

INTERVIEWS

Interview with Victor Nee: “Sociologists Always Want to Reinvent the Conceptual Map and to Come up with the New Conceptual Scheme that Identifies Something as Important” (Part 1)



NEE, Victor — Professor of Sociology, Department of Sociology; Frank and Rosa Rhodes Professor of Economic Sociology, Director of the Center for the Study of Economy and Society, Cornell University. Address: 312 / 330 Uris Hall Cornell University, Ithaca, New York 14853-7601, USA.

Email: vgn1@cornell.edu

Victor Nee was interviewed by Alexander Kurakin, Senior Lecturer at the Higher School of Economics. This interview was conducted during the international conference “Embeddedness and Beyond: Do Sociological Theories Meet Economic Realities?” (Moscow, 25–28 October 2012), at which Prof. Nee co-chaired the mini-conference “Organizations and Institutions in Emerging Markets.”

Prof. Nee emphasizes that economic sociology cannot be built only on the argument that networks matter. While important, this sole focus does not allow economic sociology to progress to the societal level. For that, economic sociology needs to incorporate institutional structures — both formal and informal — into its analysis, as it extends the sociological perspective to the study of economic behavior.

Prof. Nee also discusses his recent book “Capitalism from Below” (co-authored with S. Opper, 2012), which puts into practice and explains through such multi-dimensional institutional analysis how private-enterprise capitalism emerged in China.

In this issue the journal presents the first part of the interview. The second part will be published in May 2014.

Key words: embeddedness; networks; institutions; capitalism; China; institutional change; economic growth; economic performance.

— *So, let’s begin. Your last interview in Russia was about 8 years ago [Nee 2005].*

— That long ago?

— *Yes. Back then, you told Prof. Vadim Radaev that you began to realize that you were working in the field of economic sociology after reading Granovetter’s article about embeddedness [Granovetter 1985] and Swedberg’s book of conversations with sociologists and economists [Swedberg 1990]. So, these were two books that influenced you. I’m curious — do you think that Granovetter’s article remains so crucial for the identity of economic sociologists today?*

— Well, that's an interesting question. I noticed that the conference your institute has organized uses the title of my article "Embeddedness and Beyond" [Nee, Ingram 1998]. I was pleased to see that, because that article was written at the time when it became clear to me that you cannot build the whole field of economic sociology on the argument that networks are important and networks matter. Of course they matter. No one can argue against that. Granted, it was an important argument to make, and important in two senses: first, as a critique of economics for its omission of social structure, and second, because network analysis is an area where sociology has made important progress, both in terms of methodology and also as an important focal point of research.

But networks are a social structure at the micro-level and meso-level, while the great strength of sociology as a discipline, as Durkheim argued, is that it specializes in the study of institutions, of larger social structures and arrangements, that are above the individual and meso-levels. In short, at the societal level. Granovetter's 1985 article missed this. Why? In part, I think, because Granovetter was in a polemical argument with Williamson, who is a new institutional economist. And so Granovetter framed the argument, in part, as a dualism: networks *versus* institutions. This was not emphasized in that article, but it was there and so, in a way, Granovetter inadvertently ceded the grounds of institutional analysis to Williamson by pushing hard on the network side. Now, that's my interpretation of it. It may be so. Later on, Granovetter began to write again about institutions — before this article he had written about institutions of labor markets. So, it's not that institutions are missing in Granovetter's work. They're there. But in that particular article he posed the dualism: either/or. It's networks *versus* institutions, sociology versus economics, which is not a strong, long-term intellectual position that you can hold, because it's always both: networks *and* institutions.

Sociology and economics have, for a long time, had intellectual trade with each other, influencing each other, which is very clearly evident in Richard Swedberg's book where he interviewed both sociologists and economists. So, economic sociology is a subfield that is especially open to intellectual trade. I do think that Granovetter's article had a very important influence in revitalizing economic sociology during that period, but although it was an important orienting statement, its limitation became more and more clear to researchers.

You cannot explain economic performance or economic behavior in the broader sense by simply remaining at the level of networks, because you need to take into account the institutional structures in which the networks themselves are embedded. *How* networks matter could not be really fully examined without taking into account these institutional structures, both formal and informal. And so that was the purpose of my article with Paul Ingram, "Embeddedness and Beyond." We say: "Look, we need to bring into the focus of economic sociology the institutional structures within which economic action takes place, which must include the networks themselves. But the networks are part of the informal behavior in institutions, and the formal structures of the economy are the formal arrangements." So, it's a more complete analysis. I posed in that article the central issue that needed to be resolved: What is the relationship between the informal and the formal elements of the institutional structures? Because we do know, for example, that in some contexts they are at odds with each other and that the informal arrangements are opposed to the formal rules of the game. And in other instances that they are congruent, that the formal rules of the game and the informal norms, the social norms, are in alignment with each other, creating a very different situation. When informal arrangements and the formal rules of the game are congruent, you have lower transaction costs and higher economic performance, simply because much of the monitoring takes place within networks according to the informal rules of the game, the norms. In opposition, the informal rules are opposed to the formal rules of the game, leading to higher transaction costs with respect to both the economic players and also to the state overseeing the enforcement and monitoring of the formal rules of the game.

The use of this framework in my book, *Capitalism from Below* [Nee, Opper 2012], is a direct extension of "Embeddedness and Beyond," to explain why capitalism emerged in China. And so you have a theory that

uses a Schelling model confirmed by a massive amount of evidence using a multi-level causal model that includes the economic actors as part of the networks and also the informal and formal institutions in the explanation for the emergence of a new economic order. It is in essence a sociological explanation of capitalist economic development that builds on the shoulders of giants — Max Weber, Joseph Schumpeter, George Homans, and Harrison White.

I was very pleased that this book is being read seriously, not only by sociologists but by political scientists and economists who recognize it as an important contribution to both sociology and economics. That's the way it should be: we are not in opposition (economic sociology to economics) but we take a different approach. We extend the sociological perspective to the study of economic behavior, and if we're successful, the economists will recognize that we have come up with a robust finding that they cannot explain from within the neoclassical paradigm. They cannot explain it, even within the framework of new institutional economics. It has to be that our findings are robust enough that it's not just among our friends and within our networks of economic sociology that they're recognized, but that they have external validity.

— *And how do you know that your book is recognized? It was just published not long ago.*

— We have some reviews already. Reviews from economists, published and also unpublished.

— *And can you name some economists who recognized your book or maybe published some reviews in economic journals?*

— There is one by Nitin Desai. I was pleased to see this, by a very distinguished Indian economist based in Delhi, in a journal published by the Institute of Economic Growth, which is somewhat on the left, a Marxist-origin institute. He gave a very careful review of the book, which showed that he really thought about it. That's the first published review we have. Two economists have posted wrote short reviews of the book on Amazon. One was by Lee Alston, of University of Colorado, Denver — a very distinguished new institutional economist, a former president of the international society for new institutional economics. He said that the book changed his thinking about China. And economists are very interested in China, because it's the economy that has the highest rate of economic growth in human history for the longest period (three-and-a-half decades), emerging from the poorest-per-capita economy in the world to the second largest.

— *They have to be interested.*

— They have to be interested and they can't explain this. No explanation for it. They can't explain why it happened.

— *Even the neo-institutionalists?*

— No, they don't have an explanation, because the new institutional economics require that the institutions are in place first and then you have economic development. Good institutions prelude good economic performance, bad institutions stop it. Chinese institutions were weak — weak in terms of copyrights, especially intellectual copyrights.

Douglas North read an early version of the book, and apparently told his friends, "You've got to read this book." It was reviewed by a Greek economist who has written on new institutional economics, and we got informal feedback from another German new institutional economist who said it was a major contribution to economic science. In conversation with me, the economist John Leahy said: "This book explains economic growth. This is a major contribution."

— *Do you mean that even neoclassical economists are reading this book?*

— I don't know if they are, but there was one encounter I'll mention here. He had not read the book, but he was at my presentation and came up to talk to me afterwards. In any case, my view is that the neoclassical economist who reads the book will understand its logic, even though that logic is sociological. By the way, this is the challenge for economic sociologists. We are not just writing only for ourselves, and to convince people, we need to come up with findings, theory-driven empirical findings, that have external validity, not just within sociology but in our ally disciplines: economics and political science. That's the challenge. They may not agree with us, but after they read our work, they need to be able to say: "Yes. This would explain something we cannot explain." I recently invited James Robinson, who is the co-author of the new book *Why Nations Fail* with Daron Acemoglu [Acemoglu, Robinson 2012] and teaches in government department at Harvard, to speak at Cornell. *Why Nations Fail* basically extended the new institutional economics to the whole world and to all human history. Their theory is that there are two types of political regimes: fundamentally extractive political institutions and inclusive political institutions. Extractive political institutions are states where the government is predatory and extracts surplus from producers, and the state builds extractive economic institutions to enable politicians to extract more from the economy for the interests of the politicians, the political elite. Then there are inclusive political institutions where the rules of the game are such that the rule of law and accountable government limit what can be extracted from economic actors by the government. And in inclusive political institutions you have a tendency to develop inclusive economic institutions. This is the Acemoglu–Robinson model. In this model they essentially analyze all of human history, and it's very clear that the type of political institutions that they mean by extractive include state socialism, socialist Soviet-style economies as well as dictatorships in Africa and other underdeveloped parts of the world. In their view, China is an example of an extractive political state, but then they face a problem just like the other economists: they predict that China will collapse. I met Robinson and I gave him a copy of the book. He read it and said: "That's changed my thinking." They can see that the political institutions and the economic institutions in China, from their framework, which is not my framework, suggest that it is inclusive. So, to me it was a very strong signal that we have come up with the theory and evidence such that two major important economists — Robinson and Alston — change their thinking. That's what we want: work that causes the economists to rethink their subjects and to recognize that we stood up and we delivered.

We, sociologists especially, always want to reinvent the conceptual map and to come up with the new conceptual scheme that identifies something as important. I think there is a contribution to be made in doing this, and, as I said, Granovetter's 1985 article was just this — an orienting framework that identified something important that was missing. But if that's all you do, you haven't delivered on the robust findings. We need to do more. We have too many orienting statements. This was what George Homans emphasized and also what Robert King Merton emphasized. What sociology has to do is to deliver on theory, linked with evidence, theory-driven evidence, discover new evidence, new regularities. And this is why Merton emphasized the importance of middle-range theory rather than grand theory. Middle-range theory is the level of theory that is testable and that drives empirical research.

In my view, that's why Granovetter's article, while important, as I emphasized for myself, ran into limitations. There wasn't a specification of a theory. Its main contribution was to say: "Networks matter, both in markets and for the firm." To Mark's credit, he said: "Well, we don't know when they matter to improve trust and economic performance, because we don't know ex ante when those, whom we trust, can cheat us and can be opportunistic, because it's only those whom we trust that we're less likely to monitor. These are the people who are the great embezzlers of history, because we're not keeping an eye on them." So, it is to Granovetter's credit that he understood this limitation, but it's precisely for that reason that Granovetter argued that we need to focus on case studies to study how networks mattered. The problem with that, as Harrison White argued in *Markets from Networks* [White 2002], is that it leaves us in the position of an anthropologist, studying one

network after another without the contribution of a predictive model, without the ability to explain something that is beyond the particular case.

— *In the previous interview you were also critical concerning such case studies. Did something change in these 8 years?*

— Well, I didn't mean to imply that we should not be doing case studies. For example, our study of the emergence of capitalism in China, which I see in the same spirit as Weber's study of the rise of the capitalism in the West, also a case study. In his writings, though, Weber posed an alternative theory of capitalism to explain the rise of capitalism in its modern form in the West, not from a materialist theory, as Marx did, but from a theory that saw the causal importance of cultural beliefs, of ideas. That was an important contribution that went beyond the limited case of the rise of capitalism in the West. In *Capitalism from Below* we study endogenous institutional change that led to the emergence of economic institutions of capitalism in China, but the Schelling model in that multi-level casual theory is a general one, which can be applied to other cases.

— *So, you expect from those case studies some broader theoretical impact. Am I right?*

— Yes, that's right. For example, my current study of the knowledge-based economy in New York City, with Sonja Opper who is at Lund University in Sweden. This is a very different institutional context, and a very different case from the China case, which was in Yangzi delta. Why New York City? Well, in the last five years it has had the highest rate of high-tech startups in the United States, if not the world. The median age of new high-tech firms in New York City is about three to four years. In the 21st century New York has already founded 1,600 high-tech firms. Knowledge-based economic development just exploded in New York City.

— *The other Silicon Valley.*

— Exactly. Now, Silicon Valley is still number one in terms of high-tech startups, but it used to be Boston in second place. New York City is now number two and growing at a faster rate than Silicon Valley. Five hundred of these 1,600 firms have funding from either angels or venture capitalists. The median investment among those five hundred that have funding is about 7.5 million dollars. The mean is about a million and a half. So, these are not just fly-by-night firms. These are firms that have attracted significant venture capital, and so I'm doing this study to explain the emergence of this explosive growth of high-tech start-up firms in New York City.

This is a case of congruence. China was a case of incongruence, opposition norms, driving institutional change and putting into place economic institutions of capitalism. In the US, the economic institutions of capitalism are already in place. The formal rules support the free enterprise system, and so, the emerging norms and networks of high-tech firms are congruent with the formal rules of the game. That's what we are studying in New York, the opposite case from China. We want to see how emergence takes place where, in contrast to China, the institutions are well-aligned at the informal and formal levels. In China, it was the emergence of informal economic institutions, driven by networks and social norms, that forced the state to change the formal rules to accommodate what had already taken place on the ground, in the real economy. In New York City you have the municipal government working together with the high-tech startups, never in opposition to the emergence of knowledge-based economic growth in New York City, but accommodating it, helping, rather than standing in the way.

— *So, is this study finished or is it in progress?*

— In progress. The time-line is to finish this research in another year.

— *Will this study appear as an article or a book?*

— Articles and a book. *Capitalism from Below* also continues as ongoing research. Right now, a third wave of that study is being conducted in China with 700 entrepreneurs and 700 firms. We expect to complete it by the end of 2013. In this third wave we're working together with Sonja Opper and Ronald Burt, with more fine-tuning of the network analysis we're doing. And then there will be the fourth wave in 2015. So, I'll be very busy.

— *Like good movies. So, am I right that in the previous interview you were quite optimistic when you were talking about the connections between sociologists and economists? And now I see that you are still optimistic.*

— Well, since the previous interview there was a book published by Avner Greif, *Institutions and the Path to the Modern Economy* [Greif 2006]. In that book, which I think is important, Greif argues that there has been a sociological turn in economics. Now, not every economist agrees with Greif, but Greif is a major economist who has made this argument. As economic sociologists, we should welcome and encourage that turn, because we benefit from intellectual trade between sociology and economics. It might be a rivalry, but I think that the competition is a good competition. Whether we've made progress since? Yes, we see this in Greif's publication, that he cites sociologists, that he calls for a sociological turn in economics. Well, when I submit articles or research reports, half of them go to sociological journals and half of them go to economics journals. I've just had a co-authored paper [Holm, Opper, Nee 2013], accepted by *Management Science*, which is a leading business-school economics journal. Prior to that, I've published in *Kyklos* [Nee, Opper 2009] and in the *Journal of Institutional and Theoretical Economics* [Nee, Opper, Kang 2010]. I was pleased to do this, because with each of these articles I tried to make sociological arguments about economic performance available in the economics journals themselves.

It is a challenge to get accepted in economics journals for anybody, because submissions undergo peer review. But when economics journals ask for a review, it's usually from an economist rather than a sociologist. And so, of course, I'm pleased to get these articles accepted. Whether or not the economists are influenced by them, I just don't know. But let's say this: economists are now in the field that Mark Granovetter began his career in, network analysis, and have written the definitive text on network analysis. This is a book by David Easley and Jon Kleinberg [Easley, Kleinberg 2010] which was published about three years ago. It's a major book on network analysis by an economist and a computer scientist. And so, economics and computer science are running with network analysis now. Yes, they do cite Ron Burt, but most of their citations are to computer scientists and economists, because they've done so much of the recent work in network analysis, and their technical skills are stronger than the average sociological network analyst. So, we can still say we are the subfield of modern network analysis, but we need to acknowledge that modern network analysis, particularly the cutting-edge, is now mostly done by computer scientists and economists and mathematicians. They see what we're doing is important enough that they've grabbed it and run with it. Sociologists barely keep up with the technical aspects of the new research, and this is a problem.

That's why we need to not surrender institutional analysis to the economists. The international society of the new institutional economics is founded on the idea that institutions matter for economic performance. Well, what do they mean by institutions? The same thing that Durkheim meant by institutions? The same thing that Weber meant by institutions? The same thing that Marx meant by institutions? The economists have taken the sociological view of institutions and say: "They matter for economic performance." Now, we can't surrender that. We have to counter that with: "Yes, we agree, but our theory provides the analysis that you cannot do from the neoclassical framework. That is, the extending of sociological analysis to the field that you're researching and we're researching. And we sociologists can deliver on the promises of our past masters — not only from our framework but from yours. We've done something that you can't do, or at least haven't thought about yet."

— *You've just said that economics is ahead in network analysis, in institutional analysis. What should sociologists do, if economists are getting ahead in everything they are doing?*

— Well, I don't say, they're ahead in everything, in networks and institutions, but...

— *They have better techniques of analysis.*

— Yes, but if you only have technique and you don't have the substantive analysis, that is not going to win the game either, because methods and techniques are tools for analysis. But the main use of these tools is to discover regularities in the world we live in. So, if, their methods are coming out with better models, but not making discoveries in regularities in the world we live in, then they haven't delivered yet. They're just simply nice tools, fancy tools, perhaps. But we need to focus on what is being discovered in the way of regularities in the world we live in. By analogy, in another field I've done research in, immigration, you have oftentimes economists and sociologists working in concert. It's well accepted that a major article draws on work published by economists as well as sociologists, whether the author was an economist or a sociologist, because in this area people recognize contributions that are either theoretically important or empirically important. And so they will cite those contributions, not simply because it's in their discipline, economics or sociology, but because they need to have a proper citation of the ideas and findings being made in explaining and understanding immigration. And so, whether it's network analysis or institution analysis, hopefully that will work just as well, that economists and sociologists will refer to each other's work on its merit. This is why Greif's book is an important signal, because Greif, an economist, refers to the contributions made by sociologists to the institutional analysis of the economy.

Capitalism from Below does the same thing. We refer to important works in sociology, as well as to works that are relevant in economics, for the theory on endogenous institutional change. So, my optimism is not a Panglossian optimism, it's based on what I've seen in another field, the study of immigration. This type of ethos could well emerge in the relationship between economic sociology and, say, new institutional economics, two subfields of larger disciplines that rub shoulders, because both subfields agree that institutions are important to explain economic performance and behavior. The contribution of economic sociology is the premise that no full understanding of institutions can be achieved without examining the informal institutional processes that are embedded in networks and norms. A limited focus on the formal apparatus of institutional structure, the rules of the game in terms of laws and regulations enforced by the state, will not get you there. I think that, since 2004, we've made a lot of progress there. At least, I see it in a number of studies that have been published by economic sociologists since then. Frank Dobbin's *Inventing Equal Opportunity* [Dobbin 2009] is another example of sociologists examining institutional change, in that case, to explain how firms have incorporated a change in the rules with respect to race and gender. And then there was that recent study by Marion Fourcade on differences in the economic discipline in three countries: United States, Great Britain and France [Fourcade 2009]. And then there was another book by Greta Krippner [Krippner 2011] of neoPolanyian flavor to explain the financialization of the American economy: why finance became the leading sector of the American economy, shifting away from manufacturing. She of course is a neoPolanyian institutionalist. There are different varieties of institutional analysis in sociology. There's neoPolanyian, there's new institutional economic sociology that Ingram and I first pushed for, and then there is neo-institutional organizational theory. All three are complementary. They're complementary with different emphases.

— *And is Krippner's work in line with, for example Neil Fligstein's work? Because Neil Fligstein also wrote about the rise of the financial conception of control. And does Krippner take issue with Fligstein or not?*

— I should have mentioned Neil Fligstein as part of that "institutions and beyond" movement. Well, Krippner takes a different path from Fligstein. His work focuses on the firm and its interaction with the institutional

environment, and therefore is really more working from the neoinstitutional organizational theory, whereas she focuses on the state in the Polanyian sense and how the state makes the market.

— *Fligstein also talks a lot about the role of the state.*

— Well, he does, but in his analysis you see a difference in emphasis. Krippner is very much in the Polanyian tradition. If you recall Polanyi's work, the argument is that the state created the modern national market economy. And in that argument there's no entrepreneur and there's no firm, just the state. That's true in Krippner's work. There's no firm in the analysis, just the state creating the incentives. There's some lobbying from the financial industry but there's no focus on the financial industry itself. Whereas in my reading of Fligstein, he focuses on the firm and the way to resolve and eliminate predatory competition by creating a stable market organ. So the state is there, but in the background. In Krippner, the state is the focal actor. Congress is the focal actor, and the laws it passed opened the way to the financialization of the American economy. So, it's a difference in intellectual tradition. But the good news is that there is a variety of new institutional analyses that are coming to the fore in the movement from "Embeddedness and Beyond" that are broadly complementary, broadly insisting that economic sociology move beyond just a focus on networks. They practically and theoretically return to the framework you have in micro-level analysis that involves actors and networks, the meso-level that involves the firm, and the macro-level analysis that involves the institutional environment. And this multi causal model was first argued for in "Embeddedness and Beyond". So, that's why I'm pleased that the conference here in Moscow has picked up on that emphasis.

— *In general, what do you think a sociologist should do if he wants to be read, accepted or merely understood by economists? You just said that your book has had an effect on economists. What's the secret?*

— Well, first of all it should be very clear that the book is only published recently and so, what we have are the preliminary responses from some economists. I'm not going to be comfortable with the view that the economists are taking the book seriously until there is a major review appearing in a journal such as *Journal of the Economic Literature* or some other more serious intellectual engagements. I guess I don't have an answer to your question, because, in general, each of the social science disciplines, in the case of sociology and economics, looks inward for ideas and outward for influence. These are two social science disciplines that have imperial ambition and so the tendency, especially in economics, is to look inward to economics for the sources of their ideas and not to pay much attention to what is going on outside of economics. And the same can be said for sociologists, but we're more curious about what the economists are doing than the economists are about what we're doing

— *For example, sociology borrowed much from institutional economics. We are attentive to them, but they are not attentive to us.*

— They're less attentive to us. That's fair. And that's very much in line with how modern economics organizes itself, which is that they are inward-looking. So, in that sense, I think, that the more realistic view is that the inward-looking tendency present in each of the disciplines is the stable equilibrium and that neither discipline will be inclined towards a blurring of their disciplinary boundaries. Therefore, what I'm arguing for is more modest. It's not that we write for the economists. Rather, our work needs to move beyond emphasizing orienting statements, and needs to always have in mind that the findings should be strong enough that someone outside of our discipline will recognize them as significant. That is to say, the findings need to have external validity. That doesn't mean that they'll always be recognized by everybody in the ally discipline, but those who take the time to read should be able to see that we're not just talking to ourselves, that we have written something that has a robust nature. That's generally the idea of all sciences. In the natural sciences, the discovery of DNA is recognized not just by the people who were involved in this effort to discover modern genetics, but

by other biologists who were initially critical of this effort and also by ally disciplines in natural sciences. The discovery of DNA is robust, it's been replicated. It's recognized outside of the subfield of biological science that discovered it, and it's recognized within the larger field of biology by people who were not convinced of the approach that was taken. It's recognized outside by physicists, by chemists, so that a whole field has emerged, for example, biophysics, biochemistry, that attributes its origins to the discovery of DNA. I think, in that sense, modern sociological network analysis has some of these features. It's recognized not just within sociology, but by ally disciplines: economics, political science, and now computer science see that this is important.

— *And institutions?*

— Well, yes. Sociology is the social science that organizes itself around the idea that it studies institutions — comparative institution analysis began in sociology — and now institutions have been picked up by economics, and so it's been included into political science. New institutional economics has penetrated political science, and we should be pleased by that and join the cross-disciplinary study of institutions' role in economic performance. It began in sociology, after all. Why shouldn't we participate in the success and contribute new robust findings that others have to take seriously?

— *Professor Nee, thank you very much!*

To be continued.

Alexander Kurakin
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