

Social Institutions, Organizations, and Relations

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Helmut K. Anheier: Dear Professor Powell, you are one the most prominent sociologists in the United States today and very influentially globally in organizational studies. There are indeed few social science students in the world that have not come across your work on either institutionalism or organizations. Your work straddles different fields: innovation and technology, organizational studies, management, and nonprofit organizations among them. You are also someone who has worked at both the conceptual and empirical level. Two sociologists seem to have had a significant influence on your work: Lewis Coser and Charles Perrow. How did they shape your thinking as a sociologist and how would you describe their influence in the social sciences today?

Woody Powell: I was very fortunate as a graduate student in the 1970s to be at Stony Brook. This was a halcyon time, as the university then aspired to be the "Berkeley of the East." There were senior faculty such as Rose and Lew Coser,¹ Kurt and Gladys Engel Lang,² Charles (Chick) Perrow,³ Gerald (Gerry) Suttles,⁴ and Gene Weinstein. There was an

¹ **Rose Laub Coser** was a German-American sociologist who was born in Berlin in 1916. From a family of prominent socialists (she was named after Rosa Luxemburg, who was a family friend), her parents fled Germany in 1924, moving to New York in 1939. She earned her PhD at Columbia, where she worked with Peter Blau. She married Lewis Coser in 1942 and later taught at Harvard and Stony Brook. Her work focused on medical sociology, role theory, and sociology of the family.

Lewis Coser was born in Berlin in 1913 as Ludwig Cohen. He moved to Paris in 1939, escaping to New York in 1941, marrying Rose Laub shortly thereafter. He received his PhD from Columbia in 1955, working at Brandeis University and then Stony Brook until his retirement. Coser's work combined together structural functionalism and conflict theory, including determining the functions of social conflict.

² **Kurt Lang** was born in Berlin in 1924, fleeing the Nazis to come to New York in 1936. He served with the US Army in World War Two and completed his PhD from the University of Chicago in 1953. He taught at Columbia University, University of California at Berkeley, Stony Brook University, and the University of Washington

Gladys Engel Lang was born in Atlantic City in 1919. The daughter of recent immigrants from Eastern Europe, she was the first in her family to attend college. She worked for the US Office of Strategic Services from 1943-49, then completed her PhD at the University of Chicago in 1954. She and Kurt Lang wrote an award-winning essay on MacArthur Day in Chicago in 1953 and their subsequent work covered collective behavior, communication and the arts.

³ **Charles Perrow** was born in Tacoma, Washington in 1925 and received his PhD from the University of California, Berkeley in 1960. He focused primarily on the impact of large institutions on society, including several well-known books on disasters. He argued that in highly complex, interdependent systems, failures are hard to both predict and largely inevitable.

⁴ **Gerry Suttles** was an urban sociologist, born in western North Carolina in 1932. He completed his PhD from the University of Illinois and researched social problems in communities on Chicago's West Side. He won the American Sociological Association's Lifetime Achievement Award in 1993.

amazing group of junior faculty, including Michael Schwartz, Mark Granovetter, John Logan, Kirsten Grønbjerg, and James Rule. And my grad school colleagues were terrific; people like Craig Jenkins, Doug McAadam, Aldon Morris, Linda Stearns and Mark Mizruchi went on to have huge influence. It was an incredible place, but even beyond that, I somehow managed to have two senior mentors—Coser and Perrow—who essentially adopted me and my wife Marianne as family. We saw them regularly, socially for dinner, and independently, not together. We saw them at least monthly, but more often once a week. They invited us to their homes in the summertime. It was an incredibly close relationship. Chick passed away this past fall and I still think of him often.

They were different, they were both men on the left, deeply interested in politics. But Lew came out of a European intellectual tradition, was an émigré intellectual, and became part of the New York Democratic Socialist tradition. He was one of the founders of *Dissent*. He was a historical, or what might be called a qualitative, sociologist today. His best book was *Men of Ideas*, a sociological history of intellectual life. He also wrote a prescient little book, *Greedy Institutions*, about how organizations consume the personal lives of their members and participants.

Chick was much more of an empiricist. He was a working-class kid who went to Black Mountain College in North Carolina and hung out with painters and writers. He went back West, and ended up at Berkeley Sociology during a remarkable time. He moved through various universities before Stony Brook.

I learned organizations from Perrow and sociological theory from Coser. Max Weber was probably the intersection between the two. He was the one person they both worshipped—and to a certain extent Karl Marx. I would say Lew Coser is less well-known today, even though he was much more famous than Perrow at the time. Perrow's work has certainly had lasting influences, particularly because of a book, *Normal Accidents*, (1984) that he did

after the Three Mile Island nuclear meltdown. Chick went on to be one of the world's leading experts on accidents. Every time there was a major accident or catastrophe in the world, people immediately contacted him. Were he with us today, he would be consulted about the pandemic constantly. Chick and I had a special relationship in which we felt capable of arguing with each other and disagreeing while staying very close and friendly.

Just as an illustration, Lew Coser and Charles Kadushin⁵ and I did a book on the book industry—*The Culture and Commerce of Publishing* (1982)—that was about the transformation of the book industry in the United States from a gentleman's trade to a Wall Street-driven industry. Chick [Perrow] was always pretty critical of that book, arguing that we didn't pay enough attention to what was lost in that transformation, who had been excluded from the gentleman's trade side and what topics were slighted by the finance era of Wall Street domination. So, he pushed hard, saying there should have been a final comparative chapter that looked at the two eras. He said you have written the Alfred Chandler book on the book industry (referring of course to Alfred Chandler's great 1962 history of American corporate enterprise, *Strategy and Structure: Chapters in the History of the Industrial Enterprise*), but what was missing was: "what kinds of books are no longer being published?"

When Chick came out with *Normal Accidents*, I was a young Assistant Professor. As I said, we got along very well. So, I was very critical of *Normal Accidents* for some of the same reasons. Perrow made a huge contribution to trying to get people to think about accidents as systemic and structural errors, rather than human errors. But I pushed him about what it was about those structures that reflected capitalist enterprise or state-owned enterprise that led to so many accidents. With Chick, I had a decades-long relationship of arguing excitedly

⁵ **Charles Kadushin** is a sociologist and one of the founders of the social network field. His current his work at Brandeis' Cohen Center for Modern Jewish Studies includes surveys of Jewish populations and evaluation studies including studies of birthright in Israel.

back-and-forth, and I loved every minute of it. Over good bourbon or burgundy we would spend all evening arguing, and my son grew up watching us debate. Chick was like a grandfather to him, his “real” grandfather, as he never met his biological ones.

In Coser’s case, I think he is best known for his work in the sociology of conflict, and I would say the biggest thing I took from him that was this idea that cross-cutting relations knit disparate groups together. Of course, he wasn’t alone in this—Ralf Dahrendorf discussed it in his book, *Class and Class Conflict in Industrial Society* (1959). When [social relations] cut across ranks or units in organizations, across class, gender or ethnicity in society, organizations and societies are better able to solve problems. Lew was fantastic at helping me see that. I’ve carried that insight ever since.

HKA: Now, I’d like to ask you about different aspects of your work. In each case, I’d like you to look back and reflect on the intellectual context at the time you worked on it, to recount the reception it initially had, the impact it generated, and most importantly how both the reception and impact changed your own thinking on the topic. Perhaps we can look at the paper you wrote with Paul DiMaggio⁶ in 1983, “The Iron Cage Revisited”.

WP: It is funny, that paper has been much discussed and widely cited, of course, but no one remembers the neo-Marxist opening of the paper, this whole question about what is the origin of social control? Paul and I were young faculty at Yale. We both had appointments at the School of Management. It was a brand-new school and neither of us had seen a professional setting like it before. So, we saw first-hand the role of management training and teaching in socializing and homogenizing young minds.

⁶ **Paul DiMaggio** is a sociologist at New York University, where he moved after previously teaching at Yale and Princeton. Born in 1951, he earned his PhD from Harvard in 1979. He is well-known for his work on institutions and organizations, and for work on culture and cognition and how “high culture” is formed in the United States. His more recent work explores online communities. Throughout his research, DiMaggio argues that social functions and roles are primarily determined by structural forces.

We watched the top consulting firms come to campus to recruit. I spent time meeting with the people from Bain, McKinsey and the many other ones that visited, because I saw them as the fashion salons of “high” corporate practice, or “haute structure”, you might say. At the time, Paul and I had similar projects underway. He was studying the New York museum world and I was studying the New York world of public television and documentary film. Both were non-profit settings, both faced with issues around declining funding, and both had elite consulting firms doing pro-bono work at their head offices, telling them what they should look like. The empirical work we were doing was really the spur for the paper.

Intellectually, there were other people writing about organizations at the time, certainly out here at Stanford, like John Meyer and Richard Scott, and there was Jim March’s and Johan Olsen’s work.⁷ There were historical institutionalists like Theda Skocpol—Paul took courses from Theda.⁸ We were reading work by people like Pierre Bourdieu.⁹ We both talked with Harrison White¹⁰ about his work on structural equivalence. So, I think what that ASR paper managed to do was to connect ideas about relational equivalence with ideas about normative homogeneity. Turning that into a set of mechanisms or processes—coercive,

⁷ **John W. Meyer** is a Professor Emeritus of Sociology at Stanford University. His work focuses on institutional theory, the sociology of education and world society, most notably the impact of global society on national states and societies and the influence of globalization on organizational structures.

W. Richard Scott is a Professor Emeritus of Sociology at Stanford University. He is an organizational sociologist who studies professional organizations, including educational, engineering, medical, research, social welfare, and nonprofit advocacy organizations. His research focuses on how organizations and their institutional environments related to each other.

James G. March was a polymath who taught at Stanford University for many decades. His writings in organizational theory with Herbert Simon and Richard Cyert became the foundations for the field. His research challenged many canonical ideas about firm behavior, including that companies always maximise profit and that they have complete knowledge. He is also well-known for his work (with Johan Olsen and Michael D. Cohen) on the “Garbage Can Model,” a processual perspective on organizational decisionmaking.

Johan P. Olsen is a Norwegian political scientist who taught at the University of Bergen. In addition to his political science research on Europe and the nation-state, Olsen also worked extensively in the field of sociology, frequently partnering with James March in writings on organizational institutionalism.

⁸ **Theda Skocpol** is a sociologist and political scientist at Harvard. She completed her PhD at Harvard in 1975 and is well-known for her “state autonomy theory” and work on social revolutions and civic engagement. In 1993, she received the American Political Science Association’s award for best political science book for *Protecting Soldiers and Mothers* (1992)

⁹ **Pierre Bourdieu** was a French sociologist, anthropologist, and philosopher. His work focuses on how powerful groups and social classes maintain power across generations, in spite of the egalitarian promises of social mobility in post-industrial societies. His book *Distinction: A Social Critique of the Judgment of Taste* was ranked the 6th most important 20th century work of sociology by the International Sociological Association.

¹⁰ **Harrison White** is emeritus Giddings Professor of Sociology at Columbia University. He helped pioneer the “Harvard Revolution” in sociology, using mathematical models to map social networks and relations. He received the W.E.B. DuBois Career of Distinguished Scholarship Award from the American Sociological Association.

memetic, normative—proved to be powerful. It was a theoretical paper, but it turned out to inform a great deal of empirical research.

I think there is value in observing something, giving it a name, and seeing how it operates across different settings. This provides the insight for people to see the process in many different settings. Many people over the years have had “aha!” moments when they see two different things going on in different settings, but can abstract one level and can see their common features. I still vividly remember my Yale social psychology colleague, Richard Hackman,¹¹ running into my office, jumping up and down (and he was a very tall man), yelling, “I saw it. I saw it!” What? I said. “Mimetic isomorphism. We are hiring an empirical accountant from Stanford. I asked why and what would he teach? The Dean said it doesn’t matter. We need one because Chicago just hired one.” I don’t think Paul and I had a profound analysis of what caused the phenomenon of isomorphism, but we did a nice job of specifying conditions under which it would most likely operate. I’ve been asked over the years about the paper’s impact, and I always jokingly say that if we had known so many people would read it, we would have written a better paper. We did not imagine it would have the influence that it had across so many disciplines.

HKA: Another paper that had a significant influence followed soon thereafter, in 1990. You wrote “Neither Market nor Hierarchy: Network Forms of Organizations.” It won the American Sociological Association’s Max Weber Prize.

WP: That paper had a somewhat more straightforward development. I was very interested in alternative organizational forms. There was a workshop at Yale that Dick Nelson, Sid Winter, Ed Lindblom, Chick Perrow and others organized on alternative organizational forms. (Of course, us junior faculty—Paul DiMaggio, economist Greg Dow, sociologist Carl Milofsky and I did all the organizing work, but it was thrilling for us to have a weekly lunch seminar with such eminent scholars from so many disciplines.) You may have well attended

¹¹ **J. Richard Hackman** was the Edgar Pierce Professor of Social and Organizational Psychology at Harvard University, where he moved after teaching at Yale for 20 years. He made major contributions to social and organizational psychology, notably team performance, leadership effectiveness, and how self-organizing groups function.

that workshop! Most of the organizations community at that time was influenced by Olly Williamson's *Markets and Hierarchies* (1975). But I increasingly thought that the world of organizations looked more like a football or a rugby ball than a ruler.

Williamson saw markets and hierarchies as arrayed on a continuum, with most of the action at the poles. There were some unusual organizations that had bits of markets and bits of hierarchies. But these alternatives were viewed by transaction cost scholars as some kind of mongrel. I said, what if these organizations had a governance structure that was unique in their own right? There was a wide array of work going on at the time in different settings, such as Mike Piore and Chuck Sabel at MIT on the "Third Italy." There were lots of people studying the re-emergence of regional economies, all kinds of things were happening with hierarchies vertically disaggregating, and alliances between ostensible competitors developing. I think that the paper had amazing timing. I presented it to many diverse audiences, and it had a lot of purchase as an alternative to the simple market/hierarchy dichotomy.

I remember giving a talk at MIT with a draft of the paper, and Mike Piore said "you know there are these funny little organizations in Kendall Square called software companies, one of them is called Lotus. Want to meet Mitch Kapor?" So, I went off and met him. There were these biotechnology firms like Biogen started by very eminent scientists like Phil Sharp, so I thought I would go study them. And I also chose biotech because my wife, Marianne, had gotten her PhD in immunology at Yale, and the new molecular biology gave us a fascinating topic to talk about. The work on biotech eventually led me to the empirical instantiation of the network paper, which was our 1996 paper in *Administrative Science Quarterly* called "Interorganizational Collaboration and the Locus of Innovation: Networks of Learning in Biotechnology". That paper has had a wonderful life too, in that it helped set the groundwork for understanding all sorts of inter-organizational collaborations and different forms of affiliation that have a different governance logic. I recall a conference at the University of Minnesota in the late 1990s where they paired Williamson and me in a kind of

“showdown.” Olly said TCE applied to the important, big industries like autos and steel, and networks to little boutique industries like biotech, nanotech, software. The economist Bengt Holmstrom quipped, “Be careful, Olly. You are giving away the 21st century to sociology.” I smiled. But, of course, neither Olly nor I imagined how quickly old corporate hierarchies would downsize and dissolve. And I did not anticipate that large pharma companies would adopt the organizing models of biotech. Holmstrom and John Roberts (1998) picked up this relational contracting insight in a paper that introduced my network writings to a wider audience of economists.

HKA: At the same time that you worked on “Neither Market nor Hierarchy”, you edited a volume with Paul DiMaggio on the new institutionalism and organizational analysis, which is still required reading for anyone interested in the sociology of organizations.

WP: Well, the “orange book” (*The New Institutionalism in Organizational Analysis*, 1991) has had a fun life. I had the pleasure of being for a fellow at the Center for Advanced Study here at Stanford in 1987-88 and I worked on “Neither Market nor Hierarchy,” then, but I also put together a conference with Paul on the new institutionalism. Being out here at Stanford, I was in regular contact with John Meyer and Dick Scott, and Jim March in particular. We got the idea of having a collective gathering of people who were part of this big tent of institutionalism along with people who were on the edges.

After the workshop, Paul and I got that idea that maybe we could write an introduction that would characterize this wide array of work in the social sciences that uses the label “institutional.” Our argument was that although institutions are human built, they are not necessarily the outcomes of human design. The introduction tried to build a pragmatist theory of action. Lo and behold, the book has sold more than 30,000 copies, which is atypical for academic books of its kind. That book, too, has made its way into many, many different disciplines. Of course, with the work in institutional analysis, precisely because of

its big tent, there has been an enormous diversity of scholars who have been influenced by it.

There have also been hundreds, if not thousands, of diffusion studies of how things spread and become “institutionalized.” But unfortunately, diffusion studies treat adoption as kind of a yes/no choice, like a light switch: you either have it or not. It doesn’t say much about how things are adopted or what the consequences are. So, when I moved from the University of Arizona to Stanford in the late 1990s, one of the things I wanted to study was the commercialization of science and the marketization of the university. Stanford was “ground zero” for those developments.

I built a lab here that had some amazing students and postdocs—James Evans, Kjersten Whittington, Stine Grodal, Andrew Nelson, Kelly Packalen, Jason Owen-Smith. One person in particular I worked with was Jeannette Colyvas.¹² We wrote this paper on “Roads to Institutionalization: The Remaking of Boundaries between Public and Private Science” (2006). We did this through a data set that we built on the early adoption of technology transfer. She got department-level and scientist-level data on who were the first scientists to begin to patent their work, and how the university reacted to the commercialization of basic science. Early on, scientists’ involvement in industry was seen as deviant. There were lots of rules limiting it. Over time, it became more clarified and no longer needed so much justification. It spread, mostly among the most high-status faculty since they were somewhat exempt from criticism.

Eventually, becoming a university entrepreneur stopped being seen as deviant and became something celebrated. So, we mapped the process of the legitimation of that behavior, showing how a new category of actor – the scientist-entrepreneur - developed. Then, this whole set of activities became embraced by many universities. Stanford was a pioneer in this process, and provided a template that was widely emulated. This was a nice illustration

¹² **Jeanette Colyvas** is an Associate Professor of Human Development and Social Policy at Northwestern University. She completed her PhD from Stanford in 2007. Her work centers on the organization and design of environments that are conducive to innovation.

of how to conduct a three-decade historical study of processes of legitimation and “taken-for-granted-ness.” It reflects a larger effort that I’ve pursued to understand institutions on the ground, and to do close-hand study of how institutions develop.

HKA: The emergence of organizational forms and innovation is a recurring theme in your work and it’s also central to what you characterize as your most favorite book, with John Padgett.¹³

WP: Yes, John Padgett and I began working together around 1999. Our venue was the Santa Fe Institute. We had known each other before that, having shared a house in 1992 when he was on sabbatical and I was visiting at Stanford. We stayed up late at night. John was building a data set on Renaissance Florence, tracing the Medici Family, developing marriage records and the like. I was building little models, with similar pico-level data, on academic labs and how research teams were formed.

We’d stay up very late into the evening talking about building network data sets. Then, we started getting the idea that there were features that Renaissance Florence and contemporary Silicon Valley had in common. We started talking more and more about that and convinced some people to support us at Santa Fe. Santa Fe was not a place, at the time, that had been particularly receptive to social science. John deserves huge credit for building a community that eventually was embraced. Once we were members, it was like drinking water from a fire hydrant. We would go every summer for a period of time.

We pulled in people from different disciplines, like Walter Fontana from systems biology and Sanjay Jain from physics and Doug White from mathematical anthropology. (Doug studied kinship and migration with network models.) We would invite our students and colleagues down. We’d work initially on Florence and biotech together, trying to find the ways in which novel organizational arrangements occurred. In John’s case this was the birth

¹³ **John Padgett** is a political scientist and sociologist at the University of Chicago. He obtained his PhD from the University of Michigan in 1973 and works primarily on institutional innovation and state and market co-evolution. His research of Renaissance Florence is based on a unique, hand crafted data set of tens of thousands of individual and city records to model social networks and catalytic co-evolution from 1300-1500.

of the partnership form in Florence, in mine it was the creation of a science-based company in Silicon Valley. How did these organizations develop and evolve?

One moment I still remember as a turning point was that I was sitting with Paul McLean, who had been a former student of John's, and Paul was looking at these letters of credit from Florence in the 1430s. I was looking through letters about licensing requests to Stanford from the 1980s. Just by accident, the Florentine letters were letters asking for a loan and then the response. The letters I had a stack of—they were letters Jeanette Colyvas had collected and given to me to take down to Santa Fe to look at—were letters in response to requests to license the Cohen-Boyer gene splicing patent.

And the hair literally stood up on my back, because even though these letters were seven centuries apart, they were virtually identical. The Florentine letters said something like "*mi caro amico* (my dear friend), I have before me your request for a loan. I am sorry that I cannot grant it. We have so many entangling friendships, and bonds of neighborhood and the like, I do not want to loan you money, I propose we go into business together." And this was, in this very writing, the beginning of the partnership system.

The Stanford letters would go to little start-ups, and go "my dear friend, we have before us your request to use the Cohen-Boyer patent. We normally charge \$50,000 per year for its license, but in this case, we think that would be inappropriate. We have so many entangling ties of science and friendship, we think it not right to make this a commercial relationship. Instead, we will give you the license for free and ask that if you ever invent anything we get 4% of the royalties."

It turns out that more than 200 million dollars of royalty income came to Stanford from the small handful of companies that initially got the license for free. That got John and me immediately thinking about the ways that affiliations get repurposed in another realm. In John's case, it was a family tie getting used in the commercial realm. In my case, it was a science tie being used in a commercial realm. And that family and commercial tie in

Florence transformed the world of politics and eventually the economy. In the 21st century case, we suddenly saw the way in which the university changed and the way in which the nature of corporate enterprise changed.

Just think about it today, you can't think of any tech company that doesn't refer to their offices as a campus. They look like campuses, they try to act like campuses. And of course, universities look more like firms today. And it was through seeing this role of amphibians who move back and forth between realms, carrying practices into unfamiliar settings and then repurposing them back into their original setting and transforming it, that John and I started thinking about: "where does the emergence of fundamentally new categories of thought and new categories of organizing come from?"

The book was just a huge sprawling book, maybe too much to digest. Maybe we would have been better off writing a simpler, easier version. Certainly Peter Dougherty, formerly editorial director at Princeton University Press, has said that to us. Nevertheless, it was the most exciting intellectual project that I've ever been a part of. Our editor, Eric Schwartz, was so phenomenally enthusiastic about the project, and they produced a beautiful book. Even today, it's just an absolute blast when undergraduates or grad students walk in to my office and say, "oh I want to study vulnerability, I want to understand when systems tip, I want to see when things are likely to be vulnerable to perturbation or reconfiguration." That book tried more than any other that I've worked on to, not just describe and categorize, but really theorize something brand new. We do a very a poor job in the social sciences of studying emergence. Most social science begins with the second act of a play, after the dust has settled, to use Peyton Young's language. This was an attempt to see where the dust came from.

HKA: You also work on nonprofit organization and philanthropy. Your handbook on nonprofit organizations is now on its third edition, just published in 2020 by Stanford

University Press. How has your thinking about nonprofits evolved as an organizational form, also in terms of their role and impact?

WP: The early work on nonprofits, you were also very much a part of it, Helmut. We were at Yale, working on nonprofits, and there was a sense that there was a part of the economy that was not well-represented in national economic accounts. And yet, that part of the economy cared for children, the sick, and the elderly. It seemed like a significant part of the American civic landscape, and it ought to be studied. At Yale PONPO, we built, arguably, the first serious scholarly program for studying nonprofits, with an amazing cast of people.

The work here at Stanford has been different. I think it comes from different sources and asks different questions, less about mapping or building a theory of nonprofit enterprise, and more about consequences. There were people here at the Graduate School of Business—Joel Podolny¹⁴ in particular, who was then the academic dean—who wondered to what extent a Silicon Valley mindset of entrepreneurship was spreading into the nonprofit world. He persuaded me to start this project, the Stanford project on the evolution of the nonprofit sector, which thanks to a wonderful successive cohorts of graduate students, especially Christof Brandtner who deserves a special shout out, has been going for nearly two decades. This inquiry has now been folded into our Civic Life of Cities Lab, a large-scale comparative project, where we study civic life in six metropolitan regions around the world—San Francisco, Seattle, Shenzhen, Taipei, Sidney, and Vienna.

<https://pacscenter.stanford.edu/research/civic-life-of-cities-lab/>

We initially studied a random sample of SF Bay Area organizations nonprofits to learn how they have become more managerial, more technologically savvy, more rationalized. Of course, my interest is in whether such expertise has come at the cost of participation. So, we examined whether, in Theda Skocpol's language, nonprofits were doing for others, but not necessarily doing with them. What is gained, and what is lost, by nonprofits being more

¹⁴ **Joel Podolny** is a sociologist who is currently the Dean of Apple University, the in-house training center of Apple, Inc. He earned his PhD from Harvard University in 1991 and later went on to be Dean of the Yale School of Management. His research focuses on social networks and sociological theories of market competition.

managerial? Alongside these changes in how nonprofits are organized has been a sea change in American philanthropy, particularly on the West Coast. New wealth and new tech money has moved into the nonprofit world and done so in a major way, displacing older East Coast and Midwest philanthropy, or markedly transforming it.

The biggest foundations are now on the West Coast. You can date that to 1999-2000 with the formation of the Gates Foundation, or you can go earlier to Hewlett and Packard. My colleagues, a political philosopher Rob Reich, and organizational behavior scholar Deb Meyerson and I got together to establish a center to study civil society and philanthropy, that is how the Stanford Center on Philanthropy and Civil Society (PACS) came to be formed. So, one thing that I've really tried to keep an eye on is: what has been the role of this outsized influence of philanthropy?

We've tried to shine a light on the philanthropic sector. I currently have a chapter in the new edition of the Handbook called "Seeing Like a Philanthropist" (2020), written with my student Aaron Horvath, that draws a line between Carnegie and Rockefeller to Laurene Powell Jobs and Chan-Zuckerberg. We examine the ways that the wealthy have taken their money and used it for philanthropic purposes by using particular metrics in the same way they did in their businesses. The Gates Foundation, as one example, has championed a measurement-driven approach. This orientation, coupled with the foundation's vast financial and cultural influence, has come to be reflected throughout the nonprofit sector, shaping how many nonprofits –including those not funded by the Gates Foundation – think about their work, understand themselves, and present themselves to others. No money without metrics. We think that this transposition, to use Padgett and Powell's language, of turning business practices into moral standards for nonprofit organizations is a profound change.

HKA: Your work, of course, looks at the social relations to institutions and you put an emphasis on commerce, the economy, and how that changes social relations. But what

about the political? In your work, would it be fair to say that there is less of a political element than was the case with your mentors Lew Coser or Chick Perrow?

WP: Interesting question. Certainly, there are people who think institutional theory is weak on politics. I happen to think that is right. Others might not share that view. There is a recent essay by Kamal Munir called “Challenging Institutional Theory’s Critical Credentials” (2019) in which he makes a compelling point that institutional theory doesn’t have so much of a critical edge. Here, I think we need to define critical. The question is whether critical and political go together. By critical, he means that rather than analyzing the way things are done, we should pay more attention to why things are done the way they are. If you think about institutional analysis, its focus on “taken-for-granted-ness” and legitimacy or logics seldom asks “whose legitimacy?” So, I think that’s a fair point. Moreover, we know that organizational life is shaped by a country’s political dynamics and the power relations among capital, the state, and civil society. I don’t think organizational institutionalists have given much attention to these multi-level influences.

In Charles Perrow’s case, he was thinking much more about what are the structures of authority in society that produce products and services that have the potential to harm us. He didn’t focus that much on the political in the sense of government or politics, but his work was critical in that it always questioned the status quo.

In Lew Coser’s case, partly because so much of his life came as part of struggles against fascism and his involvement in socialism, he was much more interested in political life per se. Not that much of his writing as a sociologist appeared that way. He wrote under two names early in his career. He wrote “Lewis A. Coser” as his sociological side, and “Louis Clair” was his political side. He used that when he wrote for magazines of opinion.

The network analysis work I have done has the danger that it is “only” structural. I dissect and examine the dynamics of affiliation, but am often silent about who is advantaged by those affiliations. Networks exclude as much as they mobilize. I would say that my work

hasn't thought enough about those consequences. Of late, over the past decade or so, particularly with the creation of PACS, I've tried to focus more on the question: "what is the role of wealth in a democratic society?" And what influence does Big Money have on our conceptions of what is fair and what is appropriate?

Aaron Horvath and I (2016) tried to characterize much of contemporary philanthropy as "disruptive philanthropy," pointing out how people like Bloomberg and the Waltons and others have used their philanthropy in ways that are contrary to government provision of services, trying to find alternatives to those services, for better or for worse. In our recent political history of philanthropy (2020), we look at how the values of the wealthy become, through philanthropic efforts and through nonprofit organizations, imposed to become the values of democratic societies. Our very conception of what we are as a public today is deeply shaped by philanthropic money. So, for my earlier network and institutional side, you might say "guilty as charged," but I think recent work much less so. That's the advantage of getting older, I guess.

On the Padgett and Powell book, I think the political issue is a good question. There's a review essay, an interesting one, from two British Marxists, or critical management scholars, who argue that politics are "everywhere and nowhere in the book" (Reed and Burrell, 2019). They assert that the book is all about political life, but it is politics with a small "p", not about politics with a big "P", it's not about big political structures that constrain and close opportunities. And well, there's probably some truth to that too. But certainly, the Padgett and Powell book examines the political in a way that I don't think is tone deaf.

HKA: Let's look at the social sciences more generally and reflect on past developments, and see what you might see as possible future trajectories. Looking back over the course of your career so far, how would you characterize the general development of sociology?

WP: It's a striking trendline, because the field has changed dramatically in terms of composition. Stony Brook, a public university, was an unusual place in the 1970s in that there were more than a handful of female faculty. At most universities, there were hardly any. But there were few female graduate students. Today, the composition of graduate programs has almost entirely changed. Faculty composition, of course, changes much more slowly. I try to explain to my undergraduates that they turn over every four years, but faculty are on a thirty year clock. They are, understandably, impatient!

Topically, you've seen ebbs and flows. Organizations and the professions were central topics in the 1970s and 1980s, but a lot of the interest in organizations has moved into professional schools. Economic sociology has emerged, but it too has moved into professional schools. I think the substantive area that has had the most staying power is stratification, which today is invigorated as the study of inequalities. Locally, and I think at many universities, the new looming area is computational social science.

Departments all over are trying to think about these new methods—like computational network analysis, linguistic analysis, all kinds of things are analytically possible now. There's been an experimental turn with graduate students using Amazon Turk. There's much more quantitative precision, with the CSS influence, the data richness is dramatic. At the same time, the core set of theoretical questions and reasons for doing the work seem to not be as strongly motivated as they once were. Probably every generation says that, but so much work today asks the question, "What gap are you filling? What lacunae in the literature?" rather than "what larger questions about the social order are you speaking to?"

There is a wonderful initiative that I'm involved in at the Center for Advanced Study in the Behavioral Sciences. Bob Gibbons¹⁵ (of MIT Sloan) and I run a summer institute on organizations and their effectiveness. It's a small annual summer program with 15 assistant professors from across the social science disciplines. They all arrive well-steeped in their

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respective disciplines of economics, political science, sociology, and management. Our job in two weeks is to scramble them and teach them how to steal ideas. We require them to engage in intellectual theft, and to take an idea from another discipline and rethink their own work. We've done this for four summers now, and this summer had an exciting online convocation of roughly 45 of our alums. Bob and I think of this community as the dream department we will never have!

What's so great to see is how jazzed and how fervent the interest is in borrowing across disciplines and thinking about problems like high-performing bureaus in poor countries, or why there are persistent performance differences across hospitals, or why some international development programs work and others fail. These superb young scholars want to answer these pressing problems and use ideas and methods from as many fields as they can find. This is the most exciting thing that Bob and I do at the moment. It's just a small dent, but I think it speaks to the barriers between disciplines being much lower and more porous than they were years ago. Back in the 1980s as a junior faculty member at Yale, when I wandered into the econ department to talk with faculty such as Dick Nelson, Sid Winter, Martin Shubik, or Mike Montias, or Henry Hansmann at the Law School, and to political science to talk with Bob Dahl and Ed Lindblom, that was seen as renegade behavior. Today, I don't think that would be the case at all.

HKA: And Yale was a better place than most at that time.

WP: Yes, indeed.

HKA: Well, thank you so much Professor Powell.

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