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Article

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Interview with John Nye

John Nye is a Professor of Economics at George Mason University and holds the Frederic Bastiat Chair in Political Economy at the Mercatus Center. He is a specialist in new institutional economics and economic history. He was a founding member of the International Society for the New Institutional Economics. With John Drobak, he co-edited *Frontiers in the New Institutional Economics, 1997*. His articles have been published in a variety of journals including the *Journal of Economic History*, *Journal of Money, Credit and Banking*, and *Journal of Economic Behavior and Organization*. His book on trade war, *War, Wine and Taxes*, appeared in 2007 from Princeton University Press.

How did you personally get affiliated with the new institutional economic theory? What attracted you to this field?

This is a good question. I actually began my career as a straightforward cliometrician. I was applying economic ideas and econometric techniques to the study of economic history. But my first job was when I was hired as an assistant professor in the Economics Department at Washington University in St. Louis. There I had the chance to work with Douglass North and many other people who played important roles as founders of the new institutional economics. Washington University at that time (it was the late 1980s) was a centre of the modern work in political economy and the new institutional economics, and particularly, in political science and economics. Barry Weingast, James Alt, Ken Shepsle, Itai Sened, and Gary Miller were all there. I was also hired around the same time as other young scholars such as Jack Knight in Political Science and Jean Ensminger in Anthropology. It was like a small club. Later on Norman Schofield, Gary Cox, and Matt McCubbins joined us. All of us had a strong interest in applying economic ideas to general problems in the social sciences. We all started to become sensitized to the importance of politics, the subtleties of properly creating rational actor models, the strengths and weaknesses of standard neoclassical analysis and a broader interest in social behavior, legal rules and competitive behavior under conditions of uncertainty. We had a very lively weekly seminar series and over time most of the active participants in what became the new institutional economics passed

through St. Louis. For instance, Ronald Coase, Oliver Williamson, and Elinor Ostrom spoke several times at Washington University in just a few years time. Also people like Robert Fogel, Joel Mokyr, Vernon Smith, and Avner Greif came regularly to Washington University.

Finally, in 1997 a decision was made to start a new organization which became the International Society for New Institutional Economics (ISNIE). The early organizing group included Lee and Alexandra Benham, Douglass North, Ronald Coase, Oliver Williamson, Mary Shirley, Claude Menard, Scott Masten and myself as well as a few others. After these early meetings came the organization. It was important that there was a long discussion about what was to be included in the NIE and we decided to take a "big tent" approach. There was no single methodological system that we felt was going to be the determining part of the NIE. The feeling was that there were independent schools of thought all working in parallel and overlapping ways. There was research into organizations and hierarchies by people like Coase, Williamson and Harold Demsetz. But also there was political economy work by those who were inspired by North or Olson and there was overlap with the newer, more formal work in political science pioneered by Riker, Shepsle...

And economic history?

And economic history, exactly. We also had people in political economy like Barry Weingast. Of course, James Buchanan and Gordon Tullock were to be considered part of the group although Buchanan did not participate initially. However Tullock spoke at the very first meeting

What would you say about the positions of Douglass North in this heterogeneous group at that time? Was it before he got his Nobel prize?

It was after.

So he was already famous. What was his position given he was less formal than people like Oliver Williamson?

Coase is less formal too. Coase has always been not very formal. In fact that is one of the things that united the new institutional economics, whereas in much of economics people are united by methodology. If you look at mainstream work first of all it has a specific technique. There are certain kinds of models and a common type of statistics. And the subject does not matter as much. In contrast, the NIE was more united by subject matter. It is the feeling that you want to start from the core of the neoclassical theory but it expanded to include concerns about social behavior, about politics, about legal and social institutions, about psychology and history. But how we got there was going to be very heterogeneous. So we were more like fellow travelers. We all were interested in similar questions. But we did not all have the same interests or methodologies. There was always a feeling that standard microeconomic theory should be somewhere in the core as opposed to some early work in the old institutionalism which tended to reject or disdain standard economic theory. The NIE took for granted that standard economic theory was important.

So, was it a sort of an extension?

Yes and no. There were debates about that. You have many people, say, like Williamson, whose work is closer to an idea of extending the mainstream. North on the other hand was willing to more directly challenge a lot of things. He was willing to go beyond that. And there we had various debates. To some extent, economics itself had also been evolving with increasing interest in behavioral economics and psychology. Economics is moving in this direction but there is still a debate about how much of the NIE is just an application of standard theory to different areas or how much is a parallel movement to the core theory and other approaches which are really outside the standard theory.

Regarding the subject areas, your personal interests are very broad and spread from agricultural trade policies and alcohol taxes to human superstitions and gambling. Are there any subject areas that are increasingly

popular among the institutional economists? What are the areas they mostly focus upon? Or they just can take anything...

They really can take anything. But if you want to talk about the core trend there are two things. There is what I call the classical core of institutional economics. And I really think the core splits up into two groups. In the first one you might think about Coase, Williamson, and organizational theory issues. In the second group we have North, political science, political economy, development studies with the focus on politics, the state and the evolution of regulation in history and historical trends. They represent the two broad general tendencies.

More recently, I think, thanks to the broader interests, there are certain trends that are shared with economics. First, there is a general interest in experimental work of all kinds. Both laboratory experiments and also field experiments and randomized control trials. Economics itself has become more interested in empirical work. Especially empirical work that extends the scope of current theory by asking: How does this specific institution change what they think of the theory? What part of the theory needs to be changed because of psychological and behavioral issues? That is one area that is getting more and more interesting.

Second, people do not give up caring about the role of the state in development. There are huge debates about it. Further, I think, there is always going to be a lively debate about what the boundaries are between individual behavior and socially constrained behavior. You can think about these boundaries from a variety of perspectives. For instance, both the psychological literature and also the literature on experiments is all about trying to understand how human capital -- for instance people's abilities, people's intelligence, and people's personalities -- affect the institutions they create. And conversely, how do the institutions that are created either change or modify or expand people's natural abilities. So, it is an interesting debate. And it is tied to issues in politics.

For instance, if you debate most issues in the economic history and in the new institutional economics such as the role of corruption, there are a lot of very interesting questions that get raised by the new work. How much of corruption is a function of a weak state which itself could be a function of history, experience, and inherited rules? How much of the corruption is easy to remove just by changing

a few formal rules versus by changing people themselves? It is a huge old debate in the social sciences. And these are the things we do not really understand. What kinds of rules have a bigger effect on people's behavior? What kinds of rules depend on the kind of people you deal with? I guess it is extremely intriguing to think about this. Let's say that we have two groups in society which are very-very different by language and culture. How easy is it to bring them together, to share the same set of institutional rules? How important are the choice of enforcement mechanisms in creating the common culture? When we create the common culture, how difficult is it to change that back? Many things that we can think of might work well or poorly in different societies. Or become a function of preexisting conditions.

On the other hand, there are cases in which similar fairly stable social dimensions can be changed very rapidly. If you think about religion: on the one hand, religion seems like a very long-run process outside of economics. On the other hand, there are a lot of cases where religion changes very rapidly. My former colleague Jean Ensminger who is now at Caltech (she is an anthropologist) did a lot of work on Africa. She studied the cases in which African societies adopted Islam initially because of the success of Islam in providing certain institutions like courts and law that these small African societies did not have. But then of course the adoption of Islam itself has longer consequences which go beyond the instrumental issues. So initially they may have adopted the particular religious structure for fairly instrumental reasons. But the subsequent generations are influenced by being under this longer tradition and that of course changes the whole of society. We need to understand what is useful or limiting about narrow rational choice models. Sometimes, people seem to respond in ways that are very consistent with mainstream neoclassical theory. But having made those choices leads to what looks like non-rational choice effects in terms of preferences, in terms of future orientation, in terms of the way the society views its evolution, etc.

Actually, you have mentioned a lot of issues which are relevant for sociology in general and economic sociology in particular. Do you see any noticeable connections and mutual engagements between two related fields – institutional economics and economic

sociology? For instance, in the U.S. are there any visible connections?

I think there are some. I am not an expert in this field so I hope you forgive me if I do not know a lot of names in these areas. But I do think in my naïve understanding of sociology that there is a lot of very good work in sociology especially in areas like demography. Another area is organizational studies. And there is a lot of work on issues of ethnicity and identity that I think are potentially very important for economists. Similarly, economics is very useful because one of the things that makes economics both powerful and in some ways unpopular among sociologists is the attempt to fit everything into a universal economic model (sometimes unsuccessfully). We have a coherent set of more generally accepted theoretical frameworks. In some sense my feeling is that sociologists have more competing methodologies and more competing theories than economics. Even if many people disagree with the core model, economics has made a much more unified view of the individual rational actor model as a starting point for discussing social phenomena. I also think economists probably have the best developed mathematical apparatus for looking at statistical problems in terms of issues of endogeneity or issues of distinguishing between competing statistical claims. I think some ways which we can talk across borders areas are very-very important.

I have some difficulties when I read the sociological literature. I notice that there are two classes of sociological research. Some sociological research even when its theories are very different from economics is like economics in that it is fundamentally positivist. It is about finding out rules of social relationships that are independent of preferences or independent of ideology. In contrast, I occasionally read sociologists who start from an explicitly normative position and mix up what I consider as positive scientific statements with critical and ideological stands that start out by talking about certain behaviors as being incomprehensible or undesirable or ideologically suspect. I think this is where we have the most difficulty and end up talking at cross purposes. But I think more generally there are a lot of areas in which we look more and more at the same things, though there are of course differences in terms of methodology.

Strangely enough, the new institutionalism in economic sociology in the 1980th started to borrow a lot

(though reflexively) from the new institutional economic theory, especially from organizational theories. Although sociology of organization existed long time before, after two or three decades there was some sort of turnover. As for economists, still there is an impression that they do not pay much of attention to what is going on in economic sociology. I remember I recorded an interview with Oliver Williamson in Haas Business School (it was probably eight or ten years ago). He was rather critical about what economic sociology was doing and at the same time did not pay much of attention, as he said, to their critique (and it was a lot of critique of Williamson from sociologists at that time).

This is not my area of expertise because I am not an organizational theorist. But my feeling is the following: I really think that sociologists have found a lot of very interesting things but I think there is a gap that makes it hard to talk across the fields. Again, it might be naïve. I have seen work by people like Smelser and Granovetter that try to speak to economists more directly. But I also have seen work where they do not take seriously enough the role of market competition. So very often you will have a sociological study which looks like a case-study or series of case-studies based upon the observations about the way in which various organizations behave or various social actors within the organization behave and respond, and their social motivations. And often the author will note that the behavior doesn't seem to be very "economic". But for an economist, an interesting issue that must be asked is which peculiarities of social motivation may directly affect the assumption of profit maximization and which are irrelevant for profit maximizing behavior? That is the critical question. In other words, to say that a firm is profit maximizing does not mean that all individual actors are profit maximizing. It is a standard economic critique to show that such and such a group deviates from a simplistic conception of profit maximization, but for an economist, it is possible for firms to behave in a profit maximizing way even if every single actor in the firm doesn't seem to consciously conform to the rational actor model. Firms might still behave AS IF its individuals all conformed to the economic model even if none of them do.

If you interview, say, gasoline station owners and you ask them about how they price gasoline, nobody would know about supply necessarily. They are not economists, they do not understand the theory of marginal cost. What they will often say is something about "cost plus." They will say that they take whatever cost they paid plus a profit to determine their selling price. But of course these owners can not be right because there are many situations which say otherwise. Let us say you bought gasoline at four dollars a gallon and you normally add fifty cents. But the price suddenly fell down to three dollars and you cannot charge 4.50, you have to suddenly adjust to the market price of 3.50. It happens so fast that on average if you would look at what gasoline owners do over the course of a year, ninety nine per cent of the time it looks like this rule of cost plus really works. But the mistake that's easy to make is an erroneous inference about the economically relevant behavior. The marginal behavior affects theoretically the crucial behavior. And this is a big problem for sociologists and economists. Economics has a problem about understanding when consciousness matters. Competition means that often conscious decision making is not the correct guide to AS IF firm behavior. I would like to see this issue addressed more directly by all researchers.

That is why I stressed the focus on experimental work. Why is that? A lot of work from people like Daniel Kahneman and Amos Tversky but also Vernon Smith shows that in experiments humans deviate often very strongly from many of the economics' models in specific environments that they are put in the laboratory. However, as Vernon Smith has also pointed out there are a lot of cases in which even if people are behaving in a way that is not economic, when they are put in a competitive situation in which profitability is determinative they are forced to behave as if they are all rational. So the interesting question is when does it work that way? What aspects of, say, the sociology of big organizations are functions of the lack of competition or bad regulation or political rent-seeking. And conversely, what are so important aspects of human psychology that they would change behavior even in competitive markets. Issues like discrimination are very important. They cannot be understood just from interviews on discrimination., We need to know how much of discriminatory behavior is despite competition or because of the lack of competition. This is a very big example.

It is similar with identity. An interesting issue for economists is if the construction of person's identity is going to matter. So for economists there is a big difference when

people construct something like identity in the way that has no economic costs. Whether I care or not if the Coca-Cola label is red or blue or something equally superficial. From the economics standpoint, that is not very interesting if some people would say they prefer red Coca-Cola bottles to blue Coca-Cola bottles. It becomes more interesting if people would pay a very high price because they like red Coca-Cola bottles. They like red Coca-Cola bottles so much that they would pay an extra twenty cents for red Coca-Cola bottles over blue bottles. That point would be interesting. And how they pay the price is very important – whether in the form of cash, or willingness to wait in line, or willingness to avoid certain colors and why certain colors might tie in to how people see themselves. Now economists do not tell us why these social identities matter. And I think again that psychologists, sociologists and anthropologists have a huge amount to say about this. Notice, when they say these things, at some point it must be anchored to the question: when do these preferences change market behavior? I think it is a dialogue that needs to be stronger. And it is that dialogue that people like Williamson find hard to see in the literature.

Let us turn to Oliver Williamson and Elinor Ostrom who recently became Nobel Prize winners. It was well expected in the case of Williamson. And we can say that many economic sociologists read a lot from Oliver Williamson and criticized him a lot. He was chosen as a major target for many of sociological critics. As for Elinor Ostrom, I am not sure that she attracted much of attention before though her studies could be even closer to research interests of economic sociologists. So what can you say about the importance of her works?

To begin with, I think a lot of economists were surprised that Ostrom was selected. Because she is not an economist, she is a political scientist. But even if she is very famous as a political scientist, she has not been an important figure to mainstream economists. However, many people in the new institutional economics have long admired her work. In this sense Ostrom's work is very important. It was a clear departure for the Nobel Committee. And selecting her was very important for the Nobel Committee.

Let us think about why her work is interesting but also a challenge. And why it is so different from the kind of work that economists do. Ostrom's work is very sociological and anthropological. And a lot of this work is about observing real-world societies' response to the problems that economists care about. She is addressing the classic economic problem of the tragedy of the commons and the problem of free riders. But what is unusual about Elinor Ostrom, is her focus on a methodology that does not rely on big statistics or on big math but on making careful case-studies of different behaviors and societies around the world and on classifying them. And this is the kind of thing I think Ronald Coase has been arguing for in economics. But it's not popular in economics. It is hard to get published...

Why?

This is an interesting issue. Classification was a big part of early biology. That is to say that classification and labeling is the first step to theorizing. But economists -- for internal sociological reasons, if you like, -- have eschewed classifications. They think they are atheoretical.

And it is also true about typologies while sociology is so interested in typologies.

Exactly! Economists are not interested in typologies. And that is another area where sociologists and economists are different. And that makes Ostrom a very good bridge to sociology because she does care about typology. But it is interesting that typology has not penetrated mainstream economics.

Perhaps, economists are always too quick to look for generalizations. When they see a certain case they think what is the relevant generalization? They immediately ask two questions: first, can we formalize mathematically that generalization? And second, can we test econometrically that generalization? When they cannot do A or B they are less interested. I am not sure about all of them but Douglass North is a little bit like that. North has a lot of ideas that were not initially formal. But you could read in North's work claims that let somebody else make a formal theory or develop a statistical test. So pure typologies tend to leave economists cold as they do not find them interesting. But in my view, the virtues of Ostrom's work are to show

that so much theory is premature. Until we have good typologies and until we have more cases it is often too difficult to build theories. So I think the weakness of a lot of economics is an over-eagerness to do theory too early. The great mathematician von Neumann who was a big believer in mathematical economics and a great theorist said that economists have too quick tendencies to mathematize things without understanding the underlying theory.

I think Ostrom does good typological work. And this is a role she is trying to play. So what is particularly important in Ostrom's work is a contribution to understanding where do people overcome the free rider problem even without the state and even without very good formal rules. That is one point. The second point is that she also asks an interesting question: when are the solutions that people spontaneously come up with good for a small group but bad for a wider community or bad for the state. I think both these issues are very much present in Ostrom's work. I would also emphasize that Ostrom is very aware of the literature in game theory and experiments and in econometrics. Some of her work draws very strongly on game theory and she has a lot of work tied to experiments and to formal literature. In this sense, even if her work is eighty percent outside the standard methodology she is very careful to tie up the last twenty percent to the other work that people have done. And I think, if we are going to see more successful interaction between economics and sociology, we need to see work that addresses more points that concern both groups. For example, we need to get work that has a very good typology, very good social observation and very rich theory combined with a little bit more rigorous formalism and more statistical testing. And even if you do not test it yourself can you explain what test needs to be run to make yourself wrong? This is the part which is very important and this is often missing.

When I read works in other disciplines, sometimes I get a lot of good ideas and I agree. I read a lot of stuff from everywhere: biology, education, sociology, anthropology, demography. And the hardest thing for me when I read something is when it seems like a good explanation but does not really help us to come up with theory. In other words, it is satisfying as an ex-post description but it is not satisfying as a systematic way of thinking. It does not ask: How do I know when I am wrong? That is the hardest problem I have.

So it could not be falsified.

Yes. Though it need not be falsified in a naïve Popperian way. I think this is also misunderstood sometimes. When economists speak of falsification, I do not mean we always mean Popperian falsification. We try to put it in a broader sense by asking the question: how do you determine that something is more or less correct?

I personally have argued that new institutional economics should adopt a legal standard. What do I mean by legal standard? It is a way the we approach evidence. When you make a bigger argument, some parts of the argument can be done technically: simple econometrics, mathematics, etc. Some parts need pure explanations. Some of them are more historical. But you still need a sense of what the competing alternatives are and how one can decide between two big ideas -- which is more accurate and which is more correct. Here, I think, we can improve the dialogue between sociology and economics. When we come up with new positions we should ask ourselves: what are the set of questions we will answer that will help us to distinguish between competing methodologies or competing paradigms? Then we will get richer theories. Sometimes I think there are no competing paradigms; there are just two parts of the same problem. So the economists might be interested in mechanics of market transformation whereas sociologists might be interested in the structure of value creation or the structure of internalization of beliefs or things like that.

Parts of the same process?

Of the same process. They are just asking different questions.

Ok. We know more or less the classical names new institutional economics. But can we point to some new names which rose up within last decade and are promising to become famous in the field?

I would say that a big theme certainly is the way the mainstream economics has embraced the part of the new institutional economics agenda. If you look at the popularity of North's ideas the first names that come to mind are Daron Acemoglu, Simon Johnson, and James Robinson. Acemoglu, Johnson, and Robinson have done a lot to revive an

interest in the NIE in mainstream economics. So even if economists did not care much about people like North, they suddenly started caring. People like Acemoglu, Johnson, and Robinson started using history combined with econometrics to look at bigger issues in a way that was methodologically satisfying for economists.

I would say that today the development literature argues a lot about institutions and think that they are very important. There is a lot of disagreement about their different roles but nobody thinks they are unimportant. That is a very big change. If you look at the growth and development literature in the 1950s and 1960s, it was heavily driven by a macro consensus which was very Keynesian and which dismissed the role of institutions. And I think the fact the development scholars today think of institutions as a critical part of development is a huge change. Nobody who looked into the literature of forty or fifty years ago can doubt that this is a gigantic change.

I remember reading a volume of the 1950s, from the National Bureau of Economic Research. It was a survey of economic growth and it had representatives from all over the world including USSR, Italy, France, Sweden, and the United States. And they all talked about growth in their countries and not once did I see any serious discussion of legal institutional differences. For growth and development I read that the only differences between Sweden and the USSR or Italy and America were in unemployment, labor statistics, capital formation. Today we have a big change where things like rent seeking or regulation are considered to be very important.

Secondly, there is a lot of the work in political economy which includes both the public finance literature and also the work by political scientists like Kenneth Shepsle, Barry Weingast, and William Riker. Plus the work in public choice by James Buchanan and Gordon Tullock, and Mancur Olson is increasingly reappearing in formalized theory in the new political economy. And particularly, I will point to the names of Alberto Alesina, Torsten Persson, Guido Tabellini, Tim Besley. These are names that come up a lot and these are very important. Let me add Andrei Shleifer and Edward Glaser – these are very top economists who have brought new institutional economic ideas to the mainstream. So on the one hand, this is very-very good. On the other hand, there is a limitation that sometimes they do not care about the ideas that are not so easily formalized. This is a tension. Where institutional economics still plays a role is to emphasize the importance of ideas themselves. Whereas the

mainstream top journals in economics still care only about good ideas that are in the right methodology. Because if it is in the wrong methodology (not formal enough or not econometrically advanced), they do not care how good the ideas are.

Finally, I think that field experiments are very exciting and interesting. If you look at the kind of things that John List is doing in Chicago, they have strong overlaps with anthropology and political science and sociology. He looks at everything from differences between male and female behavior to adjustments in the way people buy things. He looks at very rigorous arguments and runs field experiments. When he runs field experiments it is very similar to sociological techniques in some ways. But it has many ties to the economic literature. And it gives you a lot of new insights that are quite different from what was done before.

Let me turn to the last point though very important one. If we take the last decade, what papers or books in new institutional economics would you point at as most stimulating, even path-breaking and at the same time interesting and relevant for economic sociology? What would you recommend to read?

Well, I will certainly say a couple of things. One of the books that is interesting for historians and economists and sociologists would clearly be Avner Greif's book "Institutions and the Path to the Modern Economy: Lessons from Medieval Trade" (Cambridge University Press, 2006). That has been a highly celebrated volume. Avner Greif's book on economic history is a famous book looking at the role of Maghribi trade in the eleventh century and comparing it with the commercial trade of city states like Genoa in the twelfth century. Greif considers the deep problem of comparing personal exchange as a solution to long-term contracting vs. interpersonal exchange tied to more formal institutions. However, what is interesting about Greif is that while reading psychological, sociological and anthropological literature and tying these questions together he also tries to fit his findings into the framework of game theory (though only with partial success). Some things he gets well but sometimes, in my view, he tries too hard. It shows you the difficulty but also the challenge of trying to bridge multiple methodologies. Precisely because he tries and still gets interesting results that book has been very

influential. Not everybody is happy about this book. But this book is a very good example of a good attempt to combine methodologies. And I would recommend it.

The book I would also recommend (it is much older and was published almost twenty years ago but I still use it in my courses) is a book written by my former colleague at Washington University – Gary Miller’s “Managerial Dilemmas” (Cambridge University Press, 1992). This book is very good because it is one of the best surveys of organizational ideas that combines work both of political science and economics and even talks about sociological ideas and about what major issues are faced in solving the problems of organizational hierarchy. But it also talks about what questions neither political science nor economics answers and where you have to go to with sociology or anthropology or psychology to answer some questions that have been left open by economics. Even though some of these issues have developed new answers by now, I think the way Miller handles different issues is very profound. And I think this book is unfairly neglected. It should be a much more famous book.

Right. And some more recent studies?

I have been very interested in the work of the sociologist Victor Nee. He has done a lot of work and that is important. Also I think of work of Neil Smelser and Richard Swedberg on economic sociology of capitalism. Both are interesting to read...

Yes. It’s widely known in sociology.

I also think that the work on experiments is important, for instance, that was done by the huge team led by Joseph Henrich, Colin Camerer, and Jean Ensminger. It was a team of economists, sociologists, anthropologists in which they went to various rural and primitive societies all over the world. They played dictator and trust games and they compared the behavior of people in these experiments. They found interesting results that the closer people have been to commercial exchange and to markets the more generous and giving they are in the trust game or in the public goods games. And this is an important issue of asking how the type of economic interaction you have had affects behavior even in games where the incentives to free ride are strong.

So game theory is applied to anthropology?

This is certainly right. The general ideas come from game theory and then anthropology gives us the answer to how people differ. We have a structured framework in which we can compare answers from different fields. So it is game theory plus economic games. It is part of what I am thinking about now – how very simple games can be combined with more standard work to ask deeper questions about personality, behavior and preferences which we did not pursue before.

Thank you very much!