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Note from the editor

Dear reader,

It is a great pleasure to take editorial responsibilities after Nigel Dodd and all previous distinguished editors who have done an excellent job. And I am thankful to the Editorial Board for this opportunity to contribute to the development of the *Newsletter*. I have been publishing the *'Economic Sociology' e-journal* for a Russian speaking audience for more than eleven years. And I fully understand how difficult it is to provide both sustainability of the project and high quality of the papers.

When planning this work I analyzed the contents of the *Newsletter* over the past years. There are plenty of good papers and interviews bringing light to many areas of economic sociology and related fields including anthropology, geography, law, and accounting. Surprisingly, we have only one review devoted to economic theory in volume 9 with the exception of numerous highlights on performativity approach. I believe that in doing economic sociology we should pay more attention to what is going on in economic theory.

Keeping this in mind, I decided to devote the first issue to the state of the art in contemporary economic theory. As the field is very broad and extremely diverse, we will focus on a single, increasingly influential stream of research which should be of special interest for economic sociologists, i. e. new institutional economics. The idea is not to confine it to a regular critique of economists' failures and limitations of their analysis but to consider their assumptions and present their most recent achievements. Most of economic sociologists know quite well the classic works of Oliver Williamson and Douglass North. But the field is moving, and it is important to see major trends and emerging areas overlapping with economic sociology.

We start with an interview recorded with John Nye. He is a representative of a younger generation of researchers applying the ideas of the new institutional economics to a great variety of subject areas. At the same time John Nye belongs to the group of scholars including Lee and Alexandra Benham, Douglass North, Ronald Coase, Oliver Williamson, Mary Shirley, Claude Menard, Scott Masten who established the International Society of New institutional Economics in 1997. John speaks on a diversity of methodological approaches, an increasing interest in experimental work, continuous debate on the role of the state in development. He explains why Elinor Ostrom was selected by the Nobel Committee and how classifications and typologies could be a bridge between economics and sociology. Finally, most important studies in the NIE are recommended for sociologists.

Then a brief but comprehensive review of the state-of-the-art in the new institutional economics is presented by a group of institutional economists led by Leonid Polishchuk. It was written specifically for the *Economic Sociology Newsletter* and selects issues which could be relevant for economic sociologists. The paper highlights the current agenda and major areas of research with special focus on recent studies. Among the topics you will find property rights allocation and rent-seeking, impact of institutions on economic development, formal and informal institutions, norms and trust. The survey tends to demonstrate that NIE is a natural field for inter-disciplinary collaboration between economists and sociologists. References to most important literature are provided.

Our next author, Olivier Favereau is well known as one of the founding fathers and prominent figures in French economics of conventions which is viewed as one of alternative approaches to the institutional analysis of economy and society. Favereau provides a reflexive external view on the new institutional economics confronting this research program with that of economics of convention. He found many important similarities at the level of key assumptions and demonstrated how they are used to produce rather different conclusions and motivate quite different styles of research. Special attention is paid to divergent approaches to the model of bounded rationality.

In the last paper János Mátyás Kovács reflects upon controversies in the reception of the new institutional economic theories in Eastern Europe. Large-scale importation of the new institutional economics (NIE) was predicted at the beginning of postcommunist transformation in Eastern Europe experimenting with deep-going institutional change. Textbook Marxism was vanishing while hardcore economics was not widely spread. Under these conditions the NIE and Ordo liberalism could be seen as good options for many economists and sociologists. Surprisingly, Eastern Europe was not flooded with the NIE concepts. Moreover, the

NIE still has rather low profile among the economists in the region giving way to rather eclectic research programmes. Using results of a research project carried out in eight countries, János Kovács gives a comprehensive picture of this peculiar transformation in economic and social sciences.

Beyond the main topic of this issue, a new book of Philippe Steiner *Durkheim and the Birth of Economic Sociology* is reviewed.

Finally, we announce a big conference in economic sociology which will take place in Moscow in October 2012.

I would like to thank all the contributors to this issue for their productive efforts. Let me stop at this point. And please meet the new institutional economics.

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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 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 someting 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!

New Institutional Economics: A State-of-the-Art Review for Economic Sociologists

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Introduction

According to a widely accepted definition by Douglas North, institutions are "rules of the game" in the society and economy which reduce uncertainty and structure behavior and interactions of economic agents. The notion of institutions is perhaps one of the most "sociological" among economic concepts, as it applies to agents' dealing with each other (there could be no institutions in a Robinson Crusoe economy), and incorporates a number of inherently sociological categories, such as networks, norms, customs, etc. It should therefore come as no surprise that the work of first institutionalists, such as Veblen and Commons, had a distinct sociological flavor. It took another several decades for mainstream economists to fully appreciate the key role of institutions – not just economic, but also legal, political, etc, for economic performance and development. This understanding heralded the advent of what is now known as the New Institutional Economics (NIE).

Unlike the early institutionalism, NIE is a deductive and empirically grounded discipline, which shares the key tenets and premises of the economic way of thinking, such as allocation of scarce resources, individual incentives and rationality, competition and market exchanges. However NIE is a major departure from the "institution-blind" neoclassical approach in that it recognizes transaction costs as a fundamental feature of economic exchanges. In the presence of transaction costs efficiency of market equilibria can no longer be assured, and institutions as devices for transaction cost management are highly relevant for social and economic outcomes. NIE maintains and expands the conceptual links of the early institutionalism to sociology, law, political science, anthropology and history, and provides a natural basis for the integration of economics with the rest of social science and humanities.

The thematic scope of NIE is vast, imprecisely defined, and evolving, which makes it difficult to produce a comprehensive state-of-the-art survey. In deciding which subject areas to include in this survey, the authors were guided, in the decreasing order of importance, by the following considerations: (i) their understanding, likely imprecise, of which NIE topics would be most interesting for economic sociologists; (ii) their perception of the saliency of particular fields; and (iii) their own professional interests and areas of expertise.

Our coverage also reflects the agenda of the latest annual conference of the International Society of New Institutional Economics (http://www.isnie.org/isnie2011.html) held at Stanford University on June 16-18, 2011, which provided a good snapshot of the state of the discipline.

Micro and macro perspectives

NIE operates at the micro and macro and levels. Micro analysis covers the impact of institutions on economic transactions and behavior, organization of economic activities, contracts, allocation of property rights, etc. The focus of macro analysis is the link between institutions and economic development, social welfare, and public sector governance. Sometimes NIE combines micro and macro perspective; i.e. property rights are essential both at the micro and macro levels. Another example are informal institutions, such as behavioral norms – being essentially a micro phenomenon, they are highly relevant for macroeconomic development, and in their turn depend on development patterns, trends, and outcomes (Lipset, 1960; Tabellini, 2008).

Institutional dichotomy

Institutions are expected to have the nature of public goods (or public production inputs) facilitating production and exchange. According to the classical view of institutional change, institutions emerge spontaneously in response to grassroots demand at a time when their benefits exceed the associated costs (Demsetz, 1967; Eggertson, 2001). The modern views are less sanguine, distinguishing between socially productive and unproductive activities;

both are individually rational and hence worth pursuing, but the former contribute to economic growth and social welfare, whereas the latter are 'negative-sum' games. Socially productive activities require level playfield and secured property rights, and hence the institutions that support such activities are known under the generic name of *property rights institutions* (Acemoglu et al., 2001). Synonym for unproductive activities is rent-seeking, and the institutions that favor such activities are known as *institutions for rent extraction*.

Large theoretical and empirical literature attests to critical significance of secured property rights for economic development; an equally impressive body of evidence points out to the detrimental effect of rent-seeking for growth. While secure property rights facilitate deployment of economic resources, first and foremost economic and human capital, for productive purposes, rent-seeking reduces payoffs to productive investments and attracts resources into socially unproductive usages (Murphy, Shleifer, Vishny, 1993). Unchecked rent-seeking is particularly harmful for the allocation of talent and entrepreneurship in the society (Baumol, 1990, Murphy, Shleifer, Vishny, 1991), dragging creative entrepreneurs and gifted individuals from conventional innovations a la Schumpeter and wealth creation more generally into socially wasteful re-distribution of wealth.

Although institutions for rent extraction are detrimental for the society at large, they could be beneficial for privileged elites, in which case the latter use their leverage to establish and sustain such institutions. This public choice perspective explains the emergence and persistence of limited access order (North e al., 2009), when collected rent is used to stabilize the society and control violence. Such arrangements are common in the modern world and prevailed throughout the recorded human history. Its inner stability notwithstanding, *limited access order* offers lower living standard and suppresses economic development; furthermore it restricts entry into the polity to the privileged class. Open access order, while being much less common, is conducive for growth and development: it maintains social order through political and economic competition, rather than rent creation and re-distribution, and is characterized by free entry into political and economic organizations.

Mechanisms and driving forces of the transition from limited to open access order continue to be debated in the NIE literature. One strand of research (Acemoglu, Robinson, 2006a) maintains that at certain times ruling elites' positions become less solid, and to prevent violent regime change, the elites share political power and economic resources with the masses. An alternative theory (Lizzeri, Persico, 2004) maintains that democratization could be appealing to elites since it improves the quality of institutions and public policies and hence offers an 'insurance' to various elite groups in the event they lose out in the inter-elite competition.

Institutions and development

An important and vibrant area of the NIE is the relation between institutions and economic development. Up until mid-XXth century economic development was perceived as primarily a resource accumulation problem. While modern development views do not reject the prima facie view of financial, physical and human capital as necessities for economic growth and social welfare, they consider accumulation of such resources endogenous to institutions that could either facilitate or suppress private investments.

Of particular significance among such institutions are secured property rights and contract enforcement; other plausible institutional determinants of economic growth are the strength of the rule of law, quality of economic regulation and public sector governance, and the protection of political rights and freedoms. While the importance of property rights and effective and impartial 'administration of justice' for investments and commerce were recognized at least since Adam Smith, it was not until mid-1990s that this conjecture was put to an empirical test. Such testing was made possible by a proliferation of various measures of institutional quality (Kaufman, 2010) which could be related to the rates of economic growth and other key economic outcomes.

The available data reveal strong correlation between the quality of institutions and economic development; this lends support to the *institutional hypothesis* which maintains that 'institutions cause growth'. More sophisticated econometric tests are used to confirm that causality indeed goes from institutions to development and to rule out the omitted variable bias. Usually such tests involve so-called instrumental variables – exogenous factors that affect institutions and hence economic outcomes, but have no immediate impact on economic development and welfare which is not mediated by institutions. Usually such instruments are found in history and/or geography (see the "History and Institutional Origins' section below) and indeed confirm that "institutions rule", being the most salient and powerful development driver over all other potential causes of economic growth.

Such evidence notwithstanding, a contrarian *development hypothesis*, which puts the causality in reverse and maintains that good institutions are not a cause but an outcome of economic development, also finds support in data (Glaeser et al., 2004). This hypothesis originates in sociology (Lipset, 1960) and reflects the development model whereby the accumulation of wealth and especially human capital makes the society more prone to consensus-building and improves its capacity to agree upon and jointly implement mutually beneficial policies leading to the establishment and upholding of efficiency-enhancing institutions. Supporters of this hypothesis point out that the mere enactment of formal institutions of democracy and market economy does not in and of itself ensure the desired outcomes, and that real allocation of power and resources often remains invariant to such changes (Acemoglu, Robinson, 2006b). In particular, democratic reforms when implemented in immature societies could frustrate their purpose and be easily subverted, if not reversed, by dominant elites. In such cases an authoritarian regime could present an appealing alternative, providing that such regime has the incentive to advance economic development, and not just to enrich itself.

NIE and political economy

The 'grand debate' between the institutional and development hypotheses illustrates the political economy dimension of the NIE. The centerpiece of such analysis is the impact of political institutions on the incentives of economic and political elites that control policy decisions. Inspired by Adam Smith's famous metaphor, McGuire and Olson (1996) proposed the concept of the 'invisible political hand' that improves economic decisions of an authoritarian ruler, bringing them closer to the public sector needs even in the absence of democratic accountability. The basis for such affinity is the ruler's concern about preserving his tax base, which requires adequate provision of public production inputs and moderation in setting tax rates. There are two important pre-conditions for the 'invisible political hand' to work: first, ruler's interests must be 'encompassing', i.e. covering all of the economy, and second, the regime's duration should be sufficiently long, to allow investments in the physical and institutional infrastructure to recoup and make them more attractive than grab-and-run-type behavior (Shleifer, Vishny, 2002).

The importance of the 'encompassing' condition is illustrated by the 'institutional resource curse' – vast natural resource endowments are shown to adversely affect the quality of institutions in less-than-perfect democracies (Karl, 1997). An explanation of this phenomenon is as follows: in resource-rich countries economic interests of the elites are usually in the resource sectors, which also generate a bulk of budget revenues. This weakens the taxation-representation mechanisms which are pivotal for well-functioning democracy; furthermore, elites' incentives to supply general-purpose institutions and public production inputs serving the interests of the private sector at large get weaker, since resource industries are less-dependent on such institutions and the latter are thus neglected by the elites as having lower priority. Furthermore, institutional distortions caused by the predominance of resource industries reallocate human resources away from productive activities towards rent-seeking (Mehlum, Moene, Torvik, 2006).

Encompassing interests could narrow the gap between the social needs and those of the ruling regime, but do not eliminate it completely. An important cause of the remaining discrepancy is the political risks of efficiency-enhancing modernization and institutional reforms (Acemoglu, Robinson, 2005). Such reforms often enhance economic rights and freedoms, improve access to markets, advance competition and social and economic mobility and hence destabilize the status quo ante and might cause a regime change. Therefore ruling elites could block progressive institutional changes even if those make the economy, including the part controlled by the elites, bigger, because the appropriation of such gains, and for that matter even the preservation of the status quo, could no longer be assured. Bargains between the ruling class and the rest of society, in the spirit of the Coase theorem (see below) could not unlock the stalemate, since such bargains would not be enforceable after a regime change (Acemoglu, 2003).

Endogenous property rights

Market-augmenting institutions, such as secured property rights, could be firmly grounded in political tradition and culture, and protected by elites and society alike. Without such firm foundations of the rule of law (more on this below in the survey) property rights protection becomes a decision variable of the ruling class and are thus endogenous. NIE studies the political and economic incentives that could sustain or undermine endogenous property rights. Without such incentives, declared commitment to secured property rights (and more generally a promise to maintain enabling conditions for private enterprise) could suffer from dynamic inconsistency and is likely to be discarded by investors as 'cheap talk' lacking credibility.

According to the preceding section of this survey, long tenure of the ruling regime could favor the provision of endogenous property rights as an institution that enhances the regime's tax base. Property rights are upheld as an equilibrium in a 'repeated prisoners' dilemma' (Besley, Ghatak, 2010): by honoring property rights and refraining from expropriation the regime maintains its reputation and avoids sanctions of private investors who would respond to violation of their rights by ceasing to invest further.

However such trigger strategy alone could not be sufficient to discipline a ruler who is not politically accountable to the society and private sector. The power of such incentives could depend inter alia on economic trends (Polishchuk, 2011): the threat of investors' boycott could be a strong argument in a rapidly growing economy, but is unlikely to make an impression against the backdrop of an investment slump.

Another factor that could uphold endogenous property rights is a degree of political competition, even if in indirect or surrogate form. This can be illustrated by the concept of market-preserving federalism (Weingast, 1995): when subnational units have to compete for mobile investments, they are compelled to offer better investment climate and in particular higher security of property rights. Such competition increases the costs of anti-market institutions and policies, even the latter serve the ruling regime's immediate self-interest, and this could tilt the cost-benefit calculus in favor of endogenous property rights.

Competition between elite groups, even if not of conventional democratic kind (when various political forces vie for voters' support), could still also improve endogenous property rights. Indeed, recent studies (and dramatic development in the Middle East) indicate that long tenure of authoritarian regimes does not necessarily improve the quality of institutions and economic policies (Polishcuk, Syunayaev, 2011). One reason is that the real prospect of losing power makes the present regime more interested in preserving the rule of law, competition, and property rights protection, even if this restricts today's opportunities for plundering the private sector, since those in power today could be ousted tomorrow and need the above institutions as a protection from the predation of the new powers-that-be.

Formal and informal institutions

Coordination of economic activities which is the main task of institutions can be accomplished by both formal and informal means. Formal institutions, such as laws, regulations, courts, government programs and agencies, are supplied by the state. Informal institutions serve similar purposes, and from this viewpoint formal and informal institutions are substitutes for each other. This is the essence of the famous Coase theorem (Coase, 1960) considered as one of NIE's cornerstone. According to the theorem, interested parties could prevent a market failure and achieve Pareto optimality by agreeing to coordinate their actions and share the accruing gains. This alternative to government regulation is feasible only if the transaction costs necessary to achieve and implement such agreements are low and do not excessively reduce, let alone exceed, the gains from collaboration.

Informal institutions which reduce transaction costs are those that facilitate collective action (Olson, 1965) and include norms of behavior, trust, and social networks, collectively known as social capital (Woolcock, 1998). The main purpose of social capital is to assist economic agents in attaining superior outcomes (over those that can be achieved unilaterally) through a concerted effort. Such ability indeed reduces demand for government coordination and hence could serve as a substitute for publicly provided formal institutions.

However, the relation between formal and informal institutions cannot be reduced to substitution alone. Proper performance of formal institutions is not guaranteed simply by their enactment (Acemoglu et al., 2008) and on many occasions de facto allocation of power, resources and economic roles remains unaffected by institutional reform, no matter how profound and far-reaching such reform could be de jure (Acemoglu, Robinson, 2006c). It is often incumbent on an institution's beneficiaries and users to ensure its proper performance and prevent manipulation and misuse of the institution (Polishchuk, 2008). Upholding and protecting formal institutions poses another collective action problem which also requires social capital to be resolved. Thus functional democracy and the rule of law are contingent upon broadly shared values and beliefs known as civic culture (Almond, Verba, 1963; Weingast, 1997), and hence social capital-like informal institutions are required for proper performance of formal ones, such as democratic governance. Therefore formal and informal institutions could also be complements to each other.

The ambivalence of the relation between formal and informal institutions explains the "paradox of social capital" (see e.g. Putnam, 1993; Aghion et al. 2010): in societies with low stock of trust and other social capital ingredients there is strong grassroots demand for greater government control and regulation (due to the inability to ensure pro-social behavior at the grassroots), and yet the very same societies often display low esteem or even contempt of government due to its poor performance and abuse of power which the society is unable to prevent. Outcomes of various combinations of formal and informal institutions can be measured by the *'costs of disorder'* which accrue due to insufficient coordination and regulation, and *'costs of dictatorship'* caused by government control, suppression of economic freedom, distorted incentives, and abuse of power (Djankov et al., 2003b). The institutional possibility frontier in these two axes characterizes the (in)ability of society to manage these costs and find an acceptable tradeoff between the two. Configuration of such frontier and its distance from the origin (where both costs are nil) depends on the social capital stock: high social capital reduces the need in government intervention (and hence the costs of disorder) and keeps the government accountable, thus reducing the cost of dictatorship.

Relevance of property rights allocation

Simplistic reading of the Coase theorem could lead to the conclusion that allocation of property rights is immaterial for economic efficiency. Indeed, even if such rights are initially assigned wrongly so that asset owners cannot put their assets in the best possible use, the subsequent trade agreed upon as a part of a Coasean bargain would correct such misallocation. NIE provides both theoretical and empirical evidence that initial property rights allocations matter and the ability of markets to fix a biased ownership structure could be severely restricted.

In their seminal paper Grossman and Hart (1986) point out to incomplete contracts as a reason of property rights relevance. If investments into privately owned assets cannot be made parts of a contract (e.g. because such investments are non-verifiable and hence non-contractible), then ex post re-negotiation would not be sufficient to fully compensate for assets misallocation. This observation proved to be instrumental in explaining boundaries of the firm, and in particular mergers and breakups. Another application of this reasoning is a theory of the outsourcing of government services to private sector firms (Hart et al., 1997). The advantages of such outsourcing are stronger performance incentives of private firms that could deliver better value for money, whereas the flipside of this advantage is the risk of cutting costs at the expense of lower quality of provided services. The criterion of privatization of government services implied by the Grossman-Hart theory of property rights is whether the quality of such services is satisfactorily verifiable or not – in the former case outsourcing is a good idea, but in the latter it could be counterproductive.

Privatization of formerly state-owned enterprises is another example of the importance of the initial allocation of property rights. In Russia in the early 1990s it was maintained that the main objective of privatization was to make market reform irreversible and create a solid political base for the new economic order (Boycko, Shleifewr, Vishny, 1995). Accordingly a bulk of the national economy was transferred into private hands in a matter of several years in an often chaotic and non-transparent manner. An efficient capital market and fully secured property rights did not ensue however due to excessive concentration of production assets in the hands of 'robber barons' known as the *oligarchs*. The oligarchs used their clout to prevent the completion of institutional reforms (Hellman, 1998, Polishchuk, Savvateev, 2004), and the failure to obtain an efficient property rights regime was in agreement with recent development of NIE (Williamson, 2000).

History and institutional origins

Having firmly established the strong link between institutions and development, NIE moved to explaining vast cross-country institutional differences, it turned to history in the search for institutional origins. A good illustration of this strand of literature is the inquiry into the legal origins. The divergence of legal families between the common (Anglo-Saxon) and civil (continental) laws is attributed to differences in allocation of power in pre-medieval England and France (Glaeser, Shleifer, 2002). Once established and sustained, these systems have had distinctly different impact on economic development.

Existing studies indicate that in Europe early institutional framework already favored development. North and Weingast (1989) find that English institutions for many centuries established checks on sovereign government, and such limited government was conducive for institutional development. To explain how Europe came to develop growth-promoting institutions one needs a comparative perspective going beyond Europe alone. Blaydes and Chaney (2011) compare European and Muslim worlds and in particular the patterns of military recruitments to explore the roots of institutional divergence. In 1000 CE the Islamic world was more economically advanced than Western Europe, but failed to develop the rule of law or parliamentary institutions (see also Kuran, 2008). In Europe wealthy individuals served as mounted military elite and were compensated for their service to the king by land grants, whereas Muslim rulers relied on the mamluks - elite military slaves characterized by cultural dissociation and personal dependence on the sultan. They were unable to transform themselves into a landed aristocracy, because the mamluk status did not pass on to descendants. This appeared to be a critically important distinction that prevented the formation in the Muslim world of the civil society serving as a check on monarchs.

Establishment of the property rights institutions (or transition to the open access order) requires suppression of 'political losers' that benefit from rent-extraction institutions, or means to co-opt them in a new institutional regime. Institutional change which is an outcome of such 'elite pacts' and other similar arrangements could facilitate reaching an efficiency-enhancing institutional consensus. Financial innovations could serve as a case in point (Jha, 2010): the issuance of shares in joint stock companies aligned the interests of disparate groups in England and facilitated the nation's transition from monarchy to representative government in the XVIIth century; similarly the introduction of shares in overseas companies helped generate a broad coalition that successfully challenged executive control and implemented public investments that were crucial for growth. The new financial and corporate governance institutions thus played an important consolidating role creating a shared interest in protecting property rights.

Norms and their origins

NIE combines the economic and sociological perspectives of behavior. Economists believe that behavior is driven by incentives, whereas sociologists emphasize the importance of norms. The two approaches are blended in more sophisticated models of behavior whereby individual preferences reflect not just 'ends' (consumption bundles, income, social and economic status etc.) but also 'means' (i.e. actions) leading to such outcomes. Internalized norms wired in preferences cause anguish if behavior is inconsistent with a person's identity (Akerlof, Kranton, 2000), and decrease the utility from material consumption. Such preferences make pro-social behavior individually rational even if the behavior is sub-optimal on purely materialistic grounds. Sometimes identity is not fully known to an individual and/or to those surrounding him or her; in that case identity could be 'managed' and its (self) perception updated based on actions; this creates additional incentives to behave pro-socially (Tirole, Benabou, 2011).

Norms exhibit significant stability across generations, in large part because of their transmission in the family from parents to children (Bisin, Verdier, 2001). Thus, studying trust levels of USA citizens, Algan and Cahuc (2010) showed that inherited trust of descendants of US-immigrants is significantly influenced by the country of origin of their forbears. According to Nunn and Wantchekon (2009), slave trade is responsible for differences in trust level across African countries. Fisman and Miguel (2007) studied the driving patterns of U.N. diplomats in New York. Because of immunity, only cultural norms would force representatives of different countries to follow parking rules. The number of parking tickets of diplomat is shown to be strongly correlated with corruption in their home countries.

Education is a powerful creator of social capital (Helliwell, Putnam, 2007; Natkhov, 2011). Tabellini (2010) uses literacy rates at the end of the XIXth century and historical political institutions to explain the differences in civic values and trust today. Better educated and more intelligent people are shown to be much more cooperative and civic-minded, consistently with Lipset's (1960) views. Recent studies show that teaching techniques may be relevant as well: working in student groups accumulates trust and cooperative attitude (Algan et al., 2010), whereas a purely 'tutorial' format erodes trust and cooperative attitudes.

Historical upheavals and dislocation could adversely affect pro-social norms, as illustrated by the above mentioned lasting impact of slave trade on social capital in Africa. Closer to home, post-communist transition to market economies is shown to diminish the social capital stocks in Central and Eastern Europe and the former Soviet Union (Aghion et al. 2010). Other studies focus on cross-regional comparisons of areas which were affected by civil wars. Conversely, historical experience of self-governance can lead to accumulation of trust. Putnam (1993) and Guiso et al. (2008) explained differences in civic values and activism across regions of Italy with the historical experience of democracy and self-rule in the Italian city-states.

Measuring informal institutions

To measure social capital one can look both at 'inputs', which are common values, beliefs or dense social networks, and 'outputs' which are participation in associations and widespread civic norms (Guiso et al., 2010). Associations and group memberships and civic behavior are classical measures of social development employed by civil society scholars from de Tocqueville to Putnam. Participating in non-profit organizations, clubs and other associations, people reveal their ability to act collectively for achieving common goals, and therefore group membership statistics could be a good proxy for social capital. Unfortunately, it is very difficult to separate *'Putnam groups'* that advance broad societal interests from *'Olson groups'* which pursue narrow interests by means of rent-seeking.

Another and perhaps more reliable source are various sociological surveys that measure values, attitudes, and behavioral patterns. The best known of those are the periodic World Values Survey and similar data collection programs in the US, Europe and elsewhere in the world. Trust is the most popular and most studied measure of beliefs and culture used in economic literature. The trust scores across countries correlate with other related indicators. Importantly, trust levels seem to be very persistent – their changes since the first wave of the World Values Survey in 1981 were minor in relation to other economic and institutional indicators.

Social capital and norms could also be gauged by voting and referenda participation, philanthropy, blood donation, newspaper readership (indicators of citizens' interest in local problems), compliance with laws and regulations, and other similar measures. Finally, and increasingly popular source of data are laboratory experiments, such as various trust and public goods-like games. However economists treat such measures with some caution, as they can be influenced by other situational and environmental factors such as legal enforcement, supporting government programs, economic payoffs, etc.

Norms, trust and development

Attempts to explain the difference in economic development across the globe and history reveal a 'missing link' that is being filled by informal institutions. The increasing number of empirical confirmations of the relevance of informal institutions is consistent with their role as substitutes and complements of formal institutions, as argued earlier in this survey. Keefer and Knack (1997) were the first to show that trust level in a country is an important determinant of its GDP per capita and investments in the national economy. A large literature that followed (reviewed in Bjornskov, 2009 and Halpern, 2005) confirms the importance of social capital-like factors for economic growth, government efficiency, institutional performance, quality of life and life satisfaction, etc.

Thus, Tabellini (2010) demonstrates the influence of culture on the development of European regions. Self-expression values form (constitute) another important group. Gorodnichenko and Roland (2010) argue that the individualism-collectivism dimension of culture (Hofstede, 2001) is the most robust cultural determinant of growth and innovations, while other values seem to be less important. Individualist culture is conducive for innovation and growth, whereas the collectivist one facilitates consensus and collective action. These two traits support resp. dynamic and static efficiency, and their interlay and relative merits and demerits require further research.

Another widely accepted, but still not properly researched, fact, is the importance of social networks for development. Networks nurture trust, used to disseminate vital labor market information and make individual reputation publicly known (Coleman, 1988), thus strengthening incentives for collaboration even in one-shot prisoners' dilemma-like situations. Greif (1993) argues that social networks allowed Maghribi traders to conduct complex trade in Mediterranean in XIth century which otherwise would not be possible. Theories of network formation will help to incorporate sociological theories of strong and weak ties (together with in- and out- group trust, general and limited morality) and estimate their impact on development. Greif and Tabellini (2010) attribute the difference in paths of institutional and societal development between Europe and China to their distinct social structures and cooperation mechanisms and patterns.

Network membership is also a basis for collective reputation that is shown to have a strong impact on economic behavior – positive collective reputation strengthens incentives for investments in human capital and behaving prosocially, whereas a negative one has the opposite effect (Tirole, 1996). Such effects explain a number of real-life phenomena, including racial discrimination, deviant behavior, etc. (Akerlof, Kranton, 2000).

Recent advances in social networks economics build theories of network formation based on standard microeconomic concepts of utility maximization, incomplete information, and risk aversion (Kovarik and Leij, 2009). An important direction of such analysis is the spreading of social norms and behavioral patterns through networks, and (in)stability of such network-supported norms to local fluctuations, as observed in minority-influenced effects vastly exceeding their initial causes. Liu et al. (2011) investigate 'driver nodes' that could control the network's entire dynamics. They show that sparse inhomogeneous networks are the most difficult to control, but that dense and homogeneous networks can be controlled and manipulated by using just a few driver nodes. Such analyses explain why norms could be stable in some societies and exhibit considerable instability in others.

Concluding remarks

In the light of recent advances in NIE, the main message of the discipline, i.e. that 'institutions matter', could be reformulated as 'institutions *and society* matter'. The society affects the links between institutions and development in at least three important ways. First, it supplies informal institutions which comprise a vital part of modern institutional setups. Informal institutions, such as norms, trust, and networks, convey essential market information, reduce transaction costs and otherwise support investments and exchange. Second, the society at large and its different groups produce demand for formal institutions, and as such are pivotal for institutional change. Third, the *performance* of formal institutions depends on actions and attitudes of various social, economic, and political actors. For all of the above reasons, NIE is a natural field for interdisciplinary collaboration between economists and sociologists, as this brief survey hopefully demonstrates.

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New Institutional Economics versus *Economics of Conventions*: The Difference between Bounded Rationality and… Bounded Rationality

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In this small note, I will discuss three points comparing research programs of New Institutional Economics (NIE) and Economics of Conventions (EC).

■ The research program of New Institutional Economics is quite close to that of the younger Economics of Conventions – indeed both are grounded on a common rejection of the mainstream model of substantive rationality, under the heading of bounded rationality. Even more, they share *the same fundamental postulate* about the world faced by economic agents: unforeseen contingencies are the lot of human condition, and so incomplete contracts are the rule, complete contracts are the exception, rather than the other way round in mainstream economics. Nevertheless, NIE and EC lead to *quite different economic styles of economic research*, within the same institutionalist tradition, and may promote even contradictory economic policies. Why?

■ All the differences may be shown to proceed from a different use of the Simon's hypothesis of bounded rationality: whereas Willamson's project aims at discovering how economic agents manage to deal with the most negative consequences of the fundamental problem (deemed as unsolvable), the conventionalist project wishes to offer a solution to the fundamental problem, by tackling it at its core (instead of counteracting its negative consequences). By examining the whole set of differences and investigating more closely their source, we are led to distinguish two models of bounded rationality: the first (NIE) is *only* calculative, the second (EC) is *also* interpretive. Once again, the EC project appears as *more inclusive* than the NIE one: a provocative but uninteresting observation, except if we can go further by using the comparison between NIE and EC to understand what lies behind or beneath the distinction of two models of bounded rationality.

■ We can throw some light on this question by putting together two strands of analysis, one inside EC, the other inside NIE. EC has stressed the idea that individual rationality in a game against nature cannot be the same as individual rationality in interactions with other individuals, for moral, methodological and technical reasons – and indeed it is a major criticism against mainstream use of optimization. This *conventionalist impossibility theorem* concerns bounded rationality, no less than unbounded rationality. And that may be the key to the distinction between the two models of bounded rationality. What gives weight to this conjecture can be found in Williamson's program, at his beginning: in 1975, he introduced the concept of "atmosphere" to grasp the fact that sometimes *the exchange relation itself becomes an object of value*. Unfortunately, "atmosphere" has been forgotten in all posterior works (with the important exception of a suggestive comment in 1996). I will argue that the solution to the incompleteness problem, in EC, may be considered as an extrapolation of Williamson's views, i.e. as a projection of "atmosphere" onto a space of intersubjective representations. Would Williamson have stuck to his initial consideration of "atmosphere", he would have gone much farther from his calculative conception of bounded rationality, and adopted an interpretive conception, which is at the heart of EC. And he would have developed a quite different style of research about law and economics.

A silly conclusion would be that NIE is wrong. The right one is rather to ask NIE researchers …. to ask themselves that question: in the problem you are studying, are you sure that "atmosphere" is negligible ?

So Close, So Far…

To begin with, let us quote Williamson about the general features of NIE:

"…The New Institutional Economics (NIE)

- holds that institutions matter and are susceptible to analysis;

- is different from but not hostile to orthodoxy;

- is an interdisciplinary combination of law, economics and organization, in which economics is the first among equals" (Williamson 1996: 3).

The same could be said roughly about the "economics of conventions" (EC), a research program in social science, appeared with a special issue of *Revue économique* in 1989, with some second-order differences

- EC defines itself as a branch of institutionalist tradition, even if it started by an in depth investigation of the most informal institutions : conventions, and indeed it did not initially put to the fore the notion of institution – but that was the case, too, of the first Williamson : there is no entry for "institution" in the index of *Markets and Hierarchies*(1975), whereas, ten years after, "institutions" became a prominent word with *the economic institutions of capitalism.*

- EC is much more rooted than NIE in an internal criticism of the new mainstream economics (incentives, contracts and game theory) and spends much space and time, dwelling upon its logical limits. Nevertheless, EC is often formulated by reference to the two pillars of economic orthodoxy (rationality and coordination), with the prospect of offering a more general view of both. In that sense, it is not radically hostile to orthodoxy: it aims at transforming its language, not at replacing it.

- EC proceeds from a cooperation between economists and sociologists, but in a spirit at variance with the posterior "economic sociology", since the objective of the cooperation was simultaneously (although it was not so clear initially) to transform economic thinking, and to promote a pragmatic sociology. The connection with law is now quite strong, but at the very beginning, at least philosophy of law was present, through the problem of interpreting the blanks of formal rules (especially contracts).

So at this level of general features this is a first kind of similarity between NIE and EC (we provisionally put aside the obvious facts that EC is much younger than NIE , and considerably less familiar in the English speaking academic world). That is not so surprising, since both claim to belong to the institutionalist tradition. Now we have to go deeper. There is a second kind of similarity, at the level of assumptions. NIE and EC share no less than 5 key assumptions:

Bounded rationality

The criticism of mainstream substantive rationality by Herbert Simon is an essential source of inspiration for both currents. There is explicit reference to the concept of bounded rationality, most notably with the correlative assumption that usually economic agents could not write complete contracts, not because it would be too expensive but because in most ordinary cases (especially in labour contracts) there will be unexpected contingencies. People could not have a complete view of the future – for bad (failure, etc.) and good (innovations, etc.) reasons.

Law and economics

Economics should be interested in all the questions covered by law, at least for the simple reason that it's as absurd to think about the economic world without law as it is without money. Initially EC was more inclined to work about money and labour, and NIE about business (and labour, indeed) but that changed afterwards. A paradigmatic example of their common attention to law is given by the fact that both will quote court decisions as analytical arguments (something mainstream law & economics will never do).

Market vs firm

The opposition between market and firm is a founding stone for both programs (and, to say the truth, for many other programs): it introduces the fundamental idea that the market is not the only means of coordination, even in the framework of methodological individualism. Economic agents live in a world with many more means of coordination than the old-fashioned neo-classical economic theory used to think.

Variety of contracts

A consequence of the previous point is that the space of contractual arrangements is enormously enlarged: first there are as many families of contracts as there are types of means of coordination; second, within each type, we will encounter a plurality of contractual tools. And the task of economics is to give account of the economic meaning of this plurality, and of the logic of choice between all these options. It's clear that a new era of cooperation between economics and law is in front of us. It's an argument shared by NIE (indeed introduced by it) and EC.

Institutions are submitted to a test of efficiency

Institutions are no longer exogenous, and they are plural. Therefore it exists inevitably some kind of comparison/competition between them. Economic agents, when they have recourse to institutions, are always simultaneously exercising their critical mind upon them. And the first question is that of efficiency: does it work correctly? It's an extraordinary extension of the field of economic theory, because we have the same extraordinary extension of the space of economic choices by economic agents.

But those *similar* key assumptions are used to derive rather different conclusions, because they motivate a quite *different* style of research (Favereau and Lazega 2002).

1. Rationality consists in computation (of costs and benefits) for NIE, whereas for EC (cf also Favereau 2005) it consists *also* and most importantly in interpretation (of relations and quality).

2. Law and economics is actually an economic analysis of law for NIE, whereas EC develops a legal analysis of economics.

3. Market failure opens the way to organizations (and organizational failure restricts it) for NIE, which supposes that market comes first, whereas for EC it is rather the firms which builds markets, but, true, within an economy of private ownership (which supposes that the possibility of market comes first, and this possibility is implemented by firms). Moreover EC do not share the view of a "supermarket" of means of coordination (of "governance structures"), where the choice is driven by a simple criterion of transaction cost minimization.

4. The variety of contracts result from variations in the degree of asset specificity (with a complementary role for price and safeguards) for NIE whereas for EC it results also and most importantly from differences about quality (different degrees of quality within an order of quality, and different orders of quality), with a complementary role for quantity and duration.

5. There is a fundamental trade-off between efficiency and equity, for NIE, which suggests that institutions are faced to two independent criteria and implies that problems of efficiency and problems of equity, with respect to institutions, can be studied separately, whereas for EC, there is some overlap between questions of efficiency and equity, so that, within that area, there is a monotonic relationship between these two criteria. More generally that implies that efficiency and equity are somehow intertwined, in no simple way – so we should not study them independently.

One Fundamental Problem, Two Answers?

The fundamental problem is simple to formulate, if not to solve. We renounce the assumption of unbounded rationality (optimization), because it relies upon an axiom of a fixed, stable and known list of states of nature. And inversely we adhere to the assumption of bounded rationality because we know that there will probably be unexpected, and even unexpectable, states of nature. So the question becomes: how do economic agents supposed to be "intendedly rational but only limitedly so" deal with the problem of (the risk of) unforeseen contingencies?

Williamson's answer is well-known: economic agents do not know what the future will be, but they know that they do not know what the future will be: they are sure they will have to deal with (bad or good) surprises. So they should prepare for anything. They devise, in advance, institutional frameworks to be used in case of execution problems during the contractual life: a system of rules actionable when surprises arrive. Williamson qualifies them as "governance structures": "an institutional matrix in which the integrity of a transaction is decided. In the commercial sector, three discrete structural governance alternatives are commonly recognized: classical market, hybrid contracting, hierarchy" (Williamson 1996: 378). Then Williamson shows that the most important causal factor, driving the minimization of transaction costs, is the degree of asset specificity. A high degree makes the hierarchy more efficient than the market which is better for minimal degrees, the hybrid representing an intermediate case.

This model appears as useful to predict the choice of a type of governance structure; but at this level of generality, it does not say much about the choice of one precise form, inside a type. More embarrassing is the fact that this model is unhelpful to give advice to an economic agent Mr X as to the option of contracting with Mr Y or Mr Z, once the governance structure has been determined: can Mr X trust Mr Y or Mr Z? How should he write the contractual details of the agreed governance structure? The theory is silent upon these questions.

Quite different is the road followed by EC. Why do intelligent (and not altruistic) people accept to sign an incomplete contract? The hint to find a solution is that since incomplete contracts are signed every day, in the actual world, the solution may be so simple that it is easily overlooked by sophisticated theoreticians. By borrowing from basic elements of communication theory and of formal logic, it becomes possible to sketch a pragmatic solution. People, through their discussions between one another, build (or not) a *scheme of a satisfactory relationship*, with its main properties. If they succeed in building such a scheme, they will agree to enter some sort of contractual relationship (that is tautological), and, (that is no longer tautological), they will compare the successive events (of which many are unexpected) with that scheme, in order to decide whether they keep on the relationship ("loyalty"), or they break it ("exit") – or they express their discontent through "voice" (cf Hirschman). This scheme is an intersubjective entity, something like a social representation, with many of the features of a "convention": a rule, rather vague, and constraining, although not enforceable through a public authority. The normative component of the scheme includes efficiency considerations but obviously too equity considerations. Justice, as a guiding value, recovers its place (lost during the New Welfare Economics of the 1930s) into the realm of economic coordination.

So EC offers a tentative general solution to the problem of incompleteness. It may explain why Mr X signs a contract with Mr Z and not with Mr Y, but it does not say anything precise, at this stage, on the type of contractual framework. Thus EC makes a sharp contrast with NIE, the objective of which is rather to offset the negative consequences of the mistrust generated by the problem of incompleteness. Williamson's model does not depend on any hypothesis to solve the general problem. With an ecumenical point of view, we must admit that EC and NIE could be considered as complimentary, at least with respect to the sole problem of incompleteness.

But of course, the EC program, in a second step, can draw more precise deductions from its general solution: for instance, let us consider the option "market versus firm". It is studied by Williamson by making the degree of asset specificity vary. For EC, the essential point is that relationships are valued in a very different way on the market and in the firm. The firm needs lasting relationships, and the market is easy with spot relationships. Therefore the only possible collective order on a market comes from the emergence of quality conventions, stabilizing the expectations of both consumers and producers. A firm needs a much higher level of cooperation to be efficient – with two strange new properties: it's collective efficiency which is required at the end, and that excludes … the exclusion of equity, to the sole benefit of efficiency. Then the collective order will be of another type, more dynamic than a quality convention: something like a successful organizational learning.

Let us have a retrospective glance at the semantics of EC: "quality", "justice", "relationship", "cooperation", "collective" –all these notions are akin to an *interpretive* model of rationality, and foreign to a purely *computational* one (for details cf Favereau 2005). Ultimately, the divergence points at the conception of homo economicus: though he is subject to the same assumption of bounded rationality, in NIE he is condemned to seek his own interest "with guile"; in EC, he is free to choose his level of identity: personal, social, human (for details cf Turner 1987).

To conclude: the divergence between NIE and EC seems to become very large indeed, if we look at the following table, summarizing the key concepts of the two research programs ; and the heart of the divergence seems to be located in the model of bounded rationality used : (only) calculative, for NIE; interpretive (which subsumes calculative) for EC.

New Institutional Economics	**Economics of Conventions**
Transaction Costs	Conventions
Governance Structure	Collective learning
Asset specificity	Quality conventions
Efficiency	Justification (efficiency + equity)
Opportunism	Self categorization & social identity

Can we go further, by investigating more closely what lies behind, or beneath, the distinction between those two models of bounded rationality? The fact that the younger program looks not only close to, but also more comprehensive than, the older one breeds the hope of a positive answer. We are not at all in the trivial case of two rival theories, delivering conflicting messages. EC and NIE come from the same source, even if each benefit from distinct secondary affluents.

Two Answers, Or One?

First let us quote quasi exhaustively the two brief (but exceptionally insightful) developments Williamson devoted to the concept of "atmosphere", in 1975 and in 1996, starting from the more recent one, which sounds like a repentance.

"A colleague noted that the economics of atmosphere plays a larger role in *Markets and Hierarchies* (Williamson 1975) than in *The economic institutions of capitalism* (Williamson, 1985) and asked about the de-emphasis. I replied that I thought atmosphere at least as important to an understanding of economic organization in 1985 as I had in 1975. Not having made more headway, however, I had little to add.

One of the lessons of the economics of atmosphere is that calculativeness can be taken to dysfunctional extremes. That can show up within governance structures as well as between them. The employment relation is one such context.

(…) If functional separability does not imply attitudinal separability, then piecemeal calculativeness can easily be dysfunctional. (…) The neglect of such interaction effects is encouraged by piecemeal calculativeness, which is to say by an insensitivity to atmosphere" (Williamson 1996: 270, 271).

"The standard economic model (…) assumes that individuals regard transactions in a strictly neutral, instrumental manner. However, it may be more accurate, and sometimes even essential, to regard the exchange process itself as an object of value. Concern for atmosphere tends to raise such systems issue; supplying a *satisfying exchange relation* is made part of the economic problem, broadly construed [the italics are within the text].

Alternative modes of organization sometimes differ in nontrivial atmospheric respects. Distinctions between calculative and quasi-moral 'involvements' are relevant. Market exchange tends predominantly to encourage calculative relations (…).

Internal organization, by contrast, is often better able to make allowances for quasimoral involvements among the parties. The sociological phenomenon of reciprocity is an example (Gouldner 1968).

Recognition that (…) [exchange] relations themselves are valued, requires that organizational effectiveness be viewed more broadly than the usual efficiency calculus would dictate (…).efficiency and a sense of well-being (that includes, but transcends, equity) are intrinsically (non separably) joined (…).

1. A full discussion of atmosphere and its ramifications raises a wider set of sociopolitical issues than can be addressed here. Suffice it to observe that (…) atmosphere is reserved for those transactions for which attitudinal spillovers are thought to be especially strong" (Williamson 1975: 38-39).

Now my conjecture: these ideas could find their full extension in EC program, and then give us the key to the distinction between the two models of bounded rationality. Thus, the strange mix of points of high agreements/disagreements which divides EC and NIE within the institutionalist tradition could be understood as the produce of two distinct strategies of research, logically and epistemologically connected.

There is obviously a close similarity between "atmosphere" (which implies that the exchange relationship itself becomes an object of value) and the "scheme of a satisfactory relationship", promoted by EC to solve the fundamental problem. Most remarkably, when Williamson begins to draw the potential consequences of the introduction of "atmosphere", the strongest oppositions between EC and NIE begin to fade away:

2. Efficiency and equity become intertwined (cf conclusion /5/ above p. 24)

3. Rationality becomes less "calculative", and must take account of "quasi-moral involvements": this is a decisive step towards a more general view of rationality, adding interpretive capacities to the calculative ones (cf conclusion /1/ above p. 24).

4. The end of "attitudinal separability" (generating possible "attitudinal spillovers") means that this time, Williamson is groping after an explanation of the reasons why Mr X signs such and such contract with Mr Y, that is searching for a general solution of the fundamental problem, contrary to his preferred strategy of finding a procedural device to counteract the destructive consequences of incompleteness (cf pp. 24-5).

5. A graph which I did not reproduce (1975, p. 40) shows "atmosphere" as a meta-judgment synthetizing and embracing all the features of the contractual framework (opportunism, small numbers, uncertainty, complexity, etc.). It suggests a provocative reading: forget "atmosphere" – you get NIE as it is; stress "atmosphere" until it "raises a wider set of sociopolitical issues than can be addressed (…) [in NIE as it is]" – you get EC!

That reading would be somehow deceptive: for EC there is more at stake than a simple academic debate about the empirical (un)importance of "atmosphere". The real question to be addressed is: should we, as economists, keep on assuming the model of bounded rationality is formally the same, whether it is applied to choices against nature or to interactions with others – as it is with mainstream substantive rationality (cf Mariotti 1985; Favereau 2004)?

For EC, the answer is: no.

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Petering Out Or Flaming Up? New Institutional Economics in East-Central Europe

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Petering out? Fading away? Does New Institutional Economics (NIE)1 not offer a large set of scientific theories of great attraction? Were they not „doomed" to flood Eastern Europe that has been experimenting with deep-going institutional change during the past few decades? Did the research communities in the region prove to be unprepared to borrow new institutionalist ideas? Was Western success converted into Eastern failure? Did the proverbial curse of arriving too late hit the Eastern European economists again?2

Failure is probably too strong a word to use. I would replace it with habituation, even fatigue that may follow any successful breakthrough in scholarly exchange. Fatigue can result in stagnation but can also be provisional, giving rise to a new wave of reception soon, not to speak of original scientific discoveries.

A hasty prognosis

Why did I nonetheless expect a quickly widening institutionlist research program in the region back in 1993 when first reflecting upon the alternatives of evolution in economic sciences after communism? Then, I predicted a large-scale venture of importation accompanied by a rivalry of two Western paradigms (ORDO and NIE) for the hearts and the minds of Eastern European economists (Kovács 1993)3. Witnessing the popularity of the concept of *Soziale Marktwirtschaft* in political discourse after communism, and the proliferation of new institutionalist notions such as transaction costs, path dependence or social capital in the economic analysis of the transition to capitalism, I presumed to see an ongoing competition between old („German") and new („American") patterns4 of institutionalist thought in Eastern European economics. This open-ended scenario with two possible outcomes rested on the following four assumptions:

- Both major schools of economic science under communism, i.e., official political economy (textbook Marxism) and reform economics (market socialism)5 will disappear: the former virtually collapsed before 1989 while the latter will merge with old and new institutionalist theories prevailing in the West. The merger may be facilitated by the fact that reform economics, including self-management programs, developed, almost instinctively, quasi-institutionalist (more exactly, speculative institutionalist) techniques of criticizing the planned economy as well as of engineering its reforms.6

- The institutionalist explanations for severe market distortions in the planned economy such as shortages, sectoral imbalances, and investment cycles, which were put forward by the reform economists between the 1950s and 1980s, can easily be incorporated in the Western literature. Concepts like overcentralization, plan bargaining, regulation by campaign, paternalism and the shadow economy will find refuge in the (then) new theories of property rights, government failure, bargaining, political business cycles, etc. – of course, following major analytical enhancement.

- Many of the reformers will turn into capitalist "transformers" studying the post-communist economy and designing large-scale deregulation (marketization and privatization) schemes. Hence, they will badly need reliable know-how for understanding and initiating institutional change.

- A good part of that know-how is available in the West, the Big Unknown of scientific development is rather on the demand side. In leaving reformism behind, the Eastern European economists will face, by and large, two rival institutionalist traditions: an essentially verbal-historical one offered by "good old" ORDO liberalism, and a relatively new one based rather on neoclassical-style formal analysis (with increasingly sophisticated game-theoretical instruments) and offered by NIE. Which of the two will be their choice? To put it simply, the former relies on holistic concepts such as economic order, promises to solve real-life problems and stresses social responsibility and the need to correct the market from outside whereas the latter prefers methodological individualism, and trusts the justice-making and self-correcting power of the market. The former is closer to the local intellectual and political traditions of the ex-reformers (and mathematically less demanding), offers an activist role to the scholars, and is justified, especially in the eyes of the older generations, by the European success story of the welfare state between the 1950s and the 1980s. The latter is widely seen as superior in terms of scientific precision and academic strength due to its intimate links to neoclassical economics, probably more attractive for the younger generations of economists in the region, and gains legitimation from the comparative advantages of the "American model" vis à vis most of the European ones during the 1980s and the 1990s. In sum, the former is assisted by a boring but reliable past that has been supported by theories of moderate sophistication whereas the latter represents the music of the future full of risks and perhaps of Grand Discoveries.

Shortly after 1989, I avoided guessing who the winner could be but expressed some fear from a combination of the old-new propensity of the transformers for state interventionism with resurgent nationalism in the region under the auspices of a statist-conservative-corporatist interpretation of the ORDO program. Thus, part of the latter's liberal constituents would be suppressed, and the ex-reformers (or even the textbook Marxists) would find refuge in the theoretical construct of a new type of social market economy flirting with a Third Way that is much more collectivist than the one advocated by Röpke – a nightmarish *National-Soziale Marktwirtschaft* somewhere between Mussolini and Meciar.**7**

Indeed, it was terribly difficult to forecast the winner (or to define the terms of an incidental – local – cohabitation between NIE and ORDO**8**) but it seemed evident that if there were a winner it would emerge from the rivalry of these two.

These expectations were contingent on a deep-going methodological and discursive change in the economic profession throughout the region. Any East-West convergence in institutionalism (even on the basis of ORDO liberalism) depended on a considerable rapprochement between the Western techniques/languages of economics and the local ones. To put it less politely, Eastern European economists could not hope, I believed, for success on the international scene if they continued to insist on their homegrown "quasi-institutionalism", or, more exactly, "speculative institutionalism" (think of the amorphous "plan-and-market" and self-management discourses used by the reformers even in the late 1980s). Normally, this kind of institutionalist research program was less empirical and, at the same time, much less abstract-axiomatic than NIE. While it, like ORDO, feared formalism, its empirical strength was often dwarfed by that, too. Nevertheless, I presumed that learning might become a two-way street: economic sciences in the West would also borrow scientific ideas from our region. In revisiting its own economic institutions under communism, Eastern Europe seemed capable of enriching not only "old" institutional thought but also some of NIE's core concepts such as fuzzy property rights, informal institutions, incomplete contracts, rent seeking, etc. It seemed capable of delivering, via the economics of communism, an institutional theory of an unfeasible economy, which could play in economics a similar role to the one assumed by the *perpetuum mobile* in physics. As for post-communism, the region, I presumed, would be able to serve, in the course of the economic transformation, as a potential hotbed of institutionalist discoveries.

These expectations reflected a rather cooperative and frictionless scholarly exchange with the West. What we, Easterners want to come in will arrive, and what actually comes in is the same as what we originally intended to receive. Also, to use the language of political correctness, the institutionalist economists in the region were portrayed not as handicapped or disabled but as *differently abled* scholars who may have authentic products to sell.

Besides methodological adjustment and a "discursive turn", I argued, the sociological context of economic sciences might also change in Eastern Europe to promote convergence in institutionalist research programs. Presumedly, party congresses, censored journals and politically embedded scholars will not determine scientific progress any longer. At the same time, "secular" (politics-free) research communities, peer-reviewed publications and the faculty library, or the faculty club for that matter, will become the main vehicle of scholarly evolution. Eastern European experts will be subjected to the same kind of rivalry in the academic market (locally and globally) as their Western colleagues, the patterns of recruitment, promotion and mobility will also be similar, a good part of scholarly output will come from private institutions of research and education, etc.

All in all, the cast was presumed to include two collective actors on the Western side, the representatives of ORDO and NIE, while on the Eastern one I saw the vanishing textbook Marxists, the ex-reformers as well as the "innocent youth" appearing on the scene of economic research after 1989. It was also reasonable to assume that generational differences would matter. The younger you are, the greater your chances for receiving proper education in neoclassical economics – a *sine qua non* of absorbing new institutionalist ideas. Here, I thought, two kinds of frustration might coincide. Both the inexactitude of the verbal research techniques applied by the older colleagues, and the sterility of certain just-acquired neoclassical models can prompt young scholars to switch to NIE (without having to lower the level of formal analysis). While being pushed by these, they are pulled simultaneously by new institutional economics in the West, a fresh, flexible and fashionable discipline that promises the best of old institutionalism and the current mainstream without making the researcher suffer from their imperfections. The Eastern European economist was offered a unique chance of becoming an orthodox and heterodox expert at the same time who borrows and invents simultaneously, avoiding in this way the path of servile imitation.

If that prognosis is not flawed – so went my argument a few years later (Kovács 2002) –, the neoclassical paradigm needs to be included in the group of Western actors. Probably, the spread of this paradigm will also accelerate the diffusion of new institutional economics as an unintended by-product (or collateral damage). However, I disregarded three other options:

- ORDO would smoothly withdraw from the competition but NIE would not become a real winner.

- Neoclassical theory would not produce its own "Eastern dissidents" for quite some time, moreover, many of its local representatives would keep a low profile on NIE.

- Under post-communism, the economic profession would face an "anything goes" (more exactly, an "any theory can melt into another") situation, in which even hybridization might turn out to be a too courageous working hypothesis.

What was disregarded in the early 1990s, became reality, and a veritable astonishment to me 15 years later when diving into a comparative research project on the reception of New Institutional Economics in eight countries of East-Central Europe: Bulgaria, Croatia, the Czech Republic, Hungary, Poland, Romania, Serbia and Slovenia.9

Rite de passage

„Have Polish economists noticed new institutionalism?"– asks Jacek Kochanowicz with a skeptical undertone in his case study. Roumen Avramov argues that „NIE's presence in the Bulgarian landscape of economic science is still incoherent and lacks a critical mass. It can hardly be considered a compact current, able to counter the dominant influence of neoclassical economics." Vojmir Franicevic speaks of „soft" institutionalism (i.e., using NIE concepts when they „fit the 'story' well") and a passive reception of new institutional economics in Croatia. In Bulgaria there is only one consistent curriculum of new institutional economics. Horia Paul Terpe and Paul Dragos Aligica warn the reader that „signals that may indicate a 'new institutionalist' explosion should not be confused with the adoption of the real thing." Institutionalism has not been institutionalized yet – quite a few authors play with the words.

In fact, expecting a series of original discoveries at the local level to be published by first-rate journals in the US would have been a vast exaggeration. However, infiltration at a snail's pace, aborted takeover, eclectic borrowing, simulated appropriation, etc, i.e., patterns of scholarly importation revealed by our studies, would have been regarded as predictions of excessive pessimism one or two decades ago. Our research team presumed that NIE must have enchanted the economists throughout the region because it offered a paradigm they badly needed, could respect, understand and believe in, not to speak of the fact that the scholarly supply was well-marketed. A special advantage of the subdiscipline is, says Avramov, that it may serve as a „proxy theory" that can substitute other theories, fill „presumed gaps left by 'conventional' economic thinking", thus, it can please even specialists of diametrically opposing persuasions. Although from time to time, NIE was packaged in radical/dogmatic libertarian rhetoric, it promised the local experts a large degree of elasticity: a balanced view of government *and* market failures, a historical approach to the evolution of institutions, multi-disciplinary analysis, etc, that is, scholarly cultures these experts were socialized in. They could expect that at last they would put in precise (yet, spectacular) scholarly terms what they had only speculated about earlier, and trust in the long-desired possibility of measuring the variables as well as testing the conclusions.

The encounters by local economists with new institutional theories (and theorists) in the West had first begun in Hungary, Poland and Yugoslavia back in the 1980s or a little earlier. Initially, the demand was instinctive, sporadic, accidental and issue-dependent. Typically, the East-Central European economist was searching for a solution of a given problem (e.g., simulating private property in Hungary, comparing economic systems in Poland, and reshaping the federation in Yugoslavia); browsed through a few chapters of Western literature; and was enchanted by the discourse of the then emerging school. At that time Armen Alchian, Harold Demsetz, Mancur Olson were among the most cited thinkers. They were accompanied by scholars like Herbert Simon, Harvey Leibenstein, even Albert Hirschman who are seldom regarded as „founding fathers" of NIE today. Their arguments could be followed easily by means of mathematical skills attained in the study of the economics of planning and self-management before.

Indeed, NIE's sporadic infiltration[10] grew into a regular marketing campaign and a simultaneous buying boom in the late 1980s and the early 1990s. The campaign was operated mainly by North-American universities, think tanks and foundations (George Mason, Texas, Atlas, Fraser, Liberty Fund, Bradley, etc.) in all countries of the region, mediated by joint research projects, seminars, conferences, university courses, summer schools, translation programs and the like.[11] Its impact was reinforced by the first Nobel Prizes given to some of the representatives of new institutional thought (Buchanan, Coase, North, Fogel).[12] The overall climate of reception became especially favorable when the World Bank, the EBRD and some other international organizations replaced their Washington Consensus-style policies with the one using the „institutions/cultures matter" rhetoric.

The institutions-centered message from the West got considerably strengthened by the EU accession of a series of ex-communist states, that is, by the very program of a comprehensive transfer of institutions as well as by the *acquis communautaire* expressing a quintessence of European capitalism. Consequently, in the East-Central Europe of the mid-1990s, you could join the NIE universum with a middle-of-the-road social-democratic commitment, and you did not have to quit it even if you cherished arch-libertarian views. NIE is a tolerant discipline, note some of my colleagues.

A small scientific revolution was in the making – a change that was not forced upon the „natives" of the region. If new institutionalist ideas have begun to colonize them, then that was rather a sort of self-colonization. The local economists were prepared to leave the first stage of reception, that is, writing review articles and organizing introductory seminars, for launching their first real research projects to adapt and test foreign models of privatization, anti-trust regulation, corruption and the like. Our case studies contend that NIE got stuck in this introductory phase in many respects, in other words, the *rite de passage* was interrupted or slowed down considerably. To put it bluntly, new institutional ideas have not become part of the „spiritual capital" (Kovács 2010) of the East-Central European economists' epistemic community.

Today, with the exception of a few tiny islands of NIE (such as the Department of Law and Economics at the Law Faculty of the Belgrade University or the Institute for Market Economics in Sofia), one sees lonely scholars scattered all over the region without any regularity. More exactly, there is a rule: no country shows extraordinary achievements in developing new institutional economics, no matter if the local economists encountered the West earlier or later. Apparently, stagnation has an egalitarian nature. As years go by, the inhabitants of the islands are happy if they survive somehow. They cannot hope for strongly affecting their own research environment soon. The typical NIE specialist in East-Central Europe continues to popularize his/her favorite authors and models, writes in domestic journals and, in the best case, applies already existing (Western) knowledge. (As a Romanian respondent complains, „we are the measurement guys at the end of the chain"). None of the case studies reports on an article published by a local expert of new institutionalism in a foreign journal of high reputation.[13] University courses of new institutionalism do not offer a comprehensive picture of the school, instead they focus on a narrow selection of „famous" authors. In most countries, just a few classic volumes written by leading theorists of new institutionalism were translated. Renowned institutions such as the CERGE in Prague or the Institute of Economics in Budapest can still easily afford to operate without any permanent contribution by NIE scholars.

Rivalry

NIE versus ORDO? Contrary to my initial expectations, in East-Central Europe (just like in the West) new institutionalism does not compete with the old one but rather with the neoclassical paradigm. In other words, NIE has no noteworthy rival inside institutionalism as well as no strong ally outside. As mentioned above, American-type old institutional thought (ranging from Thorstein Veblen, through

John Commons and Wesley Mitchell all the way down to John Galbraith or even to Geoffrey Hodgson) has never been popular in the region. As a contrast, the German/Austrian tradition did influence the economists in East-Central Europe (less in Serbia and Bulgaria) before and even under communism, no matter how ambivalent that tradition may be.14 Today, however, ORDO liberalism appears, condensed in three sentences on *Soziale Marktwirtschaft*, above all in party programs. One finds in most countries Hayek societies, clubs, institutes but they are noisy rather than strong in scholarly production.15 The marginal role played by the libertarian wing of old institutionalism is evidenced by the example of some neo-Austrian economists in Romania who, believe it or not, flirt with the Orthodox religion. (Supposedly, this is not exactly what one may call „Hayekian orthodoxy" in the history of economic thought.)

Why do I speak of a rivalry between NIE and mainstream neoclassical theory? Why is the latter reluctant to identify itself with new institutional thought in East-Central Europe? (More exactly, why is it perhaps more reluctant to do so here than in the West?) Mainstream scholars in both the West and the East contend that they have already identified themselves with NIE by incorporating many of its discoveries into the main body of neoclassical thought or its applied subdisciplines. In East-Central Europe they also claim that there are no significant scientific results produced by new institutional economists on both communism and post-communism to incorporate. Finally, they tend to discover some dark spots in the local genealogy of NIE after 1989. Providing easy refuge for former textbook Marxists and reform economists to survive, and offering a good pretext to avoid renewing their research techniques constitute two main reasons for suspicion. (As one of my interviewees, a neoclassical expert exclaimed, „when will 'these' learn at last to set up an equation!?")

Proud eclecticism

Our case studies suggest that today, virtually any research program can couple with any other in East-Central European economic sciences. That was the third – probably most shocking – surprise to me in this project. Of course, as an alumnus, I feel extremely frustrated by the story of the former Karl Marx University of Economics (today, Corvinus University), which demonstrated, in the first years of the 21st century, a strange coalition of thoughts (and interests) between a very old professor of the history of economic thought, an old expert of verbal-style international economics, a former party apparatchik in the Central Committee (currently he is professor of public choice) and a young specialist of micro-economics who has strong Marxist/anti-globalist views; a coalition cemented in an opposition to teaching modern neoclassical theories (Varadi 2007). Unfortunately, this is by no means an exotic example, just like the above-mentioned oxymoron of „Hayekian orthodoxy" in Romania is not either. Another Romanian invention, namely, combining the German historical school, structuralism, nationalism, old-style development theory and new institutionalism, also gives birth to an interesting scientific creature. Concepts come and go, and the rate of fluctuation of the attitudes of their representatives is rather high. One of the Croatian respondents calls himself a „survivalist", another one an „eclectic by default". A Bulgarian interview partner says: „There is no inconvenience in declaring oneself a follower of one, and later of another theory. The wise man keeps under control the instruments and the concepts he utilizes."

What is the reason for these „postmodern" conditions? Did Western supply diminish? Did the wheels of the mediation mechanism start squeaking? Or did local demand ebb? I think all these factors were instrumental in the slowdown of reception. Obviously, the potential supply of NIE theories did not decline (just the opposite was the case) but the attraction stemming from the novelty of exchange of ideas definitely decreased. On the supply side, the scholarly interest shrank owing to a Western-style consolidation of economic research and education in Eastern Europe, which nonetheless did not result in breathtaking scientific discoveries. The „missionary" stage of exporting new institutional ideas to the „savages" was continued by a tedious process of piecemeal construction and legitimation of the subdiscipline at the turn of the century. The Western think tanks, foundations, specialists, etc., began to withdraw from the region, leaving the „converts" behind.

On the demand side, new institutionalism did not lure neoclassical scholars out of their world of more abstract model-building. They were on an exciting learning curve, exploring the secrets of the „Grand Theory" with its booming applications that, as mentioned before, have already included a number of NIE-type solutions. „If I use nice rhetorical twists like „path dependence", do I learn anything tangible about the economy; will I be able to make better predictions?", asked one of my interview partners, a macro-economist by profession. Furthermore, what was an advantage in the eyes of the institutionalist experts, namely, the closeness of NIE to the politics of transformation, appeared to the „mainstreamers" as a disadvantage. Witnessing how often the post-communist governments were improvising large-scale institutional change using primarily *old* institutionalist rhetoric did not help convince the neoclassical specialists to join forces. In the Czech Republic, for instance, the voucher privatization scheme based on allegedly Austrian evolutionary principles triggered sarcastic remarks from our respondents in the mainstream camp. Also, today they think twice before joining the „ghetto" of new institutionalism after having been released from their own one called euphemistically „mathematical economics" prior to 1989. Apparently, low-quality institutionalism is a weak challenge for the local mainstream specialists to change their mind. „Why should I love the Eastern European clone", asked the same Hungarian respondent, „if my American colleagues are not delighted with the Western original" of a certainly higher quality?

A happy exception?

Regarding Hungary, I can't help saying, in a telegraphic style, some words about the reception of New Institutional Economics in my country in order to dispel a widespread misunderstanding. In extrapolating the success of Hungarian economists in self-Westernization during the 1960s-1980s, benevolent observers tend to think of a *Sonderweg*, assuming tacitly that NIE must have had a green light to enter research and teaching in Hungary (Kovács 2002). Yet, as regards the expected Western breakthrough in institutional economics after 1989, Hungary has also belonged to the "laggards" thus far. NIE did not invade the local research community, at the same time, ORDO did not disappear entirely. The latter is cultivated at small, conservative – Christian-oriented, including German-language – universities, typically, with no emphasis on Schumpeter and Hayek. The political parties in Hungary, no matter if they preach conservative, liberal or socialist values, have retained a diluted version of the Freiburg ideas in their programmatic documents since 1989. Sometimes they paint them green a little by using the term "eco-social" or, on the right wing, squeeze them in the concept of "national market economy" (or even "national and social market economy" or "eco-social national economy"). To be sure, the "Austrian extension" of ORDO did not entice even the liberals in my country. A faithful free-market rhetoric resembling that of Václav Klaus or Leszek Balcerowicz has never been popular among Hungarian economists. Prior to 1989, the reformers spoke their deliberately non-ideological language with a slight social-liberal accent, and marginalized the only anarcho-liberal thinker among them, Tibor Liska. Also, a less pragmatic source, the Karl Polanyi legacy of doubting the virtues of the "self-regulating market" was robust in economic sociology, and has proven such until now. Although the American sociologist and Hungarian expert, David Stark imported a few helpful evolutionary ideas, these lacked formalization as required by NIE.

Traditional Austrian economics had first been applied in the critique of Marx in the "Lukacs Kindergarten" at the turn of the 1960s and 1970s. Following a more than a decade-long break, it enchanted only a few young experts at the end of the 1980s. Neo-Austrians have been basically unknown in Hungary until today. Hayek was easily defeated, before he could have won, by both the need for social engineering in the first phases of the post-communist transformation and the rapid inflow of neoclassical economics, in the mirror of which his ideas seemed ideological, imprecise and dysfunctional to many. Apart from a former finance minister, Lajos Bokros, there are only a handful of scholars who subscribe to quasi-libertarian views in economic science from time to time.

In the prevailing spirit of pragmatism inherited from the local version of market socialism, it was not only the neoliberal doctrines that proved unable to fascinate the institutional economists in Hungary but also any strong attempt at formalization. Speculative institutionalism has remained the main genre of economic research although speculation became less and less tantamount to analytic imprecision, shaky realism and normative thinking. Verbal methods, that is, a descriptive rather than analytic approach, conceptualization rather than measurement, case study writing rather than model-building, historical arguments, thinking in terms of Big Sytems and Grand Designs, etc. still dominate the *oeuvre* of the institutionalist research community. The intellectual path of its members leads, simply put, from (Eastern) speculative to (Western) old institutionalism, and leaves the opportunity of switching from old to new institutional economics open.

The protracted and messy reception of NIE cannot be understood properly if one disregards another Hungarian specific, the immense authority of the role model of the economists' older generations, Janos Kornai. For a long time, he distanced himself from reform-making, superseded most of his colleagues in systematic description and formal analysis, and was keen on evolutionary change, yet failed to open up to accept Western institutionalist paradigms. This only happened rather late, in the early 1990s, and even then Kornai moved ahead in his own, proverbially cautious manner. He borrowed from both ORDO (e.g., comparative economic systems) and NIE (e.g., social trust), less instinctively than before, nonetheless, avoided subscribing to any of them wholeheartedly. What is more, at a certain point, he started mocking at the Eastern European "vulgar Coaseists", saying: *„I did not use the term 'institution' in every second paragraph as it recently has become fashionable to do, but I think I understood what a system means, and what the difference is between socialism and capitalism …"* (Kornai 2000).

Despite the above obstacles, the debut of new institutional economics in Hungary of the 1980s was fairly promising. Individual essays or volumes by Anthony Downs, Ronald Coase, Albert Hirschman, Mancur Olson, Herbert Simon, George Stigler were already translated into Hungarian. A majority of the leading journals of modern institutional thought were available in the Budapest libraries. Thus, a good part of the early property rights and transaction cost theories (Alchian, Coase, Demsetz, Pejovich, Williamson, etc.) were known among some liberal-minded scholars (including radical reformers). As early as 1990, Peter Galasi and Gabor Kertesi published a pioneering work on corruption in the public sector, which was based on the Jensen-Meckling model. In the Rajk College of Advanced Studies of the Karl Marx University students and young professors jumped into studying a large variety of NIE-related issues, including then unorthodox ones (e.g., social networks, capital and trust). Many of them were enrolled later at Western universities.

During the early 1990s, a translator and editor of a number of Western institutionalists, Laszlo Csontos who had given a series of formal and informal seminars in Budapest over the 1980s, returned from Connecticut where he worked together with Richard Langlois, and started teaching at the Central European University. With the help of his rational-choice-based (methodology-prone) institutionalism, he not only multiplied the number of adherents to new-institutional fields of economics through teaching and research projects but also represented a multidisciplinary approach to NIE, thereby mobilizing sociologists and political scientists, too. New translations were published (Buchanan, Pejovich, Elster, etc) but the "triumphal march" ended shortly thereafter.The subdiscipline was frequented by fellow-travellers and opportunists who blurred the boundaries between NIE on the one hand, and textbook Marxism, transformation studies as well as old-style Comparative Economic Systems on the other. They also preserved crucial teaching positions at the largest universities, determined the editorial policy of the main economics journal, *Közgazdasági Szemle*, and occupied the Hungarian section of the relevant international associations of new institutionalists.

Many of the young and middle-aged institutional-oriented talents turned (back) to "clean" neoclassical research, and/or preferred applied, empirical varieties of NIE (above all in labor economics, industrial organization and public policy) to the abstract ones. Alternatively, they left the country, weakening thereby the process of the subdiscipline's self-organization and legitimation in Hungary. The translated volumes, however, did not cease to appear (North, Acemoglu, Rabin, etc.), not to speak of a new genre, the institutionalist textbook (Cooter-Ulen, Cullis-Jones, D.B. Johnson, Milgrom-Roberts, Stiglitz) or the neoclassical textbook with significant NIE chapters (Hirschleifer, Williamson). That genre reflects the spread of path-breaking university courses all over the country in new political economy, law and economics, behavioral economics new economic history, economics of development, etc. This vibrant innovation in higher education (or on its margins) over the last decade, perhaps another Hungarian specific, has not reached the economic journals yet.

Flaming up?

By and large, the above said contain a pessimistic story. However, the word „yet" in the last sentence of the previous section refers to an optimistic one. Indeed, most of the case study authors contend that in East-Central Europe new institutional economics is in a state of silent accumulation. Its waning flame is gathering strength to burn soon. They refer to NIE specialists (predominantly young ones returning from the West) who have made huge efforts to have the standard works of the school published and to launch university courses that may result in a number of new experts and promising publications with some delay. They also call the readers' attention to other social sciences such as sociology (Bulgaria, Hungary, Poland), law (Hungary, Serbia), political science (Croatia), psychology and history, which often apply new institutional concepts in their borderlands with economics. Frequently, new institutional arguments are used in scientific debates without referring to their original sources. In other words, the case studies hint on an invisible proliferation of NIE in the academia and beyond. Public policy and corporate governance are highlighted in particular as fertile grounds for the diffusion of the philosophy of the school and for the mushrooming of NIE models ranging, for instance, from deregulation of public health care to devising the incentives of an intrapreneurship scheme. What did not work well in the academia, might do so in everyday economic life. Of course, the results vary: while new institutionalist concepts were successful in the pension reform, says Kochanowicz, they did not fare well in reshaping health care in Poland thus far. The Croatian case study reports on the success story of public finance.

A new push from the West may facilitate reception as well. Those who put their faith in a fresh start could not have imagined a better chance than a Nobel Prize given to Leonid Hurwicz, Elinor Ostrom and Oliver Williamson for research on mechanism design and economic governance, potentially the most vital themes in Eastern European economics today. All in all, the scholarly interest has not vanished, the attraction of the fresh NIE theories seems still fair, and the nascent capitalist regimes in the region lend themselves to institutional analysis no less than post-communist transformation did. Hence, one cannot exclude a new upsurge in the reception and creative application of new institutionalist ideas in the near future. What is lurking in the background in the form of reading the literature and making applied research projects today, may come to the fore and undergo a synthesis tomorrow.

Cultural encounters

The East-West encounters[16] in new institutional economics display a great number of peculiar traits, that is, irregularities as compared to a simplistic scheme describing an exchange between two actors of different cultural assets and power positions as well as of a linear sequence leading to a final cultural compromise dominated by the stronger partner.

■ By and large, the place of encounters has become indifferent by now: the individual countries and subregions do not diverge in terms of the exchange of ideas concerned. If they nevertheless do, South-Eastern Europe does not lag behind East-Central Europe (e.g., Serbia and Croatia demonstrate a faster and deeper reception of NIE than the Czech Republic and Poland).

■ Time seems to affect the encounters primarily through the age of local actors, especially due to the fact that the adjustment process took new dimensions in 1989, and the new generations of economists have been socialized in Western-like (or Western) education and research institutions.

■ As for the time structure of encounters, the period of high-intensity (although rather superficial) exchange is followed by stagnation with a hope for a new upswing.

■ In many cases, cultural adjustment is a one-way street leading from the West to the East (it rests on imitation and recombination rather than local invention) but it has its own limits. In addition, at its Eastern end, the actors hardly learn from each other across the country lines.

■ The encounters are basically geared from the West by „remote control" (i.e., the Western partner is present in his/her thought rather than physical self). Actually, this is a rather weak kind of instruction challenged by powerful local pressure in- an outside the academia. In addition, one sort of Western influence can be impeded by another one: the neoclassical mainstream both helps and hinders the reception of new institutional economics.

■ Both open resistance and dedicated emulation are rare, at the same time, eclectic and simulated adaptation is fairly frequent.

■ For the time being, the emerging compromise in institutionalist thought seems to be closer to the Eastern European point of departure, resulting in a kind of „updated"/"remixed" old institutionalism. Yet, supported by the takeover of the neoclassical paradigm (a takeover characterized by overt emulation), the local NIE hybrids may „go West" in the future.

■ The chance for a smooth evolution toward new institutionalism has not been exploited. The old epistemic community began to disintegrate but remained strong enough to prevent the consolidation of a new one. Its strategy was involuntarily assisted by the fading interest of NIE's Western core in Eastern Europe, and as a consequence, in the development of ideas in the region, and by the suspicion felt by the potential local ally, the new mainstream devotees toward institutionalism *per se*.

Detecting hybrids

Our case studies are far from offering a clear-cut typology of the emerging cultural compromises. It seems, however, certain that one cannot expect to arrive at an extremely asymmetric dual scheme, on the one side of which, we would see a few dedicated NIE specialists, while, on the other, a vast number of economists of neutral or even hostile persuasions. Instead, we saw a number of mixed types ranging from *refuge seekers* who "escape into" NIE from textbook Marxism or moderate reformism, through *intransigent verbalists* who arrived from the camp of radical reformers, *pragmatic institutionalists,* i.e., neoclassical experts ready to experiment with NIE concepts, and *neophytes* who maintain the local identity of NIE, all the way up to *potential synthesizers* who did not lose their ability to make verbal analysis but are also able to launch neoclassical-style institutional research projects.

The usual caveat applies: if filled up with names, such a typology would become even more complex. This would be all the more so if we applied any of the "muddling-through", "improvization" or "bricolage" hypotheses, widespread in cultural theory, to the reception of new institutional economics in the region. Also, as alluded on the introductory pages of this paper, one can be persuaded to abandon any attempt at classification, reflecting a pessimistic view of the current state of economic sciences in Eastern Europe. Accordingly, the economic profession is confronted with a situation, in which practically any theory can melt into another without special difficulty. As one of our Bulgarian interviewees remarked, *„even the Ponzi schemes could be considered as 'schools'."*

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Endnotes

1By New Institutional Economics I mean a great variety of expanding research programs ranging from property rights and transaction costs theory, through public choice, all the way down to evolutionary economics. Owing to the fact that NIE is famous/notorious for a profound interpenetration of economics with other social sciences, interdisciplinary fields such as „new economic history", law and economics, behavioral economics, etc were also regarded as organic parts of the school (see Hutchison 1984; Langlois 1986, 1989; Rutheford 1994; Coase 1998; Furubotn and Richter 2000; Williamson 2000; Hodgson 2001, 2004; Aoki 2001; Ménard and Shirley 2005; Nee 2005; Chavance 2008).

2The concepts of "East" and "West" will often be nuanced later in the text (see e.g., note 16).

3By ORDO I meant the Freiburg School and its intellectual *milieu* with their older and younger followers, without making a distinction between the less and more liberal authors as well as between theorists and politicians. Alfred Müller-Armack, Franz Böhm, Walter Eucken, Wilhelm Röpke and Alexander Rüstow were referred to just like some works of the secondary literature (see Watrin 1979; Zweig 1980; Peacock and Willgerodt 1989a, 1989b; Barry 1989; Johnson 1989. For more recent studies, see Sally 1996; Koslowski 1998; Vanberg 1998; Albert 2004.

4This simplistic dual typology was intended to reflect "public opinion" among economists in Eastern Europe at the time. In their discussions the alternative was presented with nonchalance to sharpen the contrast. For instance, the American type rarely included references to "old" American institutionalists (much of whose thought was directly imported from the *Historische Schule*) while in presenting the German one the importance of its Hayekian (libertarian, neo-Austrian, i.e., in a sense American) extension was underestimated. Important schools such as Comparative Economic Systems, the French regulation school or the theory of mechanism design would also not fit in well with such a dichotomy.

5At the time, one could hardly believe that a "pure" concept of market socialism cleaned from the dirt of real socialism, and reminiscent of the one used in the "socialist calculation debate" by Oskar Lange and his allies would not fade away from current economic thought (see Balcerowicz 1992; Bardhan and Roemer 1993).

6For speculative institutionalism, see Kovács 1992.

7Although this is a recurrent fear in liberal circles of Eastern Europe during the past two decades, reinforced by regime changes *á la* Milosevic and Tudjman, Lukashenka, Putin, the Kaczynskis and Orban, a coherent theory of new economic authoritarianism has not crystallized in the region ever since.

8Combining the two paradigms did not require a major scientific discovery; the pattern was set by the Hayekian (evolutionary) reinterpretation of the old Freiburg ideas. It could be safely assumed that evolutionary economics within NIE will not resist cooperation (see Schmidtchen 1984; Schüller 1987; Leipold 1988; Vanberg 2001; Pies 2001). See also the series *Konzepte der Gesellchaftstheorie* edited by Ingo Pies and Martin Leschke and published by Mohr (Siebeck) in Tübingen, in which they devote volumes to Buchanan, Coase, Hayek, North, Olson, Williamson, etc.

9The project was part of DIOSCURI, a large-scale FP7 research program (supported by the European Commission) on cultural encounters in the European economy (www.dioscuriproject.net). Unfortunately, we had to confine ourselves to making research in East-Central Europe, disregarding such important countries as Russia. I owe special thanks to Violetta Zentai with whom we run the program as a whole, as well as to Paul Dragos Aligica, Roumen Avramov, Vojmir Franicevic, Aleksandra Jovanovic, Jacek Kochanowicz, Alice Navratilova, Aleksander Stevanovic, Horia Paul Terpe and Tjasa Zivko who prepared country studies on the reception of NIE. For more details, see the following chapters in our volume (Kovács and Zentai, 2012; Aligica and Terpe 2012; Avramov 2012; Franicevic 2012; Kochanowicz 2012; Kovács 2012).

The case studies focusing on the reception of NIE were based on altogether more than 50 in-depth interviews, literature reviews including books and articles in one or two leading economic journals of the respective countries, curricula analysis at selected local universities, and participant observation. Also, in each country economic think tanks and university departments were examined in similar ways, and much of the information collected in these fields proved to be relevant for research into new institutionalist ideas, too.

10The case studies suggest that the early reception of new institutional thought was contingent on particular events such as the publication of a volume on law and economics in Hungary, a fellowship received by Leszek Balcerowicz in Germany, or a visit paid by Svetozar Pejovich in Belgrade.

11A Hungarian scholar remembers: "In the fourth year at the university, some 'wild liberals' from the George Mason University, I mean, neo-Austrians, came to Budapest, and invited those whom they thought to become the new leaders of the country to the West-Coast where we ate a lot, admired America and attended lectures. ... It was clear that they are obsessed but they did not expect us to agree with them. They ranged from anarcho-liberalism, through the idea of free banking to the classical liberals, and distributed books free of charge. ... They were mobilizing Svetozar Pejovich because he had an Eastern-European appeal. This I liked very much. ... At that time, I was a hard-headed liberal but, as time passed, my opinion has got much softer."

12With the exception of a few ex-Yugoslav (one Bulgarian and one Hungarian) scholars, the local experts were not taught by a prominent Western representative of NIE. As a rule, they met second-rate members of the school (e.g., in the framework of training programs), and encountered the top scholars at international conferences or guest lectures delivered by them in the region. Douglass North was among the few "frequent-flyers" to Eastern Europe.

13A notable exception is a group of Russian scholars at the Higher School of Economics and the Russian School of Economics in Moscow. Among them Sergei Guriev, Viktor Polterovich, Vadim Radaev, Konstantin Sonin, Andrei Yakovlev, Ekaterina Zhuravskaia and others. Some of them have been publishing in journals such as *Econometrica, Journal of Economic Perspectives, American Economic Review, Quarterly Journal of Economics,* etc.

14As the Romanian example suggests, old institutionalism does not necessarily have to originate in ORDO. It may borrow from other types of interwar theories such as "economic structuralism" that was conserved in the works of Mihail Manoilescu and instrumentalized by the national communists in the 1970s and 1980s.

15Let me refer to the unease I still feel reading, more than twenty years after 1989, about the "triumph of neoliberalism" in Eastern Europe (see Bockmann and Eyal 2002; Aligica and Evans 2009; Kovács 1991, 1998, 1999).

16The cold war concepts of "East" and "West" apply to the turbulent world of Eastern European economics less and less. Today, many of the Western professors, co-authors and project partners can also be encountered in the capitals of Eastern Europe (e.g., as an employee of CASE in Warsaw or of the CEU in Budapest). To complicate the issue, the Western professor can actually be a repatriate or an Easterner who was educated in the West. There he/she may have been taught by an Easterner, or, vice versa. Moreover, the professor may be a Pole teaching a Czech student at a Moscow University. An example from our field: as Avramov reports, a Russian NIE textbook was also used at the Sofia University.

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Call for Papers

International conference in Moscow

Embeddedness and Beyond: Do Sociological Theories Meet Economic Realities?

October 25-28, 2012

Conference theme statement

Over the last quarter century, new economic sociology emerged and evolved, by and large, within the broad theoretical framework of social embeddedness of economic action. While being initially rooted in the structural social networks perspective, the framework gradually expanded to integrate institutional and cultural arguments and to overcome the analytical separation between economic and social. More recently, it was complemented by the performativity approaches, which challenge traditional inquiries into the socially constructed nature of markets by focusing instead on their role in constructing (performing) societies. These developments show that the concept of social embeddedness has inspired a large number of insightful sociological theories and empirical studies of economic phenomena which, taken together, constitute a mature field of inquiry with its distinctive questions, arguments, and contributions. Yet, today's rapidly evolving and highly uncertain economic realities put these theories to a challenging test. Are they up to the task of thorough understanding market transitions in postcommunist and third-world countries, the continuing global financial crisis, or the new modern forms of calculability, governance, and social control? Given a rather static view of social embeddedness, how much can we say about the emergence, reproduction, and dissolution of networks, markets, and institutions, in other words, the dynamic nature of socio-economic reality? Or, on the contrary, about the stubborn resistance to change of old patterns of inequality and forms of governance? Does the proliferation of online purchases and Internet social networking sites radically alter the very notion of embeddedness? Overall, do our theories have enough "give" and can be slightly adjusted to answer such questions, or do we need a completely new toolkit to tackle them? The conference brings together the leading experts in the field who will concretize and explore these questions with regard to their own areas of research and theoretical approaches.

Status of the conference

Joint Interim conference of ISA RC02 "Economy and Society" and ESA Economic Sociology Research Network with the support of ASA Economic Sociology section.

Conference venue:

National Research University 'Higher School of Economics', Moscow, Russia

Plenary sessions and featured speakers

- Institutional change and social policy
  Frank Dobbin, Wolfgang Streeck
- Power of networks
  Roberto Fernandez, Brian Uzzi
- Culture and valuation
  Marion Fourcade, Laurent Thevénot
- Knowledge, technology, and markets
  Karin Knorr-Cettina, David Stark

Mini-conferences and their coordinators

- New theoretical perspectives in economic sociology
  Patrik Aspers, Jens Beckert
- Money, finance and society
  Nigel Dodd, Alya Guseva, Olga Kouzina
- Organizations and institutions in emerging markets
  Neil Fligstein, Vadim Radaev
- Emergence and innovation in markets and organizations
  David Stark, Valery Yakubovich
- Gender and work transformation
  Sarah Ashwin, Roberto Fernandez

- Market society and moral order
  Marion Fourcade, Kieran Healy,Philippe Steiner

- Performing economy in a material world
  Trevor Pinch, Gregory Yudin

- Capitalist globalization and its alternatives
  William Carroll, Georgina Murray

Selection of participants

Selection of the Conference participants is made by the mini-conference coordinators on the basis of submitted extended abstracts. Proposals can be accepted for oral presentation or as distributed papers. Full papers are submitted before the Conference.

Important deadlines

Deadline for submitting proposals:	February 15, 2012.
Registration is open from	March 15, 2012
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Early registration	May 15, 2012
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Deadline for submitting full papers:	September 15, 2012.

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No funding can be provided for traveling and hotel accommodation from the LOC Committee with the exception of featured speakers. All attendees, including other invited presenters, discussants, and coordinators, must pay registration fees.

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Contact information

Conference e-mail: esconf2012@hse.ru

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Announcement: The Economic Sociology Section of the *American Sociological Association*

The *American Sociological Association* has probably the largest section on economic sociology of any national sociological association.

The section is only ten years old – a young age by ASA standards – but it is one of the largest sections. Its impressive growth mirrors the growth of economic sociology as a subfield. Although classic sociological thinkers were keenly interested in the relationship between economy and society, economic sociology did not crystallize as a distinctive specialty until the 1980s. It received a much-needed boost with the seminal publication of Mark Granovetter's embeddedness article in 1985.

A dozen years later, several scholars started to discuss the possibility of organizing a formal section on economic sociology in the *American Sociological Association*. The section organizing committee was chaired by Wayne Baker and included Nicole Biggart, Neil Fligstein, Mark Granovetter, Brian Uzzi, Harrison White and Fernanda Wanderley, a graduate student from Columbia. They worked hard to set up the necessary section structure and recruit a significant number of members. In January 2001, the section was granted the status of a permanent section. The first issue of the section's newsletter Accounts: A *Newsletter of Economic Sociology*, edited by Joan E. Manley and Sarah Busse, came out in the fall of 2000. The first section-sponsored sessions on economic sociology were organized at the 2001 Annual Meetings of the ASA in Anaheim, California. Two sessions ("The Evolving Field of Economic Sociology," and "Global Financial Markets") as well as roundtables organized by Brian Uzzi ("Culture and Economy") were tremendously successful, drawing large audiences.

Ten years later, section membership surpassed 800, and it continues to grow. It is the sixth largest section of the *American Sociological Association*. Currently Woody Powell is the Section Chair.

The Section confers three annual awards:

- Viviana Zelizer Distinguished Scholarship Award for the best book

- Mark Granovetter Award for the best article (to be conferred for the first time in 2012; up to now, the Viviana Zelizer Award has alternated between books and articles)

- Ronald Burt Outstanding Student Paper Award for a paper by a graduate student

What are the benefits of joining the Section?

By becoming a member of the section you get involved in one of the largest intellectual communities of economic sociologists. It will be easier to follow ongoing debates in the field. You will receive the section newsletter, Accounts (here you can find past issues http://www2.asanet.org/sectionecon/newsletter.html), as well as information about relevant jobs, funding calls, upcoming meetings and publications. You will also have an opportunity to have announcements posted to the section website. In the very near future, we are planning on posting information about members' new publications on the website to have a comprehensive repository of economic sociology resources. If you attend the ASA's annual meetings that are held each August (next year it is in Denver, Colorado), you will be invited to section events, including the business meeting and the Economic Sociology Section reception, where the award ceremony takes place and you get a chance to mingle with fellow economic sociologists over food and drinks.

How to become a member

If you are not a member of the American Sociological Association yet, here's where you can join: http://www.asanet.org/members/join.cfm

If you are already a member of the ASA but have not yet joined the Economic Sociology Section, you can add section membership by going to the following website: https://www.e-noah.net/ASA/Login.asp Log in with your ASA ID and password, then in the upper right-hand quadrant, click on "Join a Section," and follow the instructions. For current ASA members, membership in the Economic Sociology section will only cost $12 ($5 for students).

Book Reviews

Book: Steiner, Philippe, 2010: Durkheim and the Birth of Economic Sociology. Translated by Tribe K. Princeton, NJ: Princeton University Press.

Reviewer: Alisa Maximova, Laboratory for Studies in Economic Sociology, National Research University, Higher School of Economics, Moscow, asmaksimova@edu.hese.ru

Being acknowledged as a classical figure in sociological theory, Emile Durkheim is seldom mentioned among the founders of economic sociology. However, Philippe Steiner in his new metasociological monograph *Durkheim and the Birth of Economic Sociology* tends to claim him to be one of the fathers of the discipline. The book is a translation of an original French edition entitled *L'école durkheimienne et l'économie. Sociologie, Religion et connaissance* (*Durkheimian School and the Study of Economy: Sociology, Religion and Cognition*), which was first published in 2005. Philippe Steiner is a well-known expert in economic sociology and history of sociology. The book presents the result of his long, fundamental and detailed research of French sociology.

A major idea proposed by Steiner is that Emile Durkheim and his disciples elaborated two research programs, both of them focused on sociological studies of the economy. Steiner reveals the underlying logic of these research programs and elucidates the origins and following development of the basic theoretical statements and methodological principles.

Only the first two chapters are devoted to Durkheim personally. Having outlined his approach in general, basic thoughts about the nature of economic facts, his attitude towards political economy and backgrounds of evolving research programs, Steiner turns to Durkheim's successors. François Simiand, Maurice Halbwachs, and Marcel Mauss are presented as key personalities.

First research program is related to Durkheimian critique of the political economy. In Durkheim's view, political economists largely abandoned moral and social perspective. He criticized their methodological foundations for focusing on abstract models instead of investigating reality and simultaneously argued against their inclinations to avoid complexity in analysis and interpretations. Following Durkheim, Simiand and Halbwachs revised economic notions and introduced a new sociological approach, employing social representations to explain phenomena associated with the economy and economic behavior. Moreover, Simiand came up with his own theoretical scheme aimed to provide methodological foundations for studying the economy. He created a classification of economic facts, making a distinction between production and distribution and defining several levels within these classes, ranging from social representations to institutions.

Durkheim strongly believed that it was possible to overcome the flaws of political economy by reconsidering and reformulating its methodological principles. For him, economic facts belong to social facts. He made an attempt to enrich and supplement political economy with ethical and sociological insights. Steiner indicates that Durkheim's point of view was shared by a number of economists, including such influential representatives of the German school of political economy as Albert Schäffle, Adolf Wagner and Gustav Schmoller. Steiner argues that Durkheimian approach to economic facts also resembles Keynesian approach trying to consider *homo economicus* in social context.

The second Durkheimian research program discerned by Steiner deals with the relationships between religion and economy. Using excerpts from Durkheim's personal documents (dated back to 1895), Steiner shows that Durkheim was quite impressed by a totally new understanding of the ultimate place of religion in social life that he was able to gain – at some place Durkheim calls it a "revelation". As long as all categories, including the economic ones, have their origins in religion, there is a direct link between religion and economy. Thus, sociology of knowledge acts as a mediator between sociology of religion and economic sociology. This revelation had certain effects on Durkheimian theory of society, causing gradual changes in his ideas. Second program was not developed systematically by Durkheim, although its important elements can be found in his later works. Therefore, Steiner turns to the analysis of *The Gift* by Marcel Mauss to describe the main outcomes of this program.

Steiner also dwells upon some other issues treated by Durkheim and elaborates their connection with economic sphere. One of them is the question of professional groups which plays a crucial role in Durkheim's understanding of solidarity in modern world because of the function of preserving the morality attributed to them. Another important issue deals with contract relationships that allow for sustaining economic exchanges without reestablishing trade relations and negotiating terms of transaction every single time. Durkheim also suggested a link between the origins of ownership with the distinction between sacred and profane.

Among the other issues, Steiner reviews the studies of wage and consumption carried out by Simiand and Halbwachs. He underlines Durkheimians' interest in American society, especially in systems of Fordism, Taylorism, and unionization. Steiner points at visible convergence between two Durkhemian research programs in the works of Simiand and Mauss. Finally, Steiner extends the framework by including Marx and Weber to analyze better a significance of sociology of knowledge for Durkheimian economic sociology.

After reading the volume one gains the impression that the original French title, *Durkheimian School and the Study of Economy: Sociology, Religion and Cognition* is more appropriate for its contents. The book is focused upon Durkheimian school rather than on Durkheim alone, and deals more with the study of economy rather than with ideas of economic sociology as we know it nowadays.

In this book Steiner applies a great variety of reasoning techniques and illustrative statements. The study encompasses various aspects of academic activity, methodology and theory. It describes relations between representatives of Durkheimian school, makes curious comparisons, and briefly renders the core of the most significant oeuvres. Steiner conducts a quantitative analysis by providing the number of quotations of economists made by Durkheim in his works by year, country of origin of the author and school of thought. He presents institutional background of studies which have shaped contemporary French economic sociology, explicates relations between social scientists, theoretical frameworks, and issues of critical debate. He demonstrates how the works of Durkheim and his disciples were received and developed by the academic community. Given this diversity of arguments and representations, *Durkheim and the Birth of Economic Sociology* would be an enjoyable reading for everyone who is interested in history of sociology, and economic sociology in particular.

Book: Steiner, Philippe, *Durkheim and the Birth of Economic Sociology*. Princeton UP 2010 [French original 2005]. Translated by Keith Tribe. ISBN: 9780691140551 256 pp.

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Steiner's book suggests to read Durkheim's sociology – and, of course, not only Durkheim himself, but also his colleagues and scholars - in regard to economic questions. Did Durkheim and the Durkheimians really write nothing relevant to sociology of economy? Could we believe Durkheim that he wasn't interested in the economic sphere? Is his sociology actually the antithesis of an economical sociology? – These are the questions Steiner adresses. The eight chapters then reconstruct the specific 'durkheimian' economical sociology. Several years after its publication in French, these questions might be a bit rhetorical: since the 1990s also others stressed the importance of the Durkheim school for the theory of economics (J. Heilbron, F. Lebaron, Ph. Besnard) – and since, as Heilbron (2001: 41) points out – analyzing "economy has been an integral part of the French sociological tradition". It might also seem a bit rhetorical because Mauss' *Gift* actually became a *manifest* for an entire social and sociological movement (which on the other hand distinguishes him from Durkheim). Finally, they might be a bit rhetorical, as there is a revival of Durkheim for instance in Bourdieu or the economics of conventions. However, Steiner's book is part of this revival; he pushed the Durkheimian economic sociology since 1992. After a long polemical tradition, this interest is part of a fresh look on the *école française de sociologie* – besides sociology of religion, sociology of law and morality, and cultural sociology.

Although we already know something about the Durkheimian views on economic life, it is a worthy undertaking to specify, deepen and systematize them, as they present a highly original point of view. It is obvious: that this school doesn't plead for an economic foundation of the social at all, it is by no means an economist approach and it pleas for neither materialistic nor individualistic perspectives. There are at least two statements by Durkheim in regard to economics which are broader known: *The Division of Labor in Society* (1893) characterizes segmentary and functional differentiated societies in their modes of professions, socialization and contracts, "an economic topic if ever there was one" (p. 1). But in doing so, Durkheim seems to be interested in other aspects of social life: especially in the mode of *social constraint or 'solidarity'*. Of course, there is secondly the *Suicide* (1897), in which he depicts modern economy as causing anomic suicides. Furthermore, there is Mauss' essay on non-utilitarian economies (1925); and finally there is François Simiand's *Le Salaire: L'évolution sociale et la monnaie* (1932).

But in general this school seems to be far away from economic questions since *L'Année sociologique*, at least from modern economy. Instead, it turns its eyes to the social origins of *knowledge and religion*; it is occupied with moral, legal, and parental questions in indigenous societies, with their institutions and the *'conscious collective'*. This 'socio-centric' approach emphasizes the primacy of social meanings, habits, traditions, and institutions over any individual act, emotion, thought; the primacy of social structure over the symbolic (a through ball for Lévi-Strauss to inversely stress the primacy of the symbolic). After the cultural turn, the new interest in the French school emphasizes *The Elementary Forms of Religious Life* (1912) as milestone towards a theory of the *symbolic constitution of the social* (although Durkheim himself might have thought it to work both ways); it emphasizes *The Gift* with the 'total' and constitutive function of apparently economical acts.

Steiner won't negate this. His thesis is that Durkheim nevertheless *was* interested in questions of economy; he namely developed two different research programs, which both pose the question of the origin of the social bound – with different answers. The first 'program' is the *"economical sociology of contract"* (*Division of Labor; Suicide*), which understands political and legal institutions as the foundation of moral bounds and limiting the private interest, which is the very nature of economic life. The *second* program (*Elementary forms*; Durkheim's articles in L'Année Sociologique) focuses instead on religious life as a constitutive basis for all other social spheres – including economy. This program connects three social facts: *economy, religion, and economic knowledge*. The original title of the book is therefore (for the second program) more precise: *L'école durkheimienne et l'économie: sociologie, religion et connaissance*.

These two programs are represented in a chronological order, first in Durkheim (Chap. 1, 2), than in the Durkheimians (Chap. 3-6). Mauss and Simiand are particularly present, as they synthesize these two programs. The chapter about Mauss supposes that in *The Gift* he executed Durkheim's "agenda, seeking to link […] economic sociology to the sociology of religion" (p. 140). Simiand was responsible for the section for economical sociology in *L'Année*. Besides critical reviews, he offered a 'positive economics', centering on social expectations and valuations while regarding money as *fait social*. Analogous to the religious sphere, and originally an ornament with magical functions, money is a 'hypostatisation of society' (p. 161). Thirdly, Halbwachs is discussed (but not in an own chapter, since he mainly 'repeated' Simiand, p. 76-80) who followed Simiand's position in the journal (now *Annales sociologiques*). What he shared with Simiand, Mauss and Bouglé is the critique of an economic world view, in an intense discussion with classical economists. He was the Durkheimian interested in Max Weber. While these chapters are contributions to the history of sociology, the last two emphasize the relevance of this "Sociology of economic knowledge" (Chap. 7), showing "convergences" with Weber, in particular with regard to the thesis of rationalization as an unintended process (8).

I'd like to focus on the two programs at the expense of the 'executives' (and be that to keep tension). The first program seems clear. The anti-materialistic trait, the critique of any theory, *"which reduces society to … a vast apparatus of production and exchange"* (Durkheim in 1898) is one of the driving forces of this sociology. It aims to explain 'the social from the social' and not from the economic-political sphere alone. How does the second program go? Religion and economy should be seen as intermitted "in the medium of sociology of knowledge". Still opposed to an economic foundation of the social, Durkheim wants to reveal the religious foundation of economy – namely after his "revelation" in 1895, where he 'at once' discovered the role of religion in social life (p. 39). As a letter to Mauss from 1897 indicates (p. 57), Durkheim wanted to *"make religion, not economy, the matrix of social facts"*. A first argument is unfolded in his review of Spencer/Gillen (*Sur le totémisme*, 1900): totemic animals only have a secondary, *unintentional* economic function; the religious function is its proper one. A second argument is developed at the end of *The Elementary Forms*: for its existence every society needs an *ideal* of itself (a collective representation), which has to be actualized periodically (p. 55). It needs common *passions*, an intensive collective life – whereas for Durkheim economic actions are considered humdrum, daily practice without social force (as everybody knows, that's the place of the famous invention of Marcel Mauss: the *total social fact* – of practices, which are at the same time economical, religious, 'moral', and legal). Religion is primary, constitutive; whereas economic life is only the in-between. In other words: economy is based on religion and related to a specific economic knowledge (ideals and passions). This thesis of a tension between the two spheres (sacred/profane) converges with Weber's approach.

To conclude, the (wonderfully crafted) book brings at least two benefits. 1) Blending with the cultural or symbolic turn, it offers a relevant and (together with other publications) new noticeable paradigm of economical sociology. 2) For those who are interested in the history of sociological thought, it allows insights into the mode of work of the Durkheim school, its methods, results, and theories. It makes its own way through this complex working collective. This way is surely only one of several possibilities - one could miss in particular the Durkheimian *anthropology or ethnology*, as this sociological school was actually the founding school of French ethnology. Lévi-Strauss is present very briefly and Mauss' thesis on the non-utilitarian function of exchanging goods, signs and women seems richer than Steiner indicates; Hertz could have been discussed in more detail, just as Maunier or Granet (and Halbwachs). The 'revelation' of Durkheim is more contested as it may seem. However, this would be another book – indeed, the author is interested not in an anthropological theory (where economical, legal, religious, and political aspects are intermingled), but in a sociology of modern economical knowlegde.

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Book: Chris Hann and Keith Hart, 2011: *Economic Anthropology: History, Ethnography, Critique.* Cambridge, UK: Polity Press.

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The current financial crisis has exposed a fact already well-established in economic anthropology and sociology: Even in global financial markets, which operate through complex computer networks and in which selling and buying is dematerialized and often occurs in cyberspace, the social embeddedness of economic action still matters in shaping economic exchange. Much like the Industrial Revolution and the Great Depression, which transformed society and the social sciences, the current crisis is presented by the authors as an occasion for economic anthropology to become a distinct discipline. The book *Economic Anthropology: History, Ethnography, Critique* represents an attempt to bring insights from economic anthropology to the forefront of the current debate about the socio-cultural organization of markets and their moral dimensions. The book offers a methodological and analytic platform which could make this field more relevant for policy making, create a more fruitful dialogue with economics, economic sociology and history, and make scholarly work more accessible to the wider public.

Clearly, the book is ambitious in setting its goals. It wishes to be a text book which provides an historical review of the main intellectual currents and debates in economic anthropology from the late 1800's, provide in-depth analysis of leading scholarly work within each stream of thought. At the same time the book presents a clear normative statement regarding the future development of the field. As indicated by the book's title, the authors call for studies which combine a deep and broad historical understanding of the research site as well as a good command of the intellectual history of economic anthropology itself. In addition, the authors' call for quality field work which goes beyond what they call "narrow spaces" and which relates to a broad agenda, which often requires a comparative research design. The authors, who are important contributors to economic anthropology, also wish this field to regain its critical edge by questioning well established "facts" about the local economy being investigated. While reading the first chapters, which contain a history of the field, I remained rather skeptical about the possibility of combining a text book with such a strong programmatic statement. But as I progressed with my reading, I was impressed by the authors' ability to weave together in-depth reviews and critiques, and I felt that they do deliver what they promise in their introduction.

The authors' analytic and methodological angle is best reflected in their definition of a key concept in the book: "Human Economy," which is juxtaposed with neo-liberalism and "Homo Economicus," as presented by economists. This term is important for the authors: "Because our focus is on what people actually do and think; economic action is directed towards the well-being of whole persons and communities, not a mechanical and one-sided individualism" (p. 170). In addition to the negation of the under-socialized image of human agency in economics, the term human economy also reflects the authors' inclination to focus on framed human interactions in which decision making takes place within communities and other social institutions with their distinct historical and normative background. Building on Mauss's seminal study of gifting, the authors assert that the definition of human economy is also based on the premise that "…we are all both individual and social; economic action is always in varying degree interested and disinterested. If we aspire to being human, it will not do to hang on to one pole of this dialectic as the expense of the other." (p. 173)

Following a short introduction which outlines the book's aims and structure, chapters 2-5 present the intellectual foundations of economic anthropology, including a short section about the history of economic thought and a short review of Marx' contribution to political economy. Chapter 3 was particularly interesting, with a concise review of different schools of thought within the emerging field of economic anthropology (German, British American and French). One of the most well-known debates between formalists and substantivists is the focus of chapter 4, with an emphasis on Karl Polanyi's scholarly work. Chapter 5 covers the diversification of research streams which occurred in the 1970's and 1980's, following the decline of this heated debate. This chapter covers French Marxist economic anthropology, feminist scholarly work about domestic labour as well as a discussion about women who hold a prominent role in West African markets working as traders (p. 79). The subsection titled 'The Cultural Turn', which covers Clifford Geertz and Marshall Sahlins' work, among others, was interesting and thought provoking, particularly the critique of the distinction between capitalist and non-capitalists economies based on the opposition between gift economy and the exchange of commodities. Here (p. 87), the authors comment that: "Mauss wrote his essay (1925) to refute the bourgeois opposition of commercial self-interest to the altruism of the gift. For him, the archaic gift was a hybrid of the two extremes and the problem is to understand how we have come to separate them (Parry, 1986)."

Chapters 6-8 are more analytic and critical than the previous ones, each of them focusing on a sub-theme in the more-recent economic anthropology (Unequal Development; The Socialist Alternative; and One-World Capitalism). Chapter 7 about the exploration of the socialist alternative within economic anthropology (pp. 121-139), was particularly educative, specifically the review of studies of the post-socialist transformation. Chapter 8 covers studies of post WW2 capitalist societies, and provides a good illustration of the authors' ability to promote a fruitful dialogue between economic anthropology and sociology. In various places in the book the seminal contribution of economic sociologists is acknowledged, and the ability of some economic sociologists to remain focused on larger issues rather than delving into complete relativism as many contemporary economic anthropologists have done, is acknowledged. The creative dialogue with sociology is apparent in two subsections in this chapter (Industrial Work and Corporate Capitalism). An important critique of the cultural turn in economic anthropology presented here is that "The cultural turn of the neoliberal era has tended to bury the economy from view or allows it to appear only as consumption or circulation." Following Marx, the authors' remind us that "production and consumption are inextricably linked within a single economic totality" (p. 170). The final chapter highlights the main themes of the book and offers a clear statement of the future development of the field.

Sales work and sales encounters produce and reproduce markets, and given that economic anthropologists studied selling and buying in pre-industrial society extensively, I was always struck by the scant amount of research attention to sales work and sales encounters in contemporary commodity markets. I was also hoping to find in this book an explanation to the current fixation in economic anthropology and sociology on marketing and global financial markets. The opening sentence of the subsection titled "Money and the Financial Crisis" which reads (p. 159): "The process of getting people to spend their money on consumption, the art or science of selling- is also a rapidly expanding field", created an expectation of being exposed to such studies. But in reality the following pages review mainly studies of marketing, not sales, and a few studies of financial centers. The relative neglect of selling and buying in advanced commodity markets remains to be explained and rectified.

I enjoyed reading this book, which would make important reading for graduate students in economic anthropology, as well as for graduate students and scholars within sister disciplines such as sociology, political science, history and philosophy. Given the very clear style of writing and the vivid explanations of complex ideas, educated readers outside of academia would also find this book educating and interesting.

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Parry, J., 1986: The Gift, The Indian Gift, and the 'Indian Gift'. In: *Man*, 21(3): 453-473.

Book: Matthew Gill (2009 [2011 first published in paperback]): *Accountants' truth. Knowledge and ethics in the financial world,* Oxford & NYC, Oxford University Press, 198 pages.

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At first glance, accounting may appear to outsiders all expertise, if not to say dull technical know-how (cf. the cult Monty Python vocational guidance about the accountant who wants to become a lion tamer). Accountants, the joke goes, solve problems we didn't know we had in a way we don't (want to) understand. In fact, accounting is of the foremost salience in the analysis of capitalist modern societies.1

Accounting paradigms as well as the professionals' actual practices always crystallize the relations of capital appropriation among corporations and also between firms and their stakeholders (employees, subcontractors, suppliers, local communities, tax payers, regulators of all kinds, the fiscal state, lenders, and shareholders). Therefore, accounting is not "pure" measurement accuracy; it performs numbers and valuations that reveal tensions between conflicting definitions of business activities, on the control over the firms, or the allocation of profit they generate. As Sir David Tweedie, the former head of the International Accounting Standard Board (2000-2010), bluntly put it *"accounting is not just bean-counting anymore, it's moving into the fields of economics and finance"* (*Financial Times,* March 21, 2001).2

In our times of financial scandals and economic crisis, *Accountant's truth* could appeal to social scientists at large, since the book addresses, in an accessible and concise manner, issues that are crucial for the understanding of the economy and of its actors: the construction of knowledge; the making of trust and the crumbling of professional ethics. Matthew Gill's core idea is that frauds, or at least equivocations in the production and interpretation of accounting figures, are the by-products of the increased rationalization of accountants' work. Ethics is thus narrowed to rule-compliance tricks of the trade, by accountants who have to perform their professional role in front of their clients, their management and themselves.

The book develops this idea along 6 chapters, each of which adopting a different approach on the materials gathered through the in-depth interviews of 20 male chartered accountants, in their thirties, working in London for one the four leading auditing firms worldwide (see the book's methodological appendix, p. 154-161). The author mitigates this small sample (and the absence of sociological treatment of the interviewees' social characteristics) by the breadth of his knowledge and own experience.3 Therefore, he makes his interviewees speak of their work as a performance (chapters 2 and 7). He makes them react to a simplified case-study which clearly illustrates the conflicting interests and divergent perceptions of an actual business which frames their making of accounting figures (chapter 3). Through this "experimental" sociology, the author masterfully explains to non-specialists "the disparity […] between the apparent clarity of an accounting decision once made, and the opacity of the decision-making process itself" (p. 54). The reduction of accountants' ethical issues to technicalities is then analyzed in chapter 4 and their evocation through manly metaphors inspired by sport and military strategy punctuate the fifth chapter. In chapter 6, the author addresses the classical sociological question of professionalism. It may also be here that the limits of the book crystallize.

For instance, the narrow focus on a small group of "freshmen" who do the job on a non-permanent basis, with mainly instrumental motivations (gaining a qualification, money and status, or access to a more prominent position), neglects the organizational features of the firms (the "Big Four"), the system of professions and the field (i.e. the City of London) in which these men adopt job habits, acquire common cognitive frameworks and yearn to develop their careers. This lack is all the more detrimental that Matthew Gill's conclusion calls for a redefinition and reinvigoration of "professional ethics". Such a claim may only be more than cheap talk if and only if it abandons the individual level and points to the reforms at the industry, organizational and instrument levels which could trigger-down constraints and incentives that would, over time, (re)shape the accountants' *habitus* (their categories of perception, appreciation and action) and, eventually change their actual practices.4

Endnotes

1Chiapello, Eve (2007:, Accounting and the Birth of the Notion of Capitalism. In: *Critical Perspectives on Accounting,* 18, 263-296.

2Botzem, Sebastian and Quack, Sigrid (2009): (No) Limits to Anglo-American Accounting? Reconstructing the History of the International Accountings Committee – A Review Article. In: *Accounting, organizations and society*, 34, 8, 988-998. Intriguingly, Matthew Gill does not specifically address the issue of accounting paradigms ("fair value" vs "historical cost") although it is fiercely debated among academics, regulators and accountants.

3Matthew Gill was trained and worked for 4 years as a chartered accountant for PricewaterhouseCoopers. Now, he works for the Financial Services Authority in London. For a more detailed cri tique of the sample and the methodology: Abbott, Andrew (2011): Review of Accountants' truth. Knowledge and ethics in the financial world. In: *The Accounting Review*, 86, 359-361.

4Bourdieu, Pierre (1990 [1980]): *The Logic of practice*. Cambridge, UK, Polity Press. For an application.

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