and elaborating this idea in a historical and analytic context.[4]

Our point is not to sideline Williamson; he has plainly made an enormous contribution to the theory of economic governance. Rather, we simply recognize that part of this prize went to the theory of bottom-up organization through individual initiative and communication, and the emergence of economically functional community and social structure as a result—which is where Richardson's work takes a more Ostrom-like view of the territory that Williamson sought to demystify. This is an award for the origin of economic institutions, as much as for their efficacy and governance properties, a point that is true of Williamson as well as Ostrom. Bridged by Richardson's contributions, their works sit more easily together.

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[4]Hodgson, Loasby and McCloskey have all written a significant number of other works that support our nomination.

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