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January 8: Nobel Lunch Honoring Elinor Ostrom and Oliver Williamson

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The work of Nobel laureates is usually so well known that another recitation of its highlights would seem superfluous. It remains valuable as a tribal ritual, as bards of yore used to sing about the heroic deeds of ancestors around the fire after dinner, but it conveys no new information. However, in the economists' tribe, Elinor Ostrom may be a rare exception to this rule. The announcement of her prize caused amazement to several economists, including some prominent colleagues, who had never even heard of her. That in turn was amazing to me, as I had long held her work in the highest esteem, even in awe. Some things said about her in blogs and other media were so ignorant and in such bad taste that I felt ashamed on behalf of the economics profession, even though in general I am very upbeat about it. I hope that by now others have caught up and changed their opinion, but one more statement today may still be needed for some.

Ostrom's research is perhaps best characterized as "the reality of collective action" (echoing Mancur Olson's "the logic of collective action"). She has studied numerous and varied social institutions that attempt to solve, with varying degrees of success, problems of common resource pool management. In the process she has overturned much of economists' previous framework for thinking about this important class of collective action situations, and replaced it with a richer framework that gives a much better understanding of reality.

Even more fundamentally, Ostrom was instrumental in changing our traditional mindset in which the institutions and organizations under which economic activity occurred were fixed at some other level of analysis, and economics concerned itself only with individuals' choices of the types and quantities of goods and services to produce and consume, to supply and demand. In the context of common-pool resources, traditional economics took either the Pigovian line where a government imposed optimal taxes or quotas, or the Coasian line where the government defined and enforced property rights and contracts but otherwise stayed aside. As Ostrom wrote memorably in her prize lecture: "The classic models have been used to view those who are involved in a prisoner's dilemma or other social dilemmas as always trapped in the situation without capabilities to change the *structures* themselves. ... Public investigators purposely keep the prisoners separated so they cannot communicate. The users of a common-pool resource are not so limited." (Emphasis added.) She focused on the "potentially productive efforts of individuals and groups to organize and solve social dilemmas."

Note the word "potentially." The likelihood of success of these efforts depends on the underlying conditions of the situation. Most importantly, Ostrom's own field studies, and her meta-analysis of others' studies, enabled her draw some systematic conclusions about cause and effect in this arena.

I think that three sets of her findings deserve continued special emphasis.

[1] Common-pool resources with clearly defined boundaries, and a stable group of insiders who have the collective right to withdraw the resource while outsiders do not, are more likely to succeed. The stability generates ongoing interaction. Economists' textbook understanding still follows Hardin and thinks of the commons as a one-shot game; this should have been abandoned long ago.

[2] Groups that devise clear rules stating what actions are and are not allowed, base these rules on local conditions and knowledge, adapt the rules to changing circumstances, and organize monitoring and enforcement also based on local conditions and capabilities, are more likely to succeed. A distant central authority often lacks all these advantages. The left model of a benevolent and omnipotent government, and the right model of efficient markets supported only by a centralized legal system, are both deeply flawed.

[3] Sanctions for non-compliance are never the grim trigger strategies favored by game theoretic modelers; they start at very low levels and become more severe only with repeated infractions.

I could go on, but must leave time for today's other honoree for whom I have equal admiration and awe. Therefore I will urge you to read Theodore Bergstrom's article in the Scandinavian Journal for a fuller tribute to Ostrom's work, and confine myself to just one more remark.

I find Ostrom's method of work as impressive as its substance. Although she is a political scientist by training and affiliation, she has excellent knowledge of economics research ranging from Aumann to Williamson, and a clear understanding of what economic theory and game theory have and have not done for the issues she researches. She uses many different but complementary approaches, and refuses to be doctrinaire about any one. Her own field studies, meta-analyses of hundreds of case studies by others, laboratory experiments, theoretical analyses of repeated games using conventional economic self-interest models and appropriate other-regarding behavioral models, all contribute to building up her convincing framework. She has described some of her conclusions as "going beyond panaceas"; in her work she also goes beyond methodological panaceas.

If Elinor Ostrom's work is less well known among economists than it deserves to be, Oliver Williamson has reached the stratospheric level of being probably the most cited economist of all time. For most of us, to get an up to date citation count of our work, a weekly or even monthly check suffices. For Williamson, a day or even an hour can make a big difference. I had some commitments this morning and couldn't make a final check just before coming here, so I apologize if my numbers are badly out of date. But as of last night, Google Scholar gave the following statistics:

Economic Institutions of Capitalism : 20556
Markets and Hierarchies : 14357
Mechanisms of Governance : 3576
Transaction Cost Economics, in *Handbook of NIE* : 6457
Markets and Hierarchies, AER 73 : 5777
Comparative Economic Organization, *Admin Sci Q* : 3667
8 other articles with > 1000

As these numbers suggest, Williamson's pathbreaking ideas have permeated economics very thoroughly. Now the danger is that they may seem obvious in the light of today's consolidated understanding of the issues. Therefore we need to pause and remember just how revolutionary his ideas were when he first advanced them. Once again I can give only touch on a few points, and must refer you to Robert Gibbons' Scandinavian Journal article, and the scientific statement of the Royal Swedish Academy of Science.

Williamson redirected the focus of the economics of industry from the technological conditions of production to the informational and organizational conditions of transactions. He also recognized that organizational forms are themselves endogenous, capable of adapting to the needs of transactions. He crystallized this way of thinking as the "discriminating alignment hypothesis." Like all such broad principles, this expresses a tendency, not something that holds true precisely and at all times, but it is a tendency important to remember when we observe organizations and their dynamics, and recommend reforms.

The usual statement of Williamson's contribution is that he operationalized Coase's insight about the boundaries of the firm. He produced a much more detailed and nuanced analysis of the very broad catch-all category "transaction costs". He distinguished various kinds of these costs: [1] Complete contracts specifying actions in all conceivable contingencies are infeasible; ex post unprogrammed adaptations must be made. This stood in sharp contrast to the conventional view, most rigorously formulated in the Arrow-Debreu framework of general competitive equilibrium with complete contracts. [2] Many interactions require one party or the other to make relationship-specific investments; this reduces or even eliminates the check that competition can provide and creates the hazards of opportunism. This was a big departure from the tradition, attributable to the old Chicago school, where everything was flexible and strategic manipulations such as hold-up (or entry deterrence) were not possible. [3] The ex post bargaining that ensues in these situations can have inefficient outcomes. This was again contrary to much economic thinking based on the core or the Nash bargaining solution; models of strategic posturing in bargaining resulting in inefficiencies are quite recent.

Williamson's first application of these principles was to vertical integration. He argued that instead of viewing it as a way of increasing market power, we should view it as an adaptation to reduce the transaction costs involved in arm's length

dealing in an intermediate good, when its production or use requires specific investments, and contracts covering all contingencies of cost and demand changes cannot be written or enforced. The importance of specific investments, and contractual complexities and incompleteness, are matters that can be measured or proxied, and their implications for vertical integration can be tested. Indeed, such empirical work has provided good consistent support for Williamson's theories, which is admittedly a rather rare achievement in economics.

The hypothesis that organizational forms are (or should be) chosen to economize on transaction costs has produced many other empirically supported applications. It suggests not only whether two parts of the production process should be integrated into one firm, but also who should own that firm and control the residual decision rights. It connects law and economics, helping us understand when contracts will be made and enforced using relational or other private ordering, instead of relying on the court system. It also helps connect corporate finance with corporate governance. A firm whose assets are specific must be financed using more equity, because in the event of dissolution debt-owners could not benefit by seizing the assets. Williamson's work has inspired or intrigued other researchers to develop and explore these applications. Sanford Grossman, Oliver Hart, Bengt Holmström, Jean Tirole, Michael Jensen and William Meckling, and many others have constructed rigorous theoretical models of these and other situations. This work has in some cases clarified or refined Williamson's arguments, and in others has led to important new insights or modifications of the original ideas. Today's graduate students and young researchers study organization theory in this new garb. However, we should not forget the debt that these subsequent researchers, and indeed all of us, owe to Williamson's pioneering work.

These ideas have huge implications for policy, not only in the area of antitrust and regulation where it has already had substantial impact, but in political economics more generally. Perhaps most important is Williamson's constant reminder, motivated by many examples, that unsatisfactory outcome under some institutional or organizational setup is not automatically an argument for overhaul or change in that setup. We need to ask whether our proposed alternative is feasible given the limits of information and implementation - in Williamson's terminology, whether the failure is "remediable." As a part of this thinking, we should ask why the institution or organization we observe is currently in place. Williamson's classic example is that a complex transaction may be observed to be taking place, albeit with less than perfect efficiency, under conditions of hierarchical control within a firm because it would fare even worse in an arm's length market. Similarly, an activity may be organized in the political arena because its private organization would be even more inefficient. To give a current example, today critics of the Federal Reserve need to ask themselves why that institution was created in the first place. They seem to forget that the previous private system did not fare too well in the financial crisis of 1907, and that complete collapse was averted by the skin of our teeth and only because Morgan was too big - not too big to fail, but so big that he could rescue others.

But I digress. Today my purpose is not to get into the contentious area of the financial crisis and its implications, but to honor and celebrate the achievements of two giants of our profession. Not just the economics profession, but the social sciences more generally. The research of Ostrom and Williamson has transformed and enriched our understanding of institutions and organizations that are formed and reformed to cope with all kinds of economic, social, and political interactions. They have spurred much further research; they have had a big effect on my thinking and on my own research on economic governance. More important, they are beginning to have a serious impact on policy. We will long be in their debt, and I am honored and delighted to have been chosen to say these few words that express our gratitude.

Theodore Bergstrom, "The uncommon insight of Elinor Ostrom," *Scandinavian Journal of Economics*, 112(2), 2010, 245-61.

Robert Gibbons, "Transaction-Cost Economics: Past, Present, and Future?" *Scandinavian Journal of Economics*, 112(2), 2010, 263-88.

Royal Swedish Academy of Sciences, "Economic Governance". At http://nobelprize.org/nobel_prizes/economics/laureates/2009/sci.html