

INTERVIEWS

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Sociology Should be Open to Intellectual Trade with Ally Disciplines <2 yp.>

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

Economic Sociology's May issue includes the continuation of our conversation with Prof. Victor Nee held in October 2012. (See the March issue for the first half, Vol. 15. No 2. Pp. 11–21.) The interview was conducted by Alexander Kurakin, a senior lecturer at National Research University Higher School of Economics in Moscow. Here, Professor Nee re-emphasizes the importance of developing middle-range theories in order for economic sociology to progress by obtaining external validity. He cites some examples of this, including his recent research on the rise of capitalism in contemporary China that resulted in the book "Capitalism from Below" [Nee, Oppen 2012], and the ongoing study of the knowledge economy in New York City. Both are empirical, evidence-based studies that rely on common conceptual frameworks that take into account the salience of the relationship between formal and informal institutions.

According to Nee, there is one more important condition contributing to the fruitful development of sociology; that is it should be open to intellectual trade with ally disciplines. In this context, immigration is mentioned as a field of inquiry where successful cooperation among economists, sociologists, demographers and representatives of other disciplines can be found. Within this research perspective, scholars communicate across disciplines, with an emphasis on the importance of revealed facts and regularities. The external validity of internal achievements serves as recognition of their significance for the behavioral sciences in general.

Key words: embeddedness; middle-range theory; networks; institutions; capitalism; China; innovation; institutional change; economic growth.

—*What do you think about the proliferation of the embeddedness argument to institutions and culture? Because initially Granovetter's argument was a structural or network one, but later some scholars began to talk about embeddedness in institutions, embeddedness in culture, embeddedness in politics and so on. I'm thinking, for example of the article by Sharon Zukin and Paul DiMaggio [Zukin, DiMaggio 1990]. They discuss embeddedness in everything. Is this good or bad? Because it seems to me that Granovetter's argument focused on just networks and it is a strong feature of it. When we begin to talk about embeddedness in everything, it means that we just say: "Culture matters, institutions matter, networks matter and that's all". Do you agree?*

—I think this is the right question to ask, because if you take embeddedness to infinity, then the analytic leverage of the idea as it was initially framed, with the focus on networks, is diluted very much, because everything is embedded. Of course it is. In my view, the 1985 article [Granovetter 1985] was important in its focus on the salience of social relationships in guiding and enabling economic action. But that was not a theory, more an orienting statement he made when he wrote that to understand economic action, you need to include in that analysis the ways in which economic decisions, cooperation and trust are organized and guided by social structure. Granovetter made a simple point, an important point, but if we stop there and just say: “Well, now let’s see how it works for culture, let’s see how it works for institutions,” then I think you haven’t made very significant progress. What’s important is to move from the orienting framework to Robert K. Merton’s idea of theory at the middle range, to come up with an explanation for a phenomenon or class of phenomena of interest. One of the earliest examples of a robust middle-range theory in economic sociology is Max Weber’s study of the rise of capitalism in the West, which is regarded today as a seminal contribution to economic sociology. We need more of that — an explanation of a phenomenon, relying on general mechanisms. Not a general theory, as Merton argued —that’s not what we need — but an accumulation of specialty theories that, while they may have a common conceptual framework, focus on explaining a class of phenomena, such as the importance of cultural beliefs in economic behavior, as Weber did. And extending this work was Avner Greif, an economist, who looked into the importance of cultural beliefs in the organization of economy [Greif 1994] and compared Genoese and Maghreb traders in the Mediterranean rim of late medieval Europe [Greif 2006]. Greif’s work was, in a very real sense, an extension of the Weberian interest in how ideas are an important causal element in how people behave in economic organizations. So I agree with your point that embeddedness to infinity is not a productive strategy in itself; it has to lead to a theory of something —an explanation that is confirmed by evidence, which is empirical research.

— *And your recent book [Nee, Opper 2012] is an example of this kind of middle-range theory?*

— Well, that was its aim, and I think it delivers on that aim. The rise of capitalism in China has parallel significance for the West, in that this is the main economy outside of the West, in the East. Think of it — this major economy that has produced sustained economic growth for three and a half decades, that went from being one of the poorest per capita economies in the world to the second largest economy in the world in a relatively short time, that gave rise to rapid accumulation of private wealth and very rapid decline in absolute poverty, in a country that had the highest rate of absolute poverty of any country in the world, before they launched their economic reform. And now China, which is still far from the per capita income of the advanced economies, is an upper-middle income country that, according to *The Economist* magazine, is predicted to be the largest economy in the world by around 2016. The World Bank says that this day will be closer to 2020, so let’s say somewhere between 2016 and 2020. The Chinese economy has experienced a declining economic growth rate recently. Well, that decline is from double digits to about 7.4%, which in the world economy today, still under the influence of the Great Recession of 2008, is an impressive rate of growth. What *Capitalism from Below* sought to do is provide a theory of capitalist economic development and its emergence in China—a theory that is motivated by economic sociology—to back the argument that the mechanisms driving the emergence of economic institutions of capitalism and of capitalist economic development are not economic mechanisms, narrowly defined as the market, but social mechanisms that are, if you would like to use the word, “embedded” in social norms and in networks, in all types of social structures of economic actors. In that sense, the book presents a middle-range theory, that it is a theory of endogenous economic emergence and organizational change. And by endogenous I mean that the change in institutions and in organizational form emerges from the economic actors, and it is first seen there, then exogenously. The change is then fixed with the creation of formal rules that legitimize what has already taken place on the ground through the actions of the entrepreneurs. The economic institutions that are needed for capitalist market development (for

example, the labor market, suppliers network, production market) emerge through institutional innovations from the bottom up. Remember that capitalism, distinct from all prior economic orders, is the first one in human history that makes innovative activity a requirement for survival of the firm. Innovation is a necessity, not just something that happens by chance. So, all these institutional elements are in combination. The institutional structure sustains, enables and motivates entrepreneurial action, in contrast to Schumpeter's view that entrepreneurship is an attribute of remarkable individuals. Of course, he focuses on this individual level, because at the time when Schumpeter wrote his book *Theory of Capitalist Economic Development* [Schumpeter 2004], capitalism was already well established as an economic order, and so it could be assumed that it was in place. So what type of remarkable individuals are those who come from the margins to challenge the established economic order through innovative activity? Who are the ones who cause creative destruction as a dynamic engine of capitalist economic development? Well, when the institutional order is not already established, and cannot be taken for granted, it's in this process of emergence that one can see and confirm that entrepreneurship is not about individual attributes, but evolving institutional structures.

— *Individual innovation versus institutional innovation.*

— Yes, the individual as a great entrepreneur versus the institutional structures. So, why is innovation happening in Silicon Valley? It has been the beating heart of start-up firms, high-tech firms for so many decades, because the institutions are in place there that enable a variety of different types of individuals with a good idea to set up a firm readily. The venture capitalists are there, the research universities are there, the established corporate laboratories are there, like Xerox and Kodak, the established corporations are there and the tacit knowledge is there—all concentrated in Silicon Valley. Of course, some of these individuals are remarkable, but, in fact, it's not so much their individual quality, but that the institutional framework is in place. This is what we're studying in New York—the emergence of institutions that combine into a network of institutions and organizations that has enabled the emergence of a high-tech startup economy in New York City, one that is evolving very, very rapidly now. We are taking the middle-range theory that we discovered in the study of the rise of Chinese capitalism and extending it to New York City, a very different context, a very different institutional environment. We will push hard to gain further clarification and elaboration of the theory of endogenous emergence of economical organization. It is a process that starts with having a conceptual framework—the new institutionalism in economic sociology—and then conducting research and discovering evidence and regularities that confirm the middle-range theory that comes from the conceptual framework that we can then take to different settings: the Yangzi delta region in China (Shanghai, Zhejiang, and Jiangsu) to the New York City metropolitan area. These are totally different regions of the world. It's the same new institutional economic sociology that was first developed by Paul Ingram and myself in *Embeddedness and Beyond* [Nee, Ingram 1998] and then in an article in the second edition of the *Handbook of Economic Sociology* [Nee 2005], a short article in *American Economic Review* [Nee 1998] and then now, in the first major empirical study *Capitalism from Below*. I think we have demonstrated in *Capitalism from Below* the explanatory power of the multi-level causal model that is centered on the importance of a clear specification of the relationship between the informal institutional elements of the norms and networks, and the formal institutions of law and regulation and state action. I think it's a framework that can be used for many different institutional environments, this new institutional economic sociology.

— *But I think that the statement of the interconnection between formal and informal institutions is not the middle-range theory. It resembles more general theory, doesn't it?*

— Here, I think we can go back to Merton, who was a structural functionalist. Talcott Parsons was his thesis adviser and Merton was part of the study group that Parsons led that gave rise to modern institutional sociology. What Merton did was establish at Columbia University an extension of this structural functional paradigm, which he elaborated in his book and articles, and it was within that framework that he criticized Parsons for his emphasis on grand theory.

Merton argued that it was the wrong way to build the discipline, because he knew from his work in the sociology of science that science does not have a general theory, that science made progress in specialty fields, with specific theories he called theory of the middle range, and so he advocated this for sociology. Remember, Merton had a conceptual framework, structural functionalism and his idea of manifest latent functions. From this conceptual framework, he and his students developed middle-range theories that were specialty theories. And so, in new institutional sociology the conceptual framework is the new institutionalism that has three varieties: neoinstitutional organizational theory, neo-Polanyian conceptual framework and the new institutionalism in economic sociology. Towards the end of my article in the second edition of the *Handbook of Economic Sociology* there's a conceptual map with three currents in new institutionalism: neo-institutional organizational analysis, neo-institutional economic sociology and neo-institutional economics. They are distinct enough, they have different emphases. For example, neo-institutional organizational analysis is focusing attention on the relationship between the institutional environment and the firm and on non-rational behavior, because some of the work in new institutional organizational analysis began with interest in the non-profit sector, in education, for Meyer and Rowan [Meyer, Rowan 1978] and in museums and publishing for DiMaggio and Powell [Powell 1985; DiMaggio 1987]. Well, it's understandable that they focus on legitimacy and legitimacy-seeking as the focal point of interest, and the mechanisms that DiMaggio and Powell and Meyer and Rowan identify. Well, in economic sociology we add markets and market dynamics. In markets it's competition and cooperation and bounded rationality and profit-seeking. That's what market players think about. And so, in economic sociology we naturally incorporate a focal interest in the social structure of markets. This is why I do think that Harrison White's seminal work on a sociological theory of market structure is important to the new institutionalism and to economic sociology.

— *Though he's known mainly as a network scholar--.*

— Well, because of his book *From Networks to Markets* [White 2002]... Very few people read that book, because it's difficult to read.

— *I didn't.*

— Harrison has some similarity with Talcott Parsons in that his English often is opaque. I had to read the book very carefully because I was asked to make a comment on it at a conference that was organized when it first came out, in 2005. It was organized by Neil Fligstein and Doug Guthrie at the Rockefeller Foundation's conference center, Bellagio, in northern Italy. And at the conference was Harrison White. He was sitting across the table from me. And, of course, I knew Harrison, because I went to graduate school at Harvard, and he was there at the time. Of course, I knew that I needed to make a comment that reflected that I understood what he was doing, so I did read the book carefully. *From Networks to Markets* is clearly an institutional analysis that locates networks within the institutional environment of the firm, of the market. In my *Handbook of Economic Sociology* article there is a multilevel Venn diagram that shows, I think, how Harrison fits into the new institutional economic sociology. In that book and also at this 2005 conference he was quite critical of Granovetter for sounding like an anthropologist, going from case study to case study without a theory that had predictive power. That's what he criticized.

— *And you after him.*

— Maybe even before him, because *Embeddedness and Beyond* was published in 1998. But we all were thinking this way. The engine in Harrison White's theory of markets isn't an economic engine. He draws from Michael Spence's signaling theory, and he states: "This is the engine that drives the theory of the production market." This is the way in which firms signal to each other and organize the hierarchy or the pecking order in the production market. I think that both White's *American Journal of Sociology* article [White 1981] and his book exemplify what I mean by economic sociology being in an exchange with economics. In both works, White drew very heavily on economics and on economic theory: from Spence's signaling theory, from Rosen's theory of consumption. These were very critical to White's theory of the production

market. White drew heavily on economics, which enables his theory of markets to have predictive power. And, in this sense, he's quite different from Granovetter. I think, it's regrettable, that the seminal article for the revitalization of economic sociology was done through a polemic against Williamson. And in this polemic, Granovetter framed economic sociology *versus* economics, us against them. And if we're right, they're wrong, and if they're right, we're wrong. That's the wrong way to frame the relationship between economic sociology and economics. The right way to do it was how Harrison White did it from the start. I think it is very important to also keep in mind one economic sociologist who, very early on, wrote about the relationship between economics and sociology, and this is George Homans. His argument is that sociologists, in our theory of social relations and exchange, have a general understanding of social exchange, and that economics is a specialized view of this exchange, that economics emphasizes the same mechanisms of social exchange, but within the framework of the market exchange. Well, I think, that's the right way to look at it. Each of the social sciences is so different, they have the appearance of rival tribes. But, as Homans (1967) argued in his book *The Nature of Social Science*, we are part of the same social science of human behavior.

— *I don't think that economists will agree to be a part of something.*

— Well, I think they are beginning to. They are coming around. I think Greif is right. There's been sociological turn in economics. They have become interested in institutions; they have become interested in networks.

— *Did they take back their argument that economic reasoning is the ultimate power?*

— Well, there is economic imperialism of the Becker variety and it is certainly there

— *Especially at the neo-classical core.*

— Yes, but there exists a variety of economic theory and economics and the neo-classical economists also ignore new institutional economics, and they are often very skeptical about game theory and network analysis. If you're focusing on neo-classical economics, I think, you're right, but if you're focusing on the very active interest in social norms and networks in economics and the active interest in institutions in sociology, well, I think, there's opportunity for the situation to evolve, as we now see it in another field that I study — immigration — where economists and sociologists cite each other's work, where they make a contribution to deepening understanding of the process of migration. Obviously, every discipline is inward-looking — sociology as well as economics. It's obvious; it's a part of the epistemology of knowledge. However, at the borders of each discipline you want porous boundaries open to mutually beneficial trade, not intellectual protectionism and hostile border guards. And the whole trend in the sciences now is for interdisciplinary institutes and departments: the fields of bio-physics, bio-chemistry, computer science, information science. Many of the areas in the natural sciences where there is the most rapid progress involve just this type of intellectual trade. In the intersections between disciplines, that's where the most rapid progress has been made. It would be a mistake for sociology not to position itself to make progress by opening up to intellectual trade with our ally disciplines. It doesn't mean that we follow them, it means that we're open to trading ideas, and we see this works very well in the field of immigration and in other fields.

— *Why did that happen so successfully in the field of immigration? In other fields the situation is not as good as you described.*

— Well, that's a good question. In the field of immigration the work being done by economists and sociologists generally is empirically based. Therefore, they use common language for empirically-based events in the field, when people come up with a new finding that is backed not only by theoretical argument but by empirical evidence. This leads to a situation where the finding is clear enough, the methodology is rigorous enough, and the evidence is robust enough that active researchers in both economics and sociology recognize it. Another advantage is that the field of immigration studies often feeds on the same data sets.

— *So, they can check each other.*

— They can check each other for validity. Keep in mind that many of the migration studies by sociologists are done by demographers—and that demography is a technically well-developed subfield of sociology. Demographers have never really argued on a methodological or theoretical basis with economists, because at the level of their work they generally agree on the procedures for analyzing large-scale data sets, and the data sets are shared, so people can watch each other's work.

— *That's true, but after you apply statistical procedures, you have to interpret the data and that is impossible without theory. So, what do they do? Do they borrow economic arguments?*

— No. I'll give two examples. One example is the work of Douglas Massey, who is a social demographer at Princeton. His main work in immigration [Durand, Massey 2002] was a study of migration from Mexico to the United States, and his methodological innovation was to study immigration not just from the point of view of the receiving society, but from the point of view of the sending society. So, he did the study of Mexican emigration together with a Mexican sociologist, studying it from the point of the towns and villages that sent emigrants to the United States and came up with the theory "cumulative causation," where he showed that the cost of immigration is highest for the first immigrants, but once that emigration chain began, the cost of immigration for subsequent arrivals gets lower and lower. Why? They say it's because of the network that provides information and help and resolves uncertainty for the new immigrants. So, the theory of "cumulative causation" that Massey offered and backed up with research is that all people who want to leave, eventually leave. And that becomes a new equilibrium point, because the cost of immigration gets less and less, the more people have left and settled in the U.S. Well, that is a theory, a middle-range theory, backed by evidence. After the work was published, Massey made the data set available to researchers, be they sociologists or economists. But the first reporting of it was very credible, because it was a middle-range theory backed by evidence, that made a discovery that went beyond the push-and-pull theory of economics to explain why emigration becomes self-reinforcing over time, and that because the cost of immigration declines, as emigrants at their origin gain more and more information about the immigration process and as networks are put in place to provide concrete assistance to new arrivals, that encourage more and more to come. Everybody who wants to move, moves. So, that's one example.

— *So, I see why this work could be acceptable to economists. Because he uses terms such as "equilibrium" and "cost."*

— Yes, but I framed their argument that way. And Massey probably frames it that way. Again, Massey is a social demographer and, if you look closer at his tacit assumptions, it's bounded rationality, and that is the common behavioral assumption that now all the social sciences build on.

— *Herbert Simon was an economist, wasn't he?*

— A political scientist. He was an acclaimed political scientist. Not an economist.

— *And yet he received the Nobel prize in economics.*

— Herbert Simon was very much in the spirit of George Homans's conception of the social sciences. In fact, Simon believed you couldn't be a social scientist unless you published in the journals of other disciplines: economics, psychology, political science, and sociology. And he did, but he got his PhD from the political science department at Yale University, as did James March. It's important that his work was recognized as robust, not only by political scientists but economists, psychologists and sociologists. Herbert Simon's understanding of human behavior received wide external validation, but his training was in political science — although you wouldn't have known that from the range of his publications in the leading journals of various other disciplines. He did publish in sociological journals. He published an article formalizing George Homans's exchange theory in *American Sociological Review* [Simon 1952].

— *So, the boundaries can be overcome.*

— Well, it's not the purpose of our work to persuade economists.

— *Not to persuade, but to debate.*

— Right. I think that if the debate is conducted only with words and orienting frameworks and essays, of course the economists will ignore that. We all are inventive with our concepts. We can always talk about what's important, what's missing, what should be done. Rather, the potential for progress lies in how we actually specify a theory of the middle range, which has an implicit model or a formal model, that leads to empirical research and is then backed by the research. It's a hard work to do that, to measure an idea, to take it to the field and to study actual human behavior. That's very challenging, but then, once it's done, people from within the discipline who are skeptical, people from other disciplines who are naturally more skeptical will assess the robustness of the finding. In a case like Massey's study of emigration from Mexico, he's got a theory and he's got evidence, based on a very impressive empirical study. That captures people's attention, not only within sociology, but from other social science disciplines.

— *Why do you think such recognition is so rare? Is it because sociologists' work lacks something?*

— Well, let's say that cross-disciplinary recognition of the type that Herbert Simon received, of the type that Elinor Ostrom (who is also a political scientist) received recently, of the type that...

— *And maybe James Coleman? His double recognition.*

— Yes, Coleman has that. And Kahneman and Tversky. They are psychologists but their work is very widely accepted by economists. Remember they challenged the perfect rationality assumption of the neo-classical paradigm pointing out flaws in this assumption of perfect rationality. And that has renewed interest in behavior economics, which is a very important movement in economics these days. So it is not that our work is unimportant unless it has the external validity, but the most important works that a discipline produces are those that are recognized by skeptics in ally disciplines. We don't read all the works produced by economists, but we read and are influenced by work that we think is robust enough to influence our own work, and we recognize when someone else in another discipline has made an advance that is important to our work. So, we imported from economics Spence's signal theory, we imported from George Herbert Simon bounded rationality, we imported from new institutional economics the ideas of North and Williamson.

— *Their understanding of institutions as the rules of the game?*

— Yes, that idea. That came from North, rules of the game. And economics imported earlier this idea of institutions from sociology, and then recast it in the rules of the game. So it goes back and forth. They imported institutions; we imported back the idea of the rules of the game.

— *Double-borrowing.*

— Yes. They're importing network analysis and extending it to complex large networks and dynamics, and some of that work is being brought back to sociology by people like Michael Macy, my colleague at Cornell, who has a joint appointment in sociology and information science, which is a new department at Cornell, in the school of engineering, and many of the members of that department come from computer science.

— *They have a good mathematical basis. It's easier for them.*

— Yes, to study complex networks. But some of the most interesting work in the humanities, in sciences, in these social sciences, is exactly in this area of the boundaries between disciplines. The people who are really involved, are less concerned about what discipline they're from, and more interested in breakthroughs that are theoretical or empirical.

What sociologists have overly emphasized, in my view, is an emphasis on conceptual development and conceptual framework that is only that. There are endless articles written by sociologists that only stop with developing concepts and talking about other people's concepts and commenting on the history of social thought and saying that this is a theory. But it isn't. In the logic of science, theory is always focused on some active discourse with empirical research, whether it's in physics or in a social science. Regrettably, in sociology, social theory has evolved

as a field of intellectual history merely, because they're only talking about ideas, commenting on ideas and developing, further refinements of ideas.

— *Comments on comments on comments. Endless revision.*

— Yes, and this is why I so much value Robert Merton's view that progress in sociology comes the hard way, through the hard work of discovering middle-range specialty theory that guides research, motivates research and that if the theory is confirmed with evidence (or not), then we're using our time well.

— *OK, the last question. What do you think of the performativity argument: Callon, McKenzie, Knorr-Cetina. I think, it's quite opposite to the embeddedness argument, isn't it?*

—The performativity argument—from the little I have read— basically focuses on the use of economic models by the financial industry to organize markets or to organize new financial instruments. That's their focus: the use of economic models by practitioners to innovate new financial instruments. That's fine with me; it's an extension of the social studies of science to the study of how economic models have been used by the financial industry. I respect McKenzie's work, especially his study of long-term capital funds [McKenzie 2003, 2006]. It's a classic, and it is a very good extension of the work that was done in the social studies of science by Latour and others, who focused not on simply scientific labs and the organization of science, but extended them to the social sciences, to the study of how economic theory in models have a performativity effect in financial services, financial markets. I don't see how it is related to embeddedness.

— *The arguments may mean the following: stop studying embeddedness, because economic models shape economic reality and this is a real topic for sociologists, and that there's no embeddedness. It is not important.*

— Yes, I don't see it. No, it isn't. It doesn't seem to be important in that literature. It's really how economic models as in, say, long-term capital funds, were used to build a hedge fund, a hedge fund industry. I see it as an extension of the social studies of science to the social study of economics. Notice that they are not studying sociology.

— *Maybe not yet.*

—In the future, maybe. Sociological ideas do not organize the market.

—*Thank you very much!*

Alexander Kurakin

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