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What Kind of Re-Imagining Does Economic Sociology Need?

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We are now almost 35 years into the era of modern economic sociology if one takes the publication of Harrison White's 1981 paper entitled "Where do markets come from?" as the origin point. The field has literally gone from nowhere to being one of the central fields in sociology. The field has at least three organizational manifestations: the Society for the Advancement of Socio-Economics (S.A.S.E.), the American Sociological Association Economic Sociology Section, and the Economic Sociology Research Network at the European Sociological Association. The A.S.A. Section has over 800 members and S.A.S.E. now has over 1000. The European Economic Sociology Newsletter, published from the Max Planck Institute in Koln, has over 2100 subscribers. Articles on economic sociology now appear regularly in all of the leading journals of sociology in America and Europe. Mark Granovetter's (1985) paper is the most cited paper in the post-war era. The journal produced by S.A.S.E., called the *Socio-Economic Review*, is the eleventh most cited journal in sociology, putting it in the top 8 percent of such journals. These figures do not include scholars who identify with economic sociology as a field with primary affiliations being the American Management Association, the European Group on Organizational Studies (E.G.O.S.), and the various political science organizations in which political economy and the study of the varieties of capitalism are active topics.

My purpose in this chapter is to connect this dramatic increase in scholars interested in using the term "economic sociology" to identify their intellectual identities to the issues raised by this volume. I want to take up three topics. First, I want to discuss the origins of the field from the perspective of viewing it as a kind of social movement. My main point is that when

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S.A.S.E. was formed, the A.S.A. Section was created, and the European Economic Sociology Newsletter was founded, the scholars who did so all agreed to a broad definition of the field to attract the widest number of scholars who may have had quite disparate theoretical and empirical interests. This openness was a necessity given that the field was wide open and no-one really knew what economic sociology was. It was also a necessity because there was a lot of interest in producing such a field but little or no consensus as to what it would look like. The effect of this openness was to unite under a single identity umbrella a lot of research programs. Many of these programs, such as network analysis, political-cultural analysis, performativity, institutional analysis, political economy, and some forms of cultural analysis are quite mature at this point.

Second, I want to present some recent work done by Dan Wang (2012) where he maps out how scholars teach economic sociology as a set of works. His goal is to try and identify if all of this intellectual activity has resulted in a canon of works that scholars feel compelled to teach to students. The data show that these research programs form a kind of set of canonical works, which are consistently taught. But having said this, many of these research programs do not actively try and take other programs into account in what they do. Indeed, they have kept their purity by being mostly self-referential.

These results mean that economic sociology is quite a mature field of study, one that has a fair number of research programs and a lot of organizational and institutional structure to support the circulation of these ideas. One way of thinking about the research programs is that as new ones emerge, they form groups and occupy space under an overarching organizational and publications structure that can be used to disseminate the results of the research programs to an audience already identified as being interested in economic sociology.

But the purpose of this volume is to re-imagine economic sociology. So, my third topic will be to consider how this existing structure both creates opportunities for new progress and, alternatively, blocks such thinking. Richard Swedberg has argued in this volume that the really useful thing to have come out of economic sociology is a set of concepts, a kind of conceptual toolbox. I have several critiques of his perspective. One is that a toolkit view of concepts misses quite the common themes and connections between topics which would allow us to order our toolkit into a typology. More importantly for the discussion here, I want to argue that even if this toolkit was given more structure, this perspective misunderstands how the camps of economic sociology have created research programs and that the rewards for keeping research programs pure are high. We need to explicitly recognize that the different research programs use different concepts, may be trying to explain quite different things, and are organized to keep out impure thoughts.

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This means that any attempt at re-imagining economic sociology must confront its mature structure both organizationally and also as a set of research programs. Research programs are broad ambitious conceptual frameworks that are allied with a research methodology that can be used easily in a wide variety of contexts. Such programs frequently depend on a particular scholar engaging a set of graduate students and perhaps a set of colleagues. The biggest rewards in social science are now for individual scholars to work to create new research programs. This offers an opportunity for collaborative work oriented toward a narrow set of goals and the most successful of these programs provides us with new knowledge. But it also pushes scholars into trying to make what they are doing sufficiently distinctive, encouraging the creation of jargon and a willful ignorance of similarly placed perspectives.

Although it is useful in the initial phase of creating a new research program to have blinders on, in the long run, it is important for these research programs to eventually begin to confront one another. Once a research program has begun to run out of steam, it loses its interests to even its adherents and ends up disappearing. This means that the opportunity for the accumulation of knowledge is lost. It also misguided given what we know about scientific innovation. Brian Uzzi and his colleagues (Uzzi et al. 2013) show that most innovation in natural science has occurred when scholars work at the interstices between research programs and apply what we already know in one context to new contexts. Although I like many of the chapters in this volume, and I appreciate what scholars are trying to do, an alternative tactic to re-imagine economic sociology would encourage us to put already existing research programs in dialogue, particularly on empirical puzzles that pose difficult problems for a given research program. This would require explicitly acknowledging and borrowing from different research programs and not working to produce such programs as if they existed only unto themselves. This already goes on even in sociology. What I am arguing for is more explicit consideration of other programs in economic sociology. Without such cross-fertilization, the field is likely to never be much more than a set of faddish projects, with each ending up an intellectual curiosity forgotten by almost everyone.

The Organizational Origins of Economic Sociology

The publication of White's (1981) and later Granovetter's (1985) paper are some of the scholarly and intellectual roots of economic sociology. But equally important, scholars have to organize themselves by going to their annual meetings, asking for and organizing sessions, organizing special conferences, and, finally, creating more permanent associations and publication

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outlets. An intellectual movement shares many similarities with a social movement. A successful intellectual movement takes advantage of a political opportunity just as social movements do. Social movements often attract disparate groups with disparate agendas. Those groups will vie to create a collective identity that works to encompass many of them and define who is a member and who is an outsider. Intellectual social movements work to do a similar thing. They create a collective enemy (an “other”) and work to define a collective identity that offers a moral justification for the superiority of the in groups and generates or unites a set of research programs. Such identities might be inherently vague in order to provide an umbrella for the widest possible set of groups. If successful, a social movement will change a political order. It will become routinized and engage in normal politics. Intellectual movements become routinized when they produce permanent works, a canon, a set of agreed-on research programs, and organizational and publication outlets for the dissemination of its perspective.

When we try to organize intellectual fields for non-specialists, we track this out in the way proposed by Patrik Aspers, Nigel Dodd, and Ellinor Anderberg in the Introduction to this volume. We isolate camps of scholars who share a perspective and a similar set of research problems. We discuss their main ideas and results. But this approach, while useful, misses the historical process by which these camps of scholars came to join the fray and the social movement-like processes that help structure the boundaries and identities of actors. We can learn a lot about a field by understanding how these intellectual ideas did not emerge in a vacuum, but instead were the outcome of a set of organizing processes that produced the more durable organizations and publication outlets for the field.

The intellectual political opportunity that produced economic sociology was the collapse of Parsonian theory, and, to some degree, the failure of Marxism in the 1970s. As Aspers, Dodd, and Anderberg note, Parsonian theory gave the economy as an object of study to the economists. The collapse of that theory provided a legitimate opening for a re-entry of sociologists into the realm of markets. Marxism failed to generate a theory of the economy that matched the dynamism of capitalism and the myriad ways that it came to produce wealth, income, rules, and to control social conflict. Scholars still wanted to understand how capitalism worked and how it created differentiated forms and the development of welfare states. For many, Marxism was not an adequate theoretical structure to account for the failure of socialism to emerge and the failure of societies that claimed to be communist from creating just and fair societies.

These twin opportunities brought about a resurgence of interest in approaching market processes. But the question was how to do that intellectually? White’s paper opened up this possibility by asserting that markets did

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not emerge from anonymous actors but from actors watching one another and orienting their actions to each other. This had a large dispelling effect on a generation of scholars who realized they could enter the market realm with sociological tools. But White's perspective was not the only one to emerge. It is here that we see a social movement-like process by which disparate groups with different research agendas emerged to claim the terrain of economic sociology.

First came the problem of defining who the enemy was. That was easy: neoclassical economics. But the opposition to neoclassicism was produced by a set of criticisms, some of which originated in moral theory, some of which came from political economy, and others which had their origin in organizational theory. All of these perspectives shared the view that neoclassicism was wrong because it was wrong empirically, morally defective, and damaging when used to create a set of social policies. The goal of all the builders of organizational structures for economic sociology was to unite these critiques and to produce a broad tent under which scholars from many sub-fields in sociology and from other related disciplines in political science, business studies, law, public policy, and to some degree heterodox economics, could come together.

I believe that this heterogeneity of the critiques of neoclassicism made it imperative to find a collective representation for the newly emerging field that did not try to squash one conception of the field or force everyone in the field to be part of the same research program. This does not mean that there has not been competition between research programs to dominate the field. It is certainly the case that some of the scholars who worked on particular research programs did have such imperialistic goals. But, in spite of that, there has been a proliferation of research programs precisely because the economy is a large potential object of study and there are many possible research sites for many different sociological tools. To their credit, to gather momentum and produce cooperation between groups with disparate research programs, the scholars who helped put in place organizations realized that narrowing the focus of the field would make it more difficult to grow and sustain it and expanding it and making it open-ended would more likely result in a permanent re-orientation.

It is useful to go back to the founding documents of some of these organizations to see what their organizers had in mind. The Society for the Advancement of Socio-Economics was founded in 1989 by Amitai Etzioni. Etzioni wrote a book on the economy called *The Moral Dimension* (Etzioni 1990), in which he argued for a communitarian approach to politics that would emphasize how everyone in a society was a member of that society and thereby should be treated in a dignified fashion. Communitarianism disagreed vehemently with the neoclassical view that a capitalist economy left unfettered was the economy likely to produce the most good for the greatest number.

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I went to several of the early meetings that Etzioni organized and they drew on scholars from a myriad of perspectives and disciplines.

As S.A.S.E. became a permanent organization, it incorporated some of Etzioni's original concerns but it intentionally viewed its goal as operating as an umbrella for fellow travelers opposed to neoclassicism in all of its guises. It is useful to produce a short snippet from the organization's mission statement¹:

As an emerging meta-discipline, socio-economics begins with the assumption that economics is not a self-contained system, but is embedded in society, polity, and culture. Socio-economics regards competition as a subsystem encapsulated within a societal context that contains values, power relations, and social networks. The societal context both enables and constrains competition. Socio-economics assumes that interests are not necessarily or automatically complementary and harmonious, and that societal sources of order are necessary for markets to function efficiently.

Socio-economics further assumes that individual choices are shaped by values, emotions, social bonds, and moral judgments rather than by narrow self-interest. There is no a priori assumption that people act rationally or that they only pursue self-interest or pleasure.

Methodologically, socio-economics regards inductive studies as co-equal in standing with deductive ones. For example, a study of how firms actually behave has the same basic merit as treating the firm as an analytic concept in a mathematical model. Inductive inputs and deductive derivations are assumed to correct and thus balance each other. Socio-economics is both a positive and a normative science. That is, it openly recognizes its policy relevance and seeks to be self-aware of its normative implications rather than maintain the mantle of an exclusively positive science.

S.A.S.E. has little interest in criticizing neoclassical economics per se, and seeks to develop alternative approaches that are predictive, exemplary, and morally sound. Socio-economics does not entail a commitment to any one ideological position, but is open to a range of positions that share a view of treating economic behavior as involving the whole person and all facets of society.

The Economic Sociology Section of the American Sociological Association was founded with a similar view. In February 1998, Brian Uzzi and I organized a meeting in Berkeley to discuss the degree to which economic sociology was a field. One of the goals of our meeting was to let the gathered participants decide what they wanted to discuss and to provide for them venues for small groups to debate issues and bring them to the larger group for discussion. My memory was that many of the people who came to the conference were skeptical that economic sociology was really a field. Many thought it was more of a set of loose research programs, many of which were in opposition

¹ https://sase.org/about-sase/about-sase_fr_41.html.

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or orthogonal to one another. But everyone left feeling that we had in fact found some common ground. The most significant thing to come out of that meeting was the move to form an economic sociology section. Wayne Baker, professor at the University of Michigan, more or less single handedly championed this agenda.²

It is useful to quote from the mission statement of the section on its formation:

The mission of the Section on Economic Sociology is to promote the sociological study of the production, distribution, exchange, and consumption of scarce goods and services. It does so by facilitating the exchange of ideas, information, and resources among economic sociologists, by stimulating research on matters of both theoretical and policy interest, by assisting the education of undergraduate and graduate students, and by communicating research findings to policy makers and other external audiences. Economic sociology is a distinct subfield. It is ecumenical with respect to method and theory. Economic sociologists use the full range of qualitative and quantitative methods. No theoretical approach dominates; the field is inclusive, eclectic, and pluralistic.³

As is obvious, the economic sociology section was founded with the same broad premise as S.A.S.E. Instead of creating an organization that would limit its members in terms of what they could study and how they could study it, the section was meant to be open to anyone and respect all approaches.

Finally, Richard Swedberg, Jens Beckert, Johan Heilbron, and Ton Korver decided to create the newsletter at the annual meeting of the European Sociological Association in Amsterdam in August 1999.⁴ It is useful to consider Richard Swedberg's argument in explaining the purpose of the newsletter in its first issue:

While in the United States economic sociology has been moving ahead at a brisk pace at least since the mid-1980s, this is much less the case in Europe. It is true that during the last ten-fifteen years some of the best-known sociologists in Europe—such as Pierre Bourdieu, Niklas Luhmann, Claus Offe and Anthony Giddens—have all discussed economic topics in their works and thereby made contributions to economic sociology. But they have usually done so in their capacity of general sociologists, and they have not made a call for, or otherwise encouraged, the development of a distinct economic sociology. Today, however, the situation is somewhat different in several European countries; and since a few years people have started to appear who identify themselves as economic sociologists and also have something novel to say. It is my belief that there exist more of these interesting, new practitioners of economic sociology than is currently known and also that most economic

² For Wayne Baker's account of this, see <http://www.asanet.org/Sectionecon/documents/FormationofEconSection.pdf>.

³ <http://www.asanet.org/Sectionecon/documents/FormationofEconSection.pdf>.

⁴ See the first issue and its justification at <http://econsoc.mpifg.de/archive/esoct99.pdf>.

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sociologists in Europe do not know about each other's existence. What is happening in Italy is rarely known to sociologists in Germany or Sweden and vice versa. Hopefully this newsletter will change this situation a bit—and in other ways be of help in advancing the cause of economic sociology in Europe. Economic sociology is often defined as the application of the sociological perspective to economic phenomena as well as to phenomena which are economically relevant and economically conditioned (Max Weber). This is a very broad definition, and practically all varieties of economic sociology can be made to fit under it. It is also my hope that this newsletter will encourage all of the different types of economic sociology that already exist as well as those which are about to surface.

Richard Swedberg and his colleagues decided that for economic sociology to flourish in Europe, scholars should define themselves into the group and in doing so, join the ongoing conversation. Indeed, the European Economic Sociology Newsletter functioned as much to create economic sociology as a field in Europe as it did to disseminate it. It did so not by defining what was and was not economic sociology, but by letting scholars work to find their place in an emerging discursive space.

I conclude that the founding of economic sociology as a field brought together disparate scholars with varying research programs that were interested in pursuing economic issues and opposing the hegemony of economics as a discipline over our understanding of those issues. The point of building organizations was to create an umbrella broad enough to attract lots of people and at the same time, one that would not force conformity to either a theory or a method. This reflected very much the social movement-like character of economic sociology at its origin. Scholars were struggling to find ways to unpack how capitalist societies worked. They were also looking for fellow scholars with whom to communicate. Being against neoclassicism and vaguely for trying to embed the economy in society, was enough of a collective identity to bring people together.

The Intellectual Structure of Economic Sociology

Research programs that were evolving in the 1980s and 1990s formed the core of these organizations, their publications, and meetings. This intellectual ferment has created a canon of works in economic sociology. In a recent issue of the newsletter for the economic sociology section, Dan Wang examined a set of syllabi for classes that were currently being taught at major research universities.⁵ The data were collected by asking the membership of

⁵ See <http://www2.asanet.org/sectionecon/accounts12sp.pdf>. All discussion in this section of the chapter is taken from that piece.

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Table 12.1. Twenty most canonical authors by number of syllabus appearances

Rank	Citation code	Number of syllabus appearances	Number of unique references in syllabi
1	granovetter, m	85	12
2	zelizer, v	72	23
3	fligstein, n	60	12
4	polanyi, k	51	9
5	swedberg, r	47	13
6	bourdieu, p	46	15
7	uzzi, b	41	6
8	dimaggio, p	35	10
9	carruthers, b	33	13
10	mackenzie, d	32	7
11	weber, m	29	6
12	stark, d	26	8
13	powell, w	25	6
14	burt, r	24	10
15	callon, m	23	7
16	dobbin, f	23	7
17	biggart, n	22	8
18	davis, g	22	10
19	krippner, g	22	7
20	Fourcade, m	21	6

See Wang (2012) for more information about the papers.

the section to submit their syllabi for classes they taught. There were 55 syllabi submitted and these came from individuals teaching in the U.S. and Europe and included scholars teaching not just in sociology but also business schools, public policy programs, and political science. Most of the syllabi (45) came from American universities and therefore there is a bias towards the way that economic sociology tends to get taught in American universities.

Table 12.1 contains a list of the 20 most cited authors on the syllabi. I note that if authors had multiple works cited, they were counted multiple times. Hence, three of the authors (Granovetter, Zelizer, and Fligstein) had more than 55 appearances. Not surprisingly Mark Granovetter leads the list. The list represents the field in many ways and maps onto the discussion by Aspers, Dodd, and Anderberg in this volume. But the list does not tell us much about how these readings form identifiable research programs, programs where the works share a common intellectual perspective.

Figure 12.1 presents a network analysis of the articles cited in the syllabus. Figure 12.1 explicitly examines how scholars who teach economic sociology put together reading lists that map out debates in the field. The analysis is based on the appearance of readings in a given week on each syllabus. Hence, works that appear together on the syllabus in a given week are counted as having a link to each other. The idea is that when scholars teach the field, they put together lists where a set of readings are thought to speak to each other.

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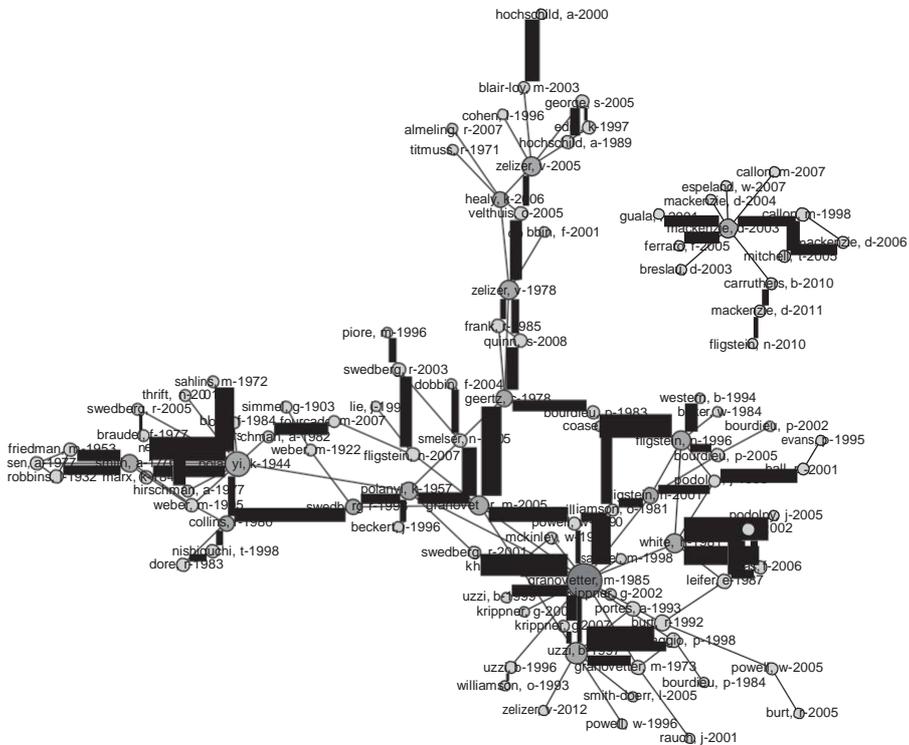


Figure 12.1. Network diagram of works that were linked within a single week’s assignment in syllabi. See Wang (2012) for a description of the graph

One can see four and perhaps up to six distinct clusters here.⁶ Three of the clusters are relatively well defined and appear to reflect bodies of work that are independent of one another. On the far left of the diagram are works centered on Karl Polanyi and, not surprisingly, they tend to be oriented towards political economy. At the top in the center are a set of works that feature Zelizer and others associated with culture and issues of morality in economic sociology. These works are linked back to the large group through Clifford Geertz. This represents one version of the cultural turn in economic sociology. The third distinct group is work in the social studies of science tradition that focus mostly on the sociology of finance but also include recent work on commensuration.

⁶ Wang’s analysis of this figure concludes ““Altogether, the analysis suggests that this rather small group of articles that forms the canon in economic sociology is largely rooted in two distinct traditions—a relational-network perspective and a cultural/political approach to understanding the social basis of economic activity.” (2012: 4).

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The large main group at the bottom of the graph is connected through Granovetter's work. I would argue that this group reflects the various ways that scholars have used multiple notions of embeddedness to study economic life. Although there is a strong sub-group of scholars easily identified as working in the network tradition (Uzzi, Burt, Powell, White, Podolny, and Leifer), that group does not dominate this block. Indeed, many of the authors listed have contributed to the debate over different forms of embeddedness (such as Borudieu, Krippner, Swedberg, Fligstein, and even non-sociologists like Oliver Williamson), and much of that conversation appears to be captured in the graph. My interpretation of this graph is that it captures key features of how scholars divide up the canon when they teach economic sociology. There is clearly a political economy, cultural, network, and science studies flavor to what people teach. But there is also a dense set of readings that takes up broadly what economic sociology is and how embeddedness is used by different scholars to study the economy in different ways.

Taken together, I would argue that the field is mature in several ways. First, there now exists a set of organizations, meetings, and publication venues to which scholars who want to be identified as economic sociologists can turn to find audiences and discussion for their work. Second, that field has coalesced around a set of works that has produced a canon that picks up the various themes that define the distinct contributions of the sociological approach to the economy. At this historical moment, if one is going to re-imagine the field of economic sociology, it is within these common understandings of what that field is and the organizations and institutions that define it.

What Barriers and Opportunities Are There to Re-imagining Economic Sociology?

One of the problems of looking at what scholars are teaching is that there may be close to fifteen or twenty current research programs, many of which are only loosely captured in what people are teaching. So, for example, Aspers, Dodd, and Anderberg identify a "cultural turn" in economic sociology. But, there might be a half dozen such research programs ongoing that invoke culture in some way in their approach (many of which are represented in this volume). So, I consider Bruce Carruthers, Frank Dobbin, Laurent Thévenot, Nina Bandelj, Nigel Dodd, and Patrik Aspers' papers all to have some interest in culture. A cursory read will cause the interested reader to scratch their head at what they have in common. Existing research programs that focus on the performativity of economics, identities, the role of law in defining markets, processes of commensuration, the role of conventions in markets, the role of meaning in market action, and the moral categories defined by

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actors in their interactions with markets, all seem to emphasize very different conceptions of culture.

Richard Swedberg's suggestion in this volume (Chapter 1) is that we not worry about this proliferation of research programs but instead concentrate on the bits of theory that have come from classical and modern versions of economic sociology that offer us guidance for research practice. The appendix to his chapter gives a set of examples of terms he considers to be useful in such an endeavor. Reading the list is jarring. On the one hand, he offers general terms like "class" and "money," on the other, extremely specific terms like the "motherhood penalty" and the "Zelizer circuit." I know he does not intend this list to be exhaustive in any way. But I believe it sets up two kinds of problems for students who might read it and try to use it in their work. First, such a list implies that these terms bear little or no relationship to each other. This is extremely misleading. A more useful kind of list would provide a typology not unlike say, the Journal of Economic Literature's classification of topics. So, for example, the term "making out" was coined by Michael Burawoy to describe why some blue collar workers came to buy into and support the system that exploited them on the shop floor. If we had a typology of more general and more specific terms, this idea would usefully be located under the broader topic of the working class and class consciousness, which itself might be located under the theory of class more generally, which is then subsumed under Marx's theory of capitalism. Such a set of concepts would not be a toolbox but more like a Linneasian system of classification drawing links between terms that share common problems and are embedded in larger conceptions.

But even more important from the perspective of this chapter, is that such an approach ignores that many of these research terms occupy distinct roles in particular research programs. Indeed, the field is quite divided with regard to what scholars are actually doing. Scholars who are interested in the dynamics of global capitalism from a Marxian perspective do not find themselves in dialogue with scholars who take their inspiration from Bruno Latour's science studies approach (as illustrated in Karin Knorr Cetina's contribution to this volume, Chapter 4). One might argue that this is not important. Research programs serve the useful purpose of bringing together a set of scholars who are thinking about a particular empirical problem from a particular point of view.

But even if one grants that this is true, they can have a long-run negative impact on the development of a field. Both Max Weber and Thomas Kuhn recognized that sociology as a discipline might be doomed to never cumulate knowledge. Instead, sociology would proceed as a set of research projects which reflected the current concerns and interests of a small set of scholars. When the group hit a dead end in producing novel results, the research program would die out only to be replaced by another one.

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But it is here that we could, if we choose, take a lesson from our colleagues in the natural sciences. In a recent paper in the journal *Science*, Brian Uzzi and his colleagues (2013) analyzed over 16 million scientific papers in physics, chemistry, and biology, published since the Second World War. Their goal was to try and figure out how new and innovative approaches could be generated from existing work. They discovered something quite fascinating. Work that borrowed ideas from nearby existing research and applied it in a new setting turned out to produce the most fruitful innovations. The lesson to be learned is that one should not shut oneself off from work that is deemed irrelevant to what one is doing, but instead, one should be on the lookout for solutions to one's research problems in already existing work in other fields.

In our attempt to re-imagine economic sociology, one lesson to be learned is for us to break down the barriers between these research groups (and indeed between scholars working on quite different topics on sociology) in our search for useful ways to think about new things. Thus the idea is not to work on the edge of some existing research program with the goal of expanding it. But instead, one should be on the lookout for new ideas to borrow to make sense of what should be done next. I believe that something like this is going on all of the time, even in sociology. Granovetter's intentional re-imagining of Polanyi's use of embeddedness worked to create the collective identity of the field.

One big source of opportunities is empirical puzzles that one research program uncovers but is unable to explain. So, for example, the sociology of finance has alerted us to the importance of financial instruments, market devices, and commensuration in organizing these markets. But this perspective has produced a functionalist account of these markets whereby these cultural and organizational features structure market interactions. Most of these accounts stress how individual actors or firms are not the real important actors in these markets (Knorr Cetina, 2005).

While the idea has proved useful in understanding how some of these markets work, the past five years have shown that the main players in most of the largest financial markets have colluded and committed fraud in the largest international financial markets. This notion, that a small set of firms engaged in behavior could have fixed the L.I.B.O.R. (London Inter Bank Offered Rate) or colluded to control prices in the global foreign exchange markets, implies that other kinds of social process matter for the structuring of activities in financial markets. This implies that a good explanation needs to include work from other research programs. This intuitively leads back to Harrison White's original idea that market participants watch each other, and my own view that market actors will engage in any kind of activities that they can get away with to produce stability for themselves and guarantee profits. The interesting question this raises is when do market devices work to

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produce stable markets and when can they be exploited for gain by a rent-seeking actor? A good theoretical answer to this question is likely to advance the field by combining insights from multiple research programs.⁷

Conclusion

I end on a whimsical note. Every year, odds makers around the world rank the possible candidates to win Nobel prizes in Economics. Mark Granovetter almost always appears on that list. It would be spectacular for our field if he won. We would all of sudden move from a troubled discipline (sociology), to a legitimate approach to understanding how the economy and society are connected. But when a journalist called you up to comment on the prize and the research that we all do to make sense of Granovetter's original insight, what would you say? I, for one, would emphasize a broad view of embeddedness, one that has led us to form alternative ways to think about how markets work that includes networks, market devices, meaning, and values, and also discusses how governments and markets have co-evolved. I would suggest to the reporter that Granovetter's contribution has pushed not just us, but also the economics profession to reconsider the role of social relationships in market structures. Hopefully, I (and you) could be coherent enough that the reporter got the point and kept calling. Hopefully.

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⁷ I note that Donald MacKenzie's account of the failure of Long Term Capital Management, a large hedge fund, ends up explaining that failure not on the technical complexity of the positions the firm took, but instead on the fact that the firm's principal competitors knew what it was doing and bet against it (MacKenzie 2003).

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