METHODOLOGICAL ISSUES

IN

NEW INSTITUTIONAL ECONOMICS

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I: Introduction.

After a long wait on the runway, with Coase in the cockpit, New Institutional Economics took off spectacularly over the last two decades and is now evolving as a very progressive research program. A coherent set of concepts has been elaborated, with transactions and their costs at the core, and empirical tests and applications have proliferated at a pace rarely observed in economics.

As with any innovative research program, New Institutional Economics has its flaws and weaknesses. Most critics have focused on methodological issues. Heterodox economists attack primarily the assumptions made, particularly the idea that economic agents are in some way calculative, although in a more restrictive sense than in standard models since they are assumed to have a bounded rationality, and that they tend to find solutions that minimize costs. Mainstream economists criticize the lack of mathematical models to support the reasoning and contribute to testable predictions, an implausible critique in light of the remarkable set of empirical tests and analysis already available in New Institutional Economics.

While these critiques are at the background of this paper, I will set in the foreground several issues of major significance for the future development of New Institutional Economics. In doing so, I rely on a representation of scientific activity in which progress depends on the simultaneous support of three legs. It requires: (1) a

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1 For a quite extensive overview of recent developments in New Institutional Economics, see Menard (2000). A more personal survey of the field is developed in Furubotn and Richter (1995).
theory, i.e., sets of questions and concepts to explore these questions; (2) models, i.e., tools rooted in theory and designed to generate predictions about a class of phenomena; and (3) tests, which usually involve measurement, to determine whether facts behave according to predictions. In the next sections, I develop these three aspects. My views are in part a dialogue with Uskali Mäki (1999), who deals with similar questions.

II: Theorizing.

One central characteristic of New Institutional Economics is that it is based on a small set of concepts that are logically coherent and that provide powerful tools for delineating questions to be explained and for shedding light on a large set of facts and relationships among these facts. Insofar as posing relevant questions and providing a pool of resources for addressing these questions are keys to building theory, then New Institutional Economics is well on its way. An illustration is the stream of research developed to account for vertical integration through the analysis of transaction costs.

Transactions and their related costs are at the core of the theory and constitute the force that unifies the sub-fields that have developed. Coase (1999) rightly emphasized the importance of transactions and their centrality in the research program promoted by neo-institutionalists. Without transactions and their relevant organizations, it would be impossible to take advantage of division of labor or of innovative technologies. In that sense, there is a primacy of transactions over conditions of production. The second key feature of the theory is that organizing transactions involves costs. This long ignored and still largely underestimated factor has major consequences. In a world of positive transaction costs, the allocation of resources and the development of new technologies depend on the prevailing governance structures, i.e., the modes for organizing transactions, and on the characteristics of users’ rights, particularly property

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2 But the usual disclaimer obviously applies.
rights\textsuperscript{3}. Hence, as Mäki noted, incorporating positive transaction costs necessarily involves institutions \textsuperscript{4} and, as a result, changes both the identification of questions that are relevant to economics and the understanding of the main causal chains. This may be why transaction cost economics, an other name for New Institutional Economics that emphasizes its analytical core, is often viewed as an alternative theory. Note that an alternative theory need not render existing theories obsolete; but it does “regionalize” them into a more complex scheme, and may restructure them\textsuperscript{5}.

However, there are at least two major weaknesses in the existing NIE theory, and I suspect that further exploration of these issues will require the development of new concepts and some methodological remodeling (I come back to this last issue later on). The first weakness concerns how we relate the analysis of transaction costs to the dynamics of innovation. Indeed, the development and extension of transactions are a key factor that prompt the search for new resources and new techniques. But what type of transactions and, in particular, what modes of organizing transactions, at both the microanalytical level and the level of the institutional environment, are most favorable to the development of capabilities and to the dynamics of innovation? Innovation is a black hole that neo-institutionalists share with other economists and other social scientists, and one that they have tended to underestimate. A second major issue concerns the interaction between institutional environments and governance structures. Thanks to Ronald Coase, Douglass North and many others, we have an increasingly precise identification of the institutions that matter most in the development of

\textsuperscript{3} This delineates the main streams of research within New Institutional Economics: the economics of governance structures (Williamson, 1985), the economics of institutions and institutional changes (North, 1990), and the overlapping issues of measurement (Barzel, 1989) and property rights (Alchian, 1977; Demsetz, 1988). Many other names should be mentioned here.

\textsuperscript{4} Hence the title of Coase’s lecture for the Nobel Prize (1991) : « The Institutional Structure of Production ».

\textsuperscript{5} To illustrate : the alternative theory that developed from Tycho Brahe and Copernicus to Newton did make the geocentric theory of the Ptolemaists obsolete, but it integrated and reinterpreted results such as those from Archimedes.
transactions (e.g., the regime of property rights and contract laws). But we know very little about the mechanisms through which the rules implemented by these institutions diffuse to governance structures and contribute to the shaping of how transactions are organized. Therefore, we know very little about comparative costs of different institutional schemes (e.g., the cost of running different kinds of judiciary systems for implementing contractual laws). We can expect that these issues will become increasingly important in the coming decades and will require cooperation of economists with specialists in legal systems and with social scientists.

**III: Modeling.**

Lack of adequate models is the main critique leveled by mainstream economists against New Institutional Economics. This has been changing rapidly over the last decade at the edges of the research program, in those areas that overlap with more conventional approaches, e.g., the analysis of incomplete contracts within the context of an extended rationality (Hart et al.) and the analysis of the internal structure of the firm (Holmstrom et al.). Nevertheless, the central core of New Institutional Economics does lack adequate models.

Models are essential because they transform central concepts, which are highly abstract and apply to a very large class of phenomena, into explanations with a predictive power for more limited sets of specific phenomena. That is, models are necessary intermediaries between the development of a pure theory and its application to the analysis of empirical facts. They are also crucial in determining the capacity to measure, a central goal in science ⁶. From this point of view, two characteristics are of particular importance: the predictive power of propositions established by a model; and the extension of the set of “facts” to which these predictions apply.
Models can be expressed in mathematical forms, but need not be so. Because the use of mathematics is now so fashionable in our discipline, too many economists ignore this. But the history of science is full of models that remained in purely common language. In a book of physics from the 18th century, or even well into the 19th century, there are few mathematical models. Modern biology has even fewer. Most mathematical models in New Institutional Economics are within the microanalytical branch, i.e., the study of governance structures. Even in this area, they remain quite primitive. When it comes to the analysis of the global rules underlying the organization of transactions, we are left with almost no mathematical models, although we do have rigorous frameworks of analysis on which we can base predictions and for which we can develop measurements.

Certainly, developing more adequate models is a priority in New Institutional Economics. But how can this be done? On one hand, we can try to adapt models developed in mainstream economics, particularly microeconomics, to encapsulate the essence of the analysis of governance structures. But these models have several prerequisites, e.g., the rationality of choice makers, the continuity of the set of possibilities for organizing transactions, or the underlying determinism, that do not fit the observable characteristics of either governance structures or economic behavior. The situation is even more complex when it comes to the analysis of institutions. Hence the hope that Game theory could help, with its emphasis on rules and their accompanying beliefs. Although I personally think that this avenue is worth further exploration, most new institutionalists are not optimistic about the possible contribution of game theory. One concern involves what Sugden (1991) identified as the difficulties of the rationality

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6 Science requires measurement. But measurement is not science: in the history of science, there is an almost infinite number of examples of measurements without theoretical foundations. Counting the saints identified by the Catholic church does not prove that Paradise exist!

7 For an exceptionally extensive effort in this direction, see Aoki (2000)
assumption. In his survey, he emphasized the problems generated by the transfer of assumptions initially developed for the analysis of games against nature to the analysis of games among agents who behave strategically. Most of New Institutional Economics is precisely about the latter. One last possibility lies in the development of experimental economics. So far, the field seems to have expanded largely in testing rationality hypothesis and in elaborating complex schemes for resolving the resulting paradoxes. Further developments may provide new insights better adapted to the understanding of how institutions actually shape beliefs and choices.

To summarize, we can predict the multiplication of models inspired by standard microeconomics in the field of New Institutional Economics. But it is also clear from the start that these models are not satisfactory. Alternative approaches such as game theory still need to demonstrate their relevance. On this issue, we can hope for progress only from an extended dialogue between new institutionalists and model builders. The dialogue is difficult because the former are critical of the deterministic view of most model builders, while the latter tend to stick to assumptions that are convenient but by far too restrictive to cope adequately with the questions raised by New Institutional Economics. Methodologists can help in developing the dialogue.

IV: Testing.

When it comes to empirical testing and analysis, New Institutional Economics is a “success story”, as noted by Joskow (1991) and emphasized continuously by Williamson. Indeed, there have been hundreds of tests of transaction cost economics over the last two decades. This is quite paradoxical, if we consider the small number of models available. Or maybe it is precisely because the theory is expressed in simple models that so many tests have been made possible. Moreover, one can expect an acceleration of the process. Indeed, collecting relevant data was a major issue in the
1980's. As noted by many historians of science, e.g., Kuhn (1962) and Canguilhem (1968), the development of a new theory always requires the collection of new data that will be adequate for new tests and that were not available in the former theoretical environment. New Institutional Economics is no exception and the pioneers of empirical studies (Anderson, Demsetz, Joskow, Palay, Masten, North and Wallis, and others) had to build new sets of data from scratch. With the development of the theory and its diffusion, more and more data are becoming available, although collecting adequate information remains a challenging issue for many aspects of the research program (e.g., the study of the internal structures of organizations, or of the costs of running different types of institutions). Based on these data, three modes of testing have been used. Each raises specific problems.

The dominant form of testing so far is along the line of standard procedures developed in economics, namely statistical evidence (e.g., North and Wallis, 1986) or econometric tests. A body of evidence is building progressively, but two major problems emerge. One is the collection of data, since so much is required both at the microlevel and at the level of the institutional environment. The second is the requirement of more refined concepts in order to make possible the collection of relevant data. Examples are the concepts required for the analysis of contracts, or for measuring the degree of specificity of assets involved in transactions, or for determining the degree of uncertainty surrounding a transaction or a set of transactions. We need to define better proxies, which supposes more detailed and better defined concepts.

In the process of doing so, case studies and the related building of “stylized facts” can be of great help. Economists do not like case studies. The reasons for that spontaneous rejection are obscure, and seem to be rooted in the dead ends of empiricist

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8 Some of the earlier essays are collected in Masten (1996). Klein and Shelanski (1995) provide a survey of the most significant essays published in English in the 1980’s and early 1990’s.
movements such as the historical school in Germany or the old institutionalism in the US. On the other hand, this is quite paradoxical considering both the development of economics and the general history of science. In economics, cases have played a major role in the breakthroughs that shaped the discipline, from the fable of the bees in Mandeville and the pin manufacture in Adam Smith to the auctions in Walras. In the exact sciences, innovators have worked and often still work on very specific cases. Paleontologists do not need thousands of fossilized skeletons to build a theory of evolution. What matters is that the case be relevant to the exploration of a theoretical question.

Two types of case studies can be distinguished. One has to do with the construction of a stylized fact and is intended to provide an in depth analysis of a specific question and of related explanatory concepts. A classical example in New Institutional Economics is the case of Fisher Body and its integration by General Motors in 1926. The study of this case has provided major insights into the factors that can push towards transactions and has recently provoked a sharp controversy about the determinant factor, with Coase challenging the role usually attributed to the risk of hold-up\(^9\). One problem with specific case studies is that detailed facts can always be found to question the prevailing explanation; hence the necessity of a robust theory to direct the interpretation of these facts. One other avenue is the development of comparative case studies. These are particularly relevant in New Institutional Economics because of the need to deal with a limited number of discrete modes of organizing transactions, both at the microlevel and at the level of the set of institutions that characterize a society. Such comparative approaches have been extremely fruitful in other disciplines (e.g., in studying the comparative morphology of languages). A growing set of studies in New Institutional Economics proceed in a comparative way, either in analyzing the trade-off
among different governance structures (many econometric studies already mentioned are of this kind) or in examining and explaining the impact of different institutional environments on the modes chosen for organizing transactions (Levy and Spiller 1994; Menard and Shirley, 2000). What is essential to the success of this approach is that a limited number of variables be isolated and kept under strict control by the researchers as they proceed to the analysis. This is difficult to do, and methodologists can help by applying the lessons from similar approaches in other disciplines.

One other method that has been explored much more recently in New Institutional Economics is experimentation. My knowledge of the field is much too limited to describe these developments here. But I am aware of at least two types of experiments that I find particularly stimulating and that may help us to better understanding the development and functioning of institutions that frame transactions. One is the study of market rules, as initiated by Vernon Smith. The other is the analysis of rules and beliefs that structure the behavior of transactors 10. Unfortunately, too much of experimental economics to date has been devoted to the exploration of the assumptions of the rational choice theory and to the resolution of the resulting “paradoxes”.

V: Conclusion.

The research program in New Institutional Economics is evolutionary, with rapid changes occurring; and it is progressive, in that the building blocks have a cumulative effect, with each part helping to better understand other parts. Like any new research program, it confronts difficult methodological issues. This is particularly so because it “de-isolates” (to quote an expression of Uskali Mäki), questions that were set aside by standard economics (e.g., the concept of the firm as a governance structure

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9 See the set of four papers in the April 2000 issue of the *Journal of Law and Economics*

10 For an illustration in the field of New Institutional Economics, see Jean Ensminger, in Menard (2000)
rather than a mere production function, and the concept that the institutions of exchange have positive transaction costs). Another difficulty arises from the search for more realistic assumptions about economic behavior, which makes model building much more difficult. To overcome these difficulties, we hope for and expect help, both from other social scientists and from specialists in economic methodology.

REFERENCES


