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NEW INSTITUTIONAL ECONOMICS: A REPORT CARD

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INTRODUCTION

During the first three decades following World War II mainstream academic economists focused their attention on developing and expanding the theoretical foundations for what is commonly called neoclassical economics and on the development and application of econometric techniques to measure empirically the parameters of these theoretical models and to test hypotheses about their properties. In microeconomics we saw the development of rigorous theoretical models of consumer demand, firm production and cost functions, the foundations of competitive market equilibrium, with and without uncertainty, and the implications of a wide range of market imperfections (e.g. externalities, oligopoly, asymmetric information) on firm behavior and market performance. Econometric techniques to estimate the parameters of demand and cost functions and to measure the effects of market imperfections on prices, costs and other market attributes were developed and applied as well.

In macroeconomics we saw the development of theoretical models to explain key determinants of aggregate economic activity --- income, consumption, investment, inflation, unemployment, and economic growth. This work focused initially on the rigorous theoretical articulation of the foundations of Keynesian economics and then on alternative non-Keynesian and post-Keynesian models linked more closely with neoclassical microeconomic foundations of firm and consumer decisionmaking, price and

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wage formation in markets, and investments in human capital. This theoretical work was accompanied by new econometric techniques to use macroeconomic data to estimate the parameters of key aggregate economic relationships. These empirical relationships were used in turn to create large macroeconomic models to assist in making predictions of the components of aggregate economic activity and the effects of government tax, expenditure and monetary policies on these variables.

In parallel with these developments in “positive” microeconomics and macroeconomics, substantial efforts were made to develop rigorous theoretical foundations and supporting econometric techniques for evaluating the societal implications of individual and market behavior and performance and the effects of various public policies on social welfare --- modern welfare economics. Going beyond simple utilitarian models of social welfare, this work confronted the challenge of dealing with diverse consumer preferences and interpersonal comparisons, aggregation of consumer preferences, and the ethical implications of wide distributions of income and wealth in the population.

There can be no doubt that these post-World War II developments in economics have been extremely important from both an intellectual and a practical perspective and have helped to expand dramatically our understanding of many aspects of market structure, economic behavior and economic performance, especially in developed economies. The tools that have been developed are widely used in government policymaking and business decision making. And progress in theory and empirical applications within the neoclassical tradition continues to be made. Nothing in this

essay is meant to diminish the many important advances in economics that have been achieved during the last fifty years.

It appears to me, however, that the incremental knowledge resulting from the ongoing work in this neoclassical tradition began to yield significantly diminishing returns by the mid-1980s or even earlier. The low-lying fruit had been picked and the remaining fruit in the tree began to become much more difficult to find and harvest. Moreover, in many ways these developments have been less than fully satisfactory, or at the very least, provide an incomplete framework, for understanding many important economic phenomena.

The neoclassical tradition adopted either an a-institutional or non-institutional approach to economic analysis. The basic underlying legal institutions that are widely assumed to be necessary to support the behavioral assumptions and market structures being analyzed --- e.g. credible property rights, enforceable contracts, private ownership, well functioning capital markets and corporate governance systems --- where either implicitly assumed to exist and to operate costlessly and perfectly (or not at all in the case of externalities) or were effectively ignored completely or swept under the rug. Firms were black boxes characterized by production functions and their horizontal expansion governed by economies of scale driven by the underlying technological attributes of these production functions. The inability to measure significant economies of scale at the plant level econometrically led many industrial organization economists to the conclusion that firms were too large and that deconcentration policies would have potential competitive benefits with little potential economic costs. Vertical integration and associated vertical contractual arrangements were difficult to explain with the prevailing tools, except

trivially by appeals to unspecified “economies of vertical integration” or as strategic responses of firms to increase market power at one or both levels of the production chain (Joskow 2004). Technological changes that led to the introduction of new products and new production processes were understood to represent important components of economic growth and consumer welfare but the theoretical and empirical foundation for understanding the rate and direction of innovation and how they are influenced by microeconomic, macroeconomic, institutional and policy considerations was poorly understood. Economic growth was driven by changes in capital and labor inputs, exogenous technological change, and poorly understood differences between countries over time and space.

Benevolent governments with public interest goals and perfect information were available to make policies “in the public interest.” While it was recognized that governments could do things that could either improve or undermine economic performance, the economic and political considerations that led to alternative government policy initiatives, and affected the structure and behavior of government institutions that influenced economic growth, from legislatures to courts, were largely ignored. Microeconomic theory focused on private profit-maximizing firms while large portions of economic activity were governed by state-owned firms, state agencies and non-profit organizations. The nature of the choices between different governance arrangements and their consequences were largely ignored. Finally, while the theory and associated empirical analysis developed over these decades was “generic” in the sense that it was thought to be applicable to any economy, in practice it was difficult to apply generically. This was particularly problematic in applications to developing countries, without

somehow taking account of the “idiosyncratic” and unmeasured attributes of social, political and economic attributes of “institutions” in different countries. There was little progress in understanding these “idiosyncratic” attributes that characterized institutions in different countries, how and why they mattered, their linkages to historical and cultural attributes, and how they could or would change over time in responses to changes in the economy, economic growth, changes in government and legal institutions, and to policy initiatives mediated through these institutions.

These limitations of neoclassical economics are now widely recognized. A growing number of scholars are engaged in research to respond to these limitations in a number of different ways. We see this evolution in several apparently different but fundamentally interrelated “new” fields of economics: law and economics, political economy, behavioral economics, organizational economics, evolutionary economics, the economics of contracts, and new institutional economics. In some ways these fields are not “new” at all since their origins can be traced back to pioneering research, sometimes largely ignored at the time, that was produced decades ago. However, in other important ways these fields are indeed new. First, they do not reject the basic progress that has been made in the neoclassical tradition over the last fifty years but recognize both its strengths and its limitations. Second, they do not reject the basic analytical tools that have been developed over the last fifty years --- mathematical modeling and econometric analysis --- but use these tools to address a broader set of issues. Third, they supplement these methods of modern economic analysis with additional analytical and empirical methods and analyses which include, for example, case studies and experimental methods, that are appropriate for addressing the relevant issues more completely. Fourth,

they draw on scholarship from a broad range of social and behavioral sciences: history, law, political science, anthropology, psychology, sociology and other disciplines to address issues that neoclassical economics addresses poorly or not at all. Fifth, they recognize that economic theory and empirical regularities are often not “generic” and are more or less relevant or relevant in different ways depending on economic, social, political, and legal attributes of different countries. One size does not fit all and, in particular, differences between developed and developing countries can lead “reasoning by analogy” to result in serious errors. Finally, rather than taking a position outside of economics and looking in at it, often critically, these efforts seek to be fully integrated into advances in economic theory, empirical methods and applications.

WHAT IS NEW INSTITUTIONAL ECONOMICS?

The efforts to move economics beyond the limitations of neoclassical methods and models, and the progress that is being achieved, is truly exciting. It is not my intention, however, to discuss all of these developments. Rather, I want to focus on developments in institutional economics or, more precisely, New Institutional Economics, that motivated the founders of the International Society for New Institutional Economics (ISNIE) in 1997. The founders of ISNIE had (and have) a broad range of interest in and approaches to economic analysis. Nevertheless, they shared a common set of basic beliefs that defined the research topics they would focus upon and the research methods that they would use and sought to foster:

- Legal, political, social and economic institutions (“institutions”) have important effects on economic performance. The effects of alternative public

policies aimed at improving economic performance in various dimensions will vary along with the institutions that are available to respond to them.

- Institutions can be analyzed using the same types of rigorous theoretical and empirical methods that have been developed in the neoclassical tradition while recognizing that additional tools may be useful to better understand the development and role of institutions in affecting economic performance.
- Theoretical and empirical analysis should be interactive and evolve together over time. Theory identifies relationships that can be examined empirically while empirical regularities and “anomalies” raise questions about the relevance of received theory and suggests new targets of opportunity for theoretical advances.
- Interdisciplinary research can make important contributions to understanding the role of institutions and how they affect economic behavior and performance. Contributions from history, law, psychology, anthropology, sociology, religion and related disciplines can play an important role in advancing our understanding of institutions and their impacts on the economy and the consequences of economic policies.
- Longer term dynamic considerations associated with technological change, the diffusion of innovations and the impacts of institutions on both should play a more central role in economic analysis.
- Our understanding of institutions should be rich enough to allow us to apply economic theory and empirical knowledge to a wide range of economic, cultural and political settings: developed and developing countries; countries with a range of political systems including variations of the implementations of “democracy”; countries with a range of cultural, religious, ethnic, tribal and family traditions.
- Institutional analysis seeks to understand the role of government and political institutions in policy formation, implementation and economic performance, but it does not itself have a political agenda.

When one adopts a phrase like “new institutional economics” to define a framework for social science research, it is fair to ask how this work differs from “old” institutional economics. It is quite clear that “institutional economics” had achieved a bad reputation among post-World War II academic economists in the U.S. and some other countries. Indeed, the economic research that flourished during this period was, at

least in part, a reaction to the “old” institutional economics that was the focus of economic research in the previous decades. The criticisms of “old” institutional economics, while perhaps not entirely fair, are important to understand. Much of what passed as institutional economics lacked rigorous and systematic theoretical foundations. It lacked comprehensive supporting empirical analysis. It was often country-specific or even case-specific and little effort (or non-credible efforts) to generalize were made. It tended to become politicized and driven by political agendas. The identification of institutional economics with Marxist economic theories and political agendas was especially damaging, though many institutional economists (e.g. John R. Commons) were hardly Marxists. Moreover, as neoclassical economics became the central focus of modern economic analysis, institutional economics became the home of the disgruntled and disaffected critics of the new methods being used in economics and of modern market economies more broadly. We see this no more clearly than in France where a schism emerged between “institutional economists” in university positions and neoclassical economists, often trained as engineers, using mathematical methods and empirical analysis in engineering schools, public enterprises and some research institutes. Clearly, new institutional economics is very different from old institutional economics.

We should recognize as well that the reaction to old institutional economics also reflected its perceived failure to explain the economic issues and problems that were revealed by the Great Depression and the associated failure of microeconomic and macroeconomic policies to bring the world out of the Depression quickly. The consequences of the Great Depression and the difficulties economists and policymakers had in explaining or responding to it brought a new generation of brilliant individuals into

economics seeking to better understand economic phenomena so that economics and economic policy could better serve the interests of the people. From this perspective, new institutional economics may be somewhat more in the position that neoclassical economics was in at the end of World War II. It is a reaction to perceived deficiencies in the state of economic science. But, while there were many outstanding post-World War II economists who remained interested in important foundations and aspects of economic institutions (e.g. Coase, Simon, Cyert, Marshak, Radner, Arrow and others), much of this work was largely ignored by mainstream economists for decades. In this sense, mainstream neoclassical economics may have thrown some babies out with the bath water, though the bath water was not lost forever.

A FRAMEWORK FOR NEW INSTITUTIONAL ECONOMICS

When one seeks to examine the role of “social, cultural, political, and economic institutions” on “economic behavior and performance” one has cut off a very big piece of cake to chew on. As I will discuss presently, new institutional economics has not tried to focus on all institutions that might fit under this umbrella. Nor has it focused on all aspects of economic performance. While the field has been reasonably inclusive, it has also been reasonably well focused. To better understand the (perhaps soft) boundaries of new institutional economics it is useful to work from a more expansive description of the full range of relevant institutions, and the relationships between them, and then to identify the subset of institutions upon which research in new institutional economics has focused.

The most useful framework to work from is the one proposed by Oliver Williamson a few year ago (Williamson 2000). I will make use of Williamson’s

analytical framework here, including a number of adaptations of my own to it. Williamson's framework identifies four interrelated levels of social or institutional analysis (Figure 1).

Level One: Embeddedness or Social/Cultural Foundations. The highest level of the institutional hierarchy encompasses informal institutions, customs, traditions, ethics and social norms, religion and some aspects of language and cognition. This level provides the basic foundations for a society's institutions. These basic social and cultural institutional foundations change very slowly over time, with adaptation periods of as long as a thousand years and no shorter than 100 years.

Level Two: Basic Institutional Environment. This second level of the institutional hierarchy encompasses the basic institutional environment or what Williamson calls "the formal rules of the game." At this level are defined constitutions, political systems and basic human rights; property rights and their allocation; laws, courts and related institutions to enforce political, human rights and property rights, money, basic financial institutions, and the government's power to tax; laws and institutions governing migration, trade and foreign investment rules; and the political, legal and economic mechanisms that facilitate changes in the basic institutional environment. The nature of the basic institutional environment at any point in time reflects, among other things, the attributes of a society's basic social and cultural foundations. In a society in a dynamic equilibrium a given set of basic institutions as this level will be compatible with the society's social foundations at any particular point in time. Changes in the basic institutional environment occur more quickly than changes in the cultural or social

foundations (Level 1), but change is still relatively slow and partially constrained by the slow rate of adaptation of the underlying social and cultural foundations, with response times as short as 10 years but as long as 100 years.

Level Three: Institutions of Governance. This third level of the institutional hierarchy encompasses what Williamson calls “the play of the game.” Given the basic institutional environment, choices are made regarding the institutional (governance) arrangements through which economic relationships will be governed given the attributes of the basic institutional environment. The basic structural features of the institutions (e.g. competitive markets) through which individuals trade goods, services and labor are defined; the structure of contractual/transactional relations, the vertical and horizontal structure of business firms and the boundaries between transactions mediated internally and those mediated through markets; corporate governance, and financial institutions that support private investment and credit, are defined at this level. The choice of governance arrangements is heavily influenced by the basic institutional environment as well as by a county’s basic economic conditions (e.g. natural resource endowments) at any point in time. Changes in governance arrangements also take place more quickly than do changes in the basic institutional environment. Williamson suggests a change time frame of one to ten years.

Level Four: Short-term resource allocation (neoclassical market economics). This level refers to the day-to-day operation of the economy given the institutions defined at the other three levels. Prices, wages, costs, quantities bought and sold are determined here as are the consequences of monopoly, oligopoly and other neoclassical market imperfections. Williamson would include agency theory and incentive alignment within

and between organizations here. I would, instead, consider these arrangements to be more appropriately included under Level 3's institutions of governance.

The division of social, political, legal and economic institutions into four levels is necessarily somewhat arbitrary. However, I think that this qualitative characterization is quite useful. A society's social and cultural foundations place constraints on the attributes of the basic institutional environment that will be feasible at a particular point in time. For example, societies that have no tradition of private property and have relied instead on communal exploitation of resources and collective allocation decisions cannot be expected to adopt successfully the basic institutions of capitalism that characterize the U.S. or Western Europe overnight. Nor will societies with hierarchical non-democratic political systems, easily shift instantly to modern democratic political or human rights institutions (these are positive not normative observations.) Similarly, when certain basic institutions, such as private property rights, centralized monetary institutions, and decentralized credit institutions first begin to be introduced, we cannot simply assume that they will instantly have the same attributes as they do in societies with many years of experience with them. Moreover, the institutions of governance that have attractive allocational and adaptive properties with one set of basic institutions may have different and less attractive attributes with another set of basic institutions. Finally, familiar capitalist market institutions may not work very well if the supporting institutional structure composed of basic institutions and compatible governance arrangements are not in place. Alternative allocation mechanisms may be better adapted to the supporting institutions that are in place at any particular point in time.

Williamson's framework also makes important observations about the speed with which adaptation can be expected to take place. Changes in basic social and cultural foundations take place most slowly and are most "embedded" in the institutions of a society. To the extent that changes to the basic social and cultural environment also constraint the choice of basic institutional arrangements, adaptation at this second level may be slowed as well. Within the boundaries established by the basic social and cultural environment, the basic institutional environment also can be expected to change fairly slowly. This not only places limits on the speed with which the basic "modern" institutions of capitalism will be adopted and work well, but also may influence the most effective intermediary governance arrangements compatible with the state of the basic institutional environment. Periods of relatively rapid change in social and cultural norms and the basic institutional environment can be expected to lead both to rapid change and potentially significant instability in governance arrangements as well. Adapting to rapid changes at these levels can lead to major dislocations and adaptation costs as a society moves forward (or perhaps two steps forward and one step backwards) with fundamental changes at all levels.

Williamson's framework also makes it clear that the speed and direction of changes at these levels is not exogenous or necessarily monotonic. Change is stimulated through two basic paths. First, the performance of the society, broadly defined to include aggregate income and wealth (the size of the pie), distributions of income and wealth (how the pie is shared), the quality of life and its direction of change, the incidence of poverty and starvation, personal and family security, responses to changes in the availability of natural and human resources (driven by natural, human and political

variables) and opportunities for individuals to fulfill their ambitions for themselves and their families will influence the rate and direction of change. Good performance supports the status quo. Poor performance stimulates change, but not always in a direction that makes things better overall.

Second, changes in lower level institutions in the hierarchy can stimulate supporting changes in higher level institutions. For example, increased reliance on long-term contracts between “strangers” rather than reliance on transactions between members of the same family or ethnic group (Greif) may lead to pressures to better define the basic institutions governing enforcement of private property rights and contractual performance. Or the effects may be more indirect. Industrialization may lead to more air pollution and, in the absence of clearly defined property rights and enforcement institutions, or more informal institutions to mediate between those who benefit and those who are harmed by pollution, may create pressures for governments to enact laws to control pollution, effectively deciding who has the property rights to clean air.

Whatever the pathways of change, both the speed and nature of any changes will necessarily be affected by the time that it takes to make significant adjustments in the attributes at the different levels of this institutional hierarchy. Adjustment and adaptation lags and costs become an important consideration in implementing public policies to improve economic performance.

New institutional economics has focused primarily on analyses of aspects of institutional arrangements that fall in level 2 and level 3 of this hierarchy (or both). At ISNIE’s annual conference in 2003 about 85% of the papers presented fell within these categories and were divided roughly equally between them. Only 5% of the papers were

on topics that would be categorized as level 4 (and some of these featured applications of experimental economics), while about 10% involved issues on level 1 of the hierarchy, focused heavily on the role of religion, ethics and social norms. While, a large number of topics can easily fit into levels 2 or 3, the bulk of the research presented at the conference fell into a fairly well defined subset of topics that lie at these levels. Among the papers that fell into levels 3 and 4, the vast majority focused on issues associated with the definition, allocation and enforcement of property rights and their effects on economic performance, contracts, vertical integration and various hybrid organizational forms, privatization, positive political economy, regulation, deregulation and industry restructuring, and competition policies. Most of these papers involved empirical analysis (including case studies) and many focused on developing countries. It is also my impression that there has been growing interest over time in issues that naturally fall into level 1 and their implications for the attributes of the basic institutional environment of level 2.

SUBSTANTIAL PROGRESS HAS BEEN MADE

Looking back over the research in the general area of institutional economics over the last ten or fifteen years, it is clear to me that very substantial progress has been made. There also remains much to do to advance our understanding of institutions, how they affect economic performance, and how they change. Perhaps most importantly, the central role of institutions in understanding economic performance, growth and development and the strengths and weaknesses of alternative public policies aimed at promoting improvements in individual welfare is now widely accepted by the economics

profession. While there remain (healthy) differences in views about which institutions are most important, how they should be analyzed and the relative importance of formal theory, less formal theories, and empirical analysis, research devoted to institutional economics has increased dramatically and has become fairly mainstream. This is a very dramatic change over a period of less than two decades.

Identifying the specific issues upon which the most progress has been made is necessarily a matter of taste. Let me identify my own “top three” areas where I believe substantial progress has been made in the last two decades. In my view, very substantial progress has been made in understanding the definition, allocation and enforcement of property rights in different level 1 and level 2 institutional settings, how property rights affect key attributes of economic performance and, in turn, how the role of property rights are affected by economic performance and other attributes of the social, cultural and basic institutional environment (Libecap and Smith, North 1991, Acemoglu, Alston, Libecap and Schneider). The research here has gone well beyond fairly banal observations like “well functioning markets require credible property rights” to explore more fundamental issues of how property rights emerge, what they mean, how they are enforced, how these rights are limited and adjusted in very different institutional settings. Historical, cross-country, cross-cultural and developing country studies have been especially powerful in developing a much more complete understanding of property rights and their effects.

A second area where I believe very substantial progress has been made is in understanding vertical integration, or the make or buy decision, and associated issues of comparative governance arrangements for commercial transactions (Joskow 2004).

Indeed, from both a theoretical and empirical perspective there is perhaps no other level 3 area that has been worked on so extensively (Williamson 1985, 2000). I will discuss the work on vertical integration in more detail presently. Related research on relational contracting, contract enforcement mechanisms, and hybrid forms has also progressed very nicely from both a theoretical and an empirical perspective.

The third area where I think very significant progress has been made is the area of positive political economy. Important research work here involves both level 2 and level 3 lines of inquiry (Dixit; Weingast and Marshall; Acemoglu, Johnson and Robinson). From a level 2 perspective we have gained a much better appreciation for how the institutions of government, broadly defined to include election rules, legislative, executive and legal institutions, can affect economic behavior and performance and, in turn how economic behavior and performance can affect the basic institutions of government. From a level 3 perspective we have learned a lot about how the structure of government and supporting institutions have evolved to respond to instabilities and various transactions costs associated with pure democracy and as well, the key role of interest groups in determining the behavior of government and political institutions. Related work on the structure, behavior and importance of regulatory agencies and supporting institutions has also progressed significantly (Levy and Spiller).

VERTICAL INTEGRATION AND THE COMPARATIVE GOVERNANCE PARADIGM²

It is not my intention to review all of the research accomplishments that can be (loosely) placed under the umbrella of new institutional economics. Instead, by way of example, I will explore (relatively briefly) the progress that has been made in understanding why firms become vertically integrated (or de-integrated) backward into input production or forward into distribution and retailing (the “make or buy” decision). Understanding the factors that determine which types of transactions are mediated through markets and which within hierarchical organizations called firms has been an important subject of theoretical and empirical work in microeconomics generally and central to work in new institutional economics in particular for at least the last 25 years. Much of this research falls squarely into level 3’s consideration of governance arrangements, focuses on the role of transactions costs (broadly defined) arising from incomplete contracts and relationship specific investments, and adopts the powerful comparative institutional analytical framework. I will refer to this line of research as transactions cost economics (TCE) that is a component of new institutional economics (NIE). Pioneering theoretical research in this general area can be attributed to Ronald Coase, Oliver Williamson, Benjamin Klein and Oliver Hart. Perhaps more importantly, there now exists a vast empirical literature that provides very strong empirical support for, in particular, the transactions cost/comparative governance approach to understanding the choice of organization structure to most effectively govern commercial transactions.

² This section draws heavily on Joskow (2004).

Virtually all theories of vertical integration turn in one way or another on the presence of market imperfections on some type. Traditional approaches to vertical integration have tended to focus on vertical integration as a response to pre-existing market power problems (e.g. double marginalization) or as a strategic move to create or enhance market power in upstream or downstream markets (e.g. foreclosure strategies). While not excluding these rationales for vertical integration, the NIE approach to the analysis of alternative market and internal organizational governance arrangements is much broader. It focuses on a well-defined array of attributes of individual transactions between buyers and sellers of goods or services and how they affect the performance (total cost) of alternative governance arrangements. It recognizes that there is a wide array of governance structures through which transactions can be mediated --- from anonymous spot markets to internal administrative procedures within hierarchical organizations. It recognizes further that the task of consummating transactions must confront a variety of potential transaction costs, contractual, and organizational hazards, which are related to the attributes of the transactions at issue and their interplay with the attributes of alternative governance arrangements. These transactions costs involve the direct costs of writing, monitoring and enforcing contingent contracts as well as the costs associated with the ex ante investment and ex post performance inefficiencies that arise as a consequence of contractual hazards of various types and various bureaucratic costs associated with internal organization.

The transactions costs of particular interest are those that arise as a consequence of ex post bargaining, haggling, pricing and production decisions, especially those that arise as the relationship must adapt to changes in supply and demand conditions over time, though inefficiencies in ex ante investments are also relevant. (Williamson, 1975, 2000) The

governance structures that are chosen, whether market or hierarchical, are those that are best adapted to the attributes of the transactions of interest in the sense that they economize on the total costs (including transactions costs) of the trading relationship.

Contractual incompleteness, and its interaction with the attributes of different types of transactional attributes including asset specificity, complexity, and uncertainty, plays a central role in the evaluation of the relative costs of governance through market-based bilateral contracts versus governance through internal organization. Contracts may be incomplete because of the direct costs of specifying and writing contracts that anticipate all contingencies, because of "bounded rationality" that makes it unlikely that the transacting parties can foresee all possible contingencies, and/or because of high monitoring, verification, and enforcement costs. When transactions are mediated through market-based contracts, circumstances may arise where the buyer and seller have conflicting interests. The potential advantage of internal organization in this case is that internal organizations are likely to better harmonize these conflicting interests and provide for a smoother and less costly adaptation process under these circumstances, facilitating more efficient ex ante investment in the relationship and more efficient adaptation to changing supply and demand conditions over time.

If hierarchical organizations have these attractive properties, why don't we see more economic activity taking place within very large organizations rather than through markets? The answer is that internal organization is good at some things, but not at others. Williamson (1996, Chapter 4) observes that when we look at the bigger dynamic picture, internal organization is a last resort that we turn to only in the presence of significant contracting hazards and associated transactions costs. This is because, opportunistic

behavior associated with specific investments aside, decentralized market arrangements have superior adaptive properties to internal organization in many other important dimensions. For example, employees may be less willing to reveal information that adversely affects their promotion possibilities or continuing employment. The kinds of low-powered incentives that characterize internal compensation arrangements may also mute incentives to exert the optimal amount of worker effort (Williamson, 1985, Chapter 6; Holmstrom and Milgrom, 1990). In addition, while internal organization is likely to be better at removing certain kinds of internal information asymmetries in the short run, it may be an inferior structure for obtaining, processing and using external information about prices, costs, quality, and technological change in the long run compared to repeated market transactions. For example, when a firm vertically integrates (or enters into a very long term full requirements contract) it is likely to lose some of the benefits associated with continually examining and accessing outside opportunities through repeated contracting. These opportunities include information about the "least cost" prices of the goods and services that the firm is producing internally and the availability of new technologies and production methods.

For these reasons, even in the face of significant contractual hazards resulting from specific investments and incomplete contracts, firms may still find it advantageous to continue to rely on arms-length market transactions for all or a fraction of their input or distribution requirements (dual sourcing) involving specific investments rather than turning to complete vertical integration.

The bottom line is that there are benefits and costs of internal organization. Market transactions incur transactions costs associated with writing and enforcing contingent

contracts and the inefficiencies ex ante and ex post resulting from opportunistic behavior that exploits specific investments. Internal bureaucratic allocation mechanisms can help to mitigate these types of transactions costs but incur other types of transactions or organization costs. The costs of internal organization are associated with the relatively inferior adaptive properties of bureaucratic hierarchies to rapidly changing outside opportunities over the longer term and the difficulty of designing compensation mechanisms to give managers and employees appropriate incentives to control costs and product quality. No governance structure is free from at least some transactions costs. The decision whether or not to vertically integrate then becomes a tradeoff between the costs of alternative governance arrangements. Vertical integration is favored when the benefits of mitigating opportunism problems by moving the transactions inside the firm, by reducing ex ante investment and ex post performance inefficiencies, are greater than other sources of static and dynamic inefficiency associated with resource allocation within bureaucratic organizations.

The choice of governance structure and how this choice is affected by transaction cost considerations have attracted considerable empirical study. There have been at least 500 papers published that have examined various aspects of comparative institutional choice from a TCE perspective. A significant fraction of these studies have examined the vertical integration or “make or buy” decision. There have also been several survey articles that have reviewed the empirical literature stimulated by TCE theories, including many related to vertical integration and non-standard vertical contracting arrangements (Joskow, 1988; Shelanski and Klein, 1995; Crocker and Masten, 1996; Coeurderoy and Quélin, 1997; Vannoni 2002).

These empirical studies of vertical integration and how the choice of this governance structure is influenced by the importance of specific investment and other variables that could lead to ex ante and ex post contractual inefficiencies overwhelmingly show that the importance of specific investments is both a statistically and economically important causal factor influencing the decision to vertically integrate. Indeed, it is hard to find many other areas in industrial organization where there is such an abundance of empirical work supporting a theory of firm or market structure. And it is the combination of compelling theoretical analysis combined with a large body of supporting evidence that makes the TCE approach to understand vertical integration and alternative vertical governance arrangements so important.

Does the extensive theoretical and empirical analyses of vertical integration lead us to conclude that the topic has been so well worked over that there is little more to do on it? I believe that the answer is “no.” As Masten, Meehan and Snyder (1991) show (see also Joskow 2004), the empirical tests that have characterized much of the econometric literature on vertical integration are not nearly as powerful as first meets the eye. The primary problem is that the literature has focused primary attention on the causal variables that are thought to affect the costs of market contracting. However, relatively little attention has been paid to the state and dynamic costs of internal organization and the variables that affect these costs. As previously noted, the comparative governance approach teaches us to compare the costs of alternative governance arrangements. By focusing on the factors that affect the costs of market contracting only, we are implicitly assuming that the associated variables do not also affect the costs of internal organization. This may not be a good assumption in all situations. Moreover, most of the empirical research does not measure the

costs of alternative governance arrangements directly, but rather measures the variables (often ordinal) that are thought to influence their relative costs, relying on the revealed preferences of economic agents, revealed through their choice of governance arrangements, to identify the importance of various causal variables.

It seems to me that the empirical analysis of TCE theories of vertical integration can be improved in a number of ways. More attention should be paid to both the attributes and costs of internal organization. Direct measurement of the costs of alternative governance arrangements would also increase the power of the empirical tests. Finally, research that examines dynamic shocks to demand or cost attributes and the associated responses of governance arrangements would also add power to the empirical analyses of TCE theories of vertical integration.

There are also significant theoretical disputes regarding the factors that influence the make or buy decision (Gibbons 2003). Property rights theories of vertical integration (Hart) have attracted a lot of attention, in part because they are more formal than TCE theories. Some view the property rights theories as formalizations of TCE theories. This view is incorrect. Property rights theories focus primarily on the effects of incomplete contracts and specific investments on ex ante investment incentives with or without vertical integration. TCE theories focus on ex post adaptation problems while recognizing that ex ante investment incentives cannot be ignored. The property rights literature assumes that ex post bargaining is efficient. Moreover, the property rights theories' characterizations of what constitutes a firm and the nature of internal governance arrangements is quite different from the nature of firms laid out in TCE and many other theories of organizations. In my view, the property rights approach strips the firm of most of its organizational features and focuses

on how ownership and the associated residual rights of control affect the bargaining power of otherwise self-interested economic agents engaged in bilateral trade. This approach does not allow for any other changes in incentives and behavior of the transacting parties when the relationship is brought from the market inside of the firm (vertical integration). Thus, it largely ignores important differences between market transactions and internal organization other than simply a change in relative bargaining power between self-interested managers (Williamson, 1996, Chapter 4), despite the fact that the objective functions possessed by managers and the incentive and payoff structure that they face are different for managers within a firm as compared to managers in separate firms. Nevertheless, gaining a better understanding of the similarities and differences theoretically between property rights and TCE theories of vertical integration would be very useful.

CONCLUSIONS

One of my colleagues recently suggested that institutional economists had “won the war” in the sense that it is now widely recognized that understanding how institutions affect economic performance and why different institutional arrangements emerge in different social, cultural and economic settings is now widely accepted by economists. It may be that in this sense the war has been won. However, there is still much work to do. As the discussion of vertical integration in the previous section should indicate, even in this relatively well worked over area there are still unresolved theoretical questions and opportunities to improve the quality of empirical analysis. As we consider the state of knowledge in other less well-developed areas it is clear that whatever war has been won there are still many important issues that are targets of opportunity for theoretical,

empirical, and policy oriented research on institutions and their effects on economic performance.

The broad acceptance that “institutions matter” has meant that there are many scholars working on institutional issues from a variety of different perspectives. In my view new institutional economics has not done enough to reach out to the research relevant to institutional economics that has emerged from other fields in the last decade. Perhaps the field has become too insular and runs the danger of being isolated as scholars working in other fields turn their attention to institutional issues. For example, I believe that new institutional economics has devoted too little attention to the details of individual decisionmaking, relying on broad characterizations of bounded rationality and self-interest seeking behavior. Expanding analyses of and integrating research on individual decisionmaking and cognition (psychology) in the presence of uncertainty, imperfect information, and various social and cultural norms --- the focus of the rapidly growing field of “behavioral economics” --- into research on institutions could be very productive (Rabin and Thaler; Thaler; Kahneman and Tversky). Concepts of altruism, trust, and human responses to uncertainty, information, search and cognition costs, clearly have implications for institutional choice and impacts. Behavioral economics can play an important role in accounting for these dimensions of human behavior more directly. Increased use of experimental methods widely used in behavioral economics can help to expand the data available to test hypotheses about the formation of and impacts of different types of institutions.

Much of the work in the comparative governance arena (level 3) traces its origins to work done by researchers identified with the “Carnegie School” in the 1950s and early

1960s (Cyert and March). Concepts of “bounded rationality” articulated by Herbert Simon are central to the analysis of incomplete contracts as well as to recent research in behavioral economics. Efforts to integrate behavioral economics into institutional economics thus will, in a sense, bring institutional economics back to its roots. So too would more research devoted to the structure, behavior and performance of organizations (private, public, for-profit, not-for-profit), their internal structures, their behavior and performance. These were topics of particular interest to the Carnegie School, which has somehow attracted much less attention than it deserves in the comparative governance literature, as I noted in the previous section.

It is also my view that new institutional economics has had too limited an impact in the public policy arena. Some of the problems that have emerged in the privatization, restructuring and deregulation of electric power networks during the 1990s can be traced to a failure to incorporate learning from new institutional economics into the restructuring and market design process (Joskow 1996). This reflects, in part, the heavy reliance on “economists” trained in engineering and operations research who have no appreciation for the subtleties underlying simple economic principles and the importance of institutional economics considerations. In the area of economic development policy it has become routine for policymakers to trumpet their recognition that institutions matter and that development policies must be tailored to the institutional attributes of the particular countries to which they are applied. In practice, however, these institutional considerations are often ignored and policy prescriptions often continue to reflect the application of developed country concepts to countries with very different level 1 and level 2 institutional environments that also imply effective level 3 governance

arrangements that may be quite different from those that characterize developed countries. There is very exciting academic research going on in the field of development economics (Banerjee and Duflo; Banerjee and Munshi; Banerjee and Iyer) that can and should be integrated into the work on new institutional economics as it applies to developing countries, helping to move policymakers away from banal prescriptions for developing countries that ignore relevant developing country institutions.

New institutional economics gets a very good report card in most dimensions, but there is still are important intellectual challenges and a lot of interesting work to be done.

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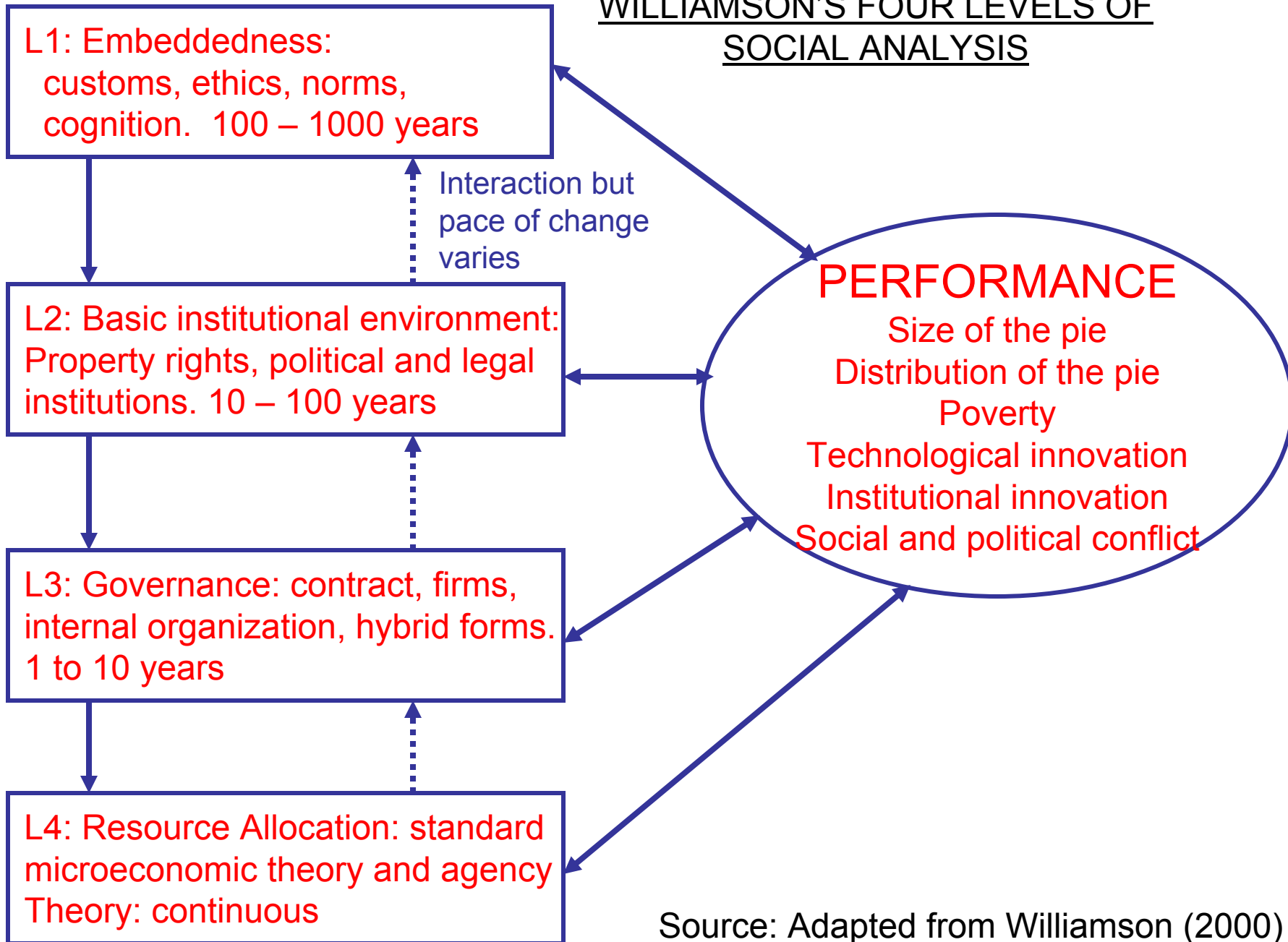
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FIGURE #1

WILLIAMSON'S FOUR LEVELS OF SOCIAL ANALYSIS



Source: Adapted from Williamson (2000)