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CORPORATIONS AND CAPITALISM: A CONVERSATION WITH JERRY DAVIS AND MARK MIZRUCHI

Accounts Editors: You have both written books recently that address the transformation of the corporation during the 20th century. Both books argue that fragmentation was the central dynamic confronting the corporation in the late 20th century. Yet, you interpret this fragmentation differently. Whereas Jerry identifies radical democratic possibilities in economic localism, Mark seems less optimistic. Mark, do you think the fracturing of the corporate elite could create spaces for new, more democratic models of economic organization to emerge? Jerry, do you think Mark should join a CSA and stop worrying so much?

Mark: Well, actually, one characteristic of the U.S. that I don't think has changed over time is that there have always been opportunities for nontraditional forms of organization. There have always been niches in the United States. It's one of the great virtues of this country. If you wanted to have a co-op in the 1960s, you could. If you wanted to live in a commune, you could do that. If you wanted to start an unconventional organization, it might be difficult to succeed—because any time you try something that could cut into the mar-

kets of better established organizations, you're going to face certain obstacles—but you could still try. I think the question you are asking is: the fact that the elite is more fragmented now, does that open more opportunities for these alternatives to emerge? And, well, I'm not sure I see a necessary connection between one and the other.

Jerry: I'm going to be a technological determinist, which is not like me, but I think that's what's different now. One of the things that's undermining the ability for elite cohesion is that corporations can't make a living anymore the way that they used to. The number of listed corporations has dropped by 55% in the last 15 years, and they keep going belly up or morphing out of existence (Borders, Circuit City, Westinghouse, Lehman Brothers). If you look at the companies that are still around, like Sony, they're not going to survive for the next 10 years, at least as electronics companies, because Vizio is cheaper at doing what Sony does. There's a lot of overhead in being a social institution. If there's a cheaper alternative way of

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*Kelly Russell
Mikell Hyman
Linroy Marshall II
Richard Rodems*

*Department of Sociology,
University of Michigan*

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producing stuff, then the cheaper way is likely to win. Airbnb is cheaper than Hyatt. Uber is a lot cheaper than taxis with medallions in NYC. So if the economic basis of corporations erodes, you can't just say corporations are really powerful, they always get their way. There were five big investment banks at the beginning of 2008 and there were two at the end. There was one giant insurance company, AIG, and it got taken over by the state. GM used to be the world's most powerful corporation, and then it wasn't. So you can't just say just because they're powerful at one point, they're always going to be powerful. I think the transaction costs that favored the large corporation in the 20th century don't favor it anymore, and that's why I think there's opportunity for alternative forms. Given how cheap it is to start a business these days, then why not try worker ownership? Why not an alternative? If capital equipment is cheap, if communication technologies are cheap, then why not have something at a small and local scale that is more democratic. The outcome of this shift is not foreordained. I think we're at a turning point where it could go in a couple of different ways. Joyce Rothschild wrote this lovely book in 1986 about co-ops. She had done her dissertation work in UCSB about all these co-ops started by hippies in the 1970s, but the problem with co-ops is that people spend 20% of their time in meetings. I mean some of it might be...

Mark: You just gave a good example of why democratic management might not be the way to go.

Jerry: Oh, really?

Mark: Yeah, the problem is, if you're going to run a company democratically you have to spend 20% of your time in meetings.

Jerry: Okay, some of that 20% is Habermastica-

tion™. Sometimes meetings are worthwhile because you're sharing information and figuring things out, but sometimes meetings are a waste of time. Some decisions could be more efficient because we have the technology to be more democratic and local than we used to (say, using a voting app). The alternative less-democratic version of that is let's say Uber, where you use the same technology to create a class of Student Loan Activated Volatile Employment... it's an acronym.

Mark: ...Slave.

Jerry: Yes. In Ann Arbor there must be 5,000 people driving for Uber this second who are recent sociology undergraduates, who have discovered that they are unemployable but they have to repay their student loans. That's the digital immiseration version of this technology.

Editors: You both take the provocative position that corporations and their leaders aren't especially powerful anymore. If corporations have collapsed, unions have been destroyed, and the state has lost legitimacy, who is running the show?

Mark: One theory is that the show is being run by the capital market [see Davis 2009]. Another theory that I am playing around with now is that the system is being run by what I'm calling "inadvertent robust action." The outcomes are as if we have Cosimo running the show [see Padgett and Ansell 1993], but without Cosimo. There is confusion, ambiguity, and it's very difficult for any kind of insurgent group to oppose the elite right now, because they are difficult to identify and locate.

Editors: It seems like people have identified "the 1%" as the elite right now.

Mark: Well yes, Bill Domhoff's theory about power is, "Just look at the outcomes." Assume everyone

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wants to be rich. Find out who is rich, and then assume that they must be the ones with power, since they have what everyone wants. It's not the 1%, that's too broad; it's more like the .001%. And interestingly, one of the reasons I suspect there's so much fragmentation now, is the higher up you go on the income and wealth distribution, the more severe the level of inequality. People's relative deprivation compared with their perceived peers is greater for those who have \$20 million of wealth than it is for those who have \$100,000. And it's lower than for people who have \$50 million of wealth. I don't think that's the cause of the fragmentation but I think it exacerbates it. So where is the power now? I don't know.

Jerry: I want to sound sociological for a moment. Imagine hegemony were a plausible idea. If people all bought into a set of ideas, the ideas themselves would rule without the need for particular people to step in and take action. Fred Block wrote this wonderful article in 1977 called, "The Ruling Class Does Not Rule," arguing that you don't really need a bunch of people serving on each other's boards and going to Bohemian Grove to plot the doom of the rest of us, if action happens without their direct intervention. Imagine that corporations do what the stock market tells them to do. You can play with it a little bit, but you can't really tell the stock market how to price things. It doesn't work that way. If companies do what the stock market tells them to do without any intervention, that's like hegemony. If you've got presidents like Clinton who lived and died by what the bond market did in response to his speeches, you don't need individual capitalists or a vanguard of elites who know each other, you respond to the market because the market tells you what's in the long-term interests of the class. And if you get more than 50% of the population invested in the stock market, which the U.S. has had since 2000, then the people whose money is at stake and the corporations are both organized according to

this signal: the stock market. What this looks like for me is my sister, the retired 4th grade teacher. She is invested in Fidelity, and every morning to this day she logs into her Fidelity account and sees what the market's doing. If the market's up, lunch at Red Lobster, if the market is down it's a tuna sandwich at her desk. Her well-being on a day-to-day level depends on what the market's doing. That's hegemony. You don't need individuals to act if we all believe that this system works and serves our long term interest. If you get much of the population thinking as investors rather than workers, mission accomplished.

Mark: That's the long version of my inadvertent robust action theory.

Editors: People who argue the reverse would point to massive political donations and profits that corporations are reaping. Seemingly politics continues to go their way. Is that also unintentional?

Jerry: It's not as straightforward to buy elections as you'd think. Imagine that someone who had been CEO of a giant corporation, had \$100 million of their own money to spend, and was a female Republican wanted to become governor of California, or a senator for California. Turns out it doesn't work! You could spend tens of millions of your own money and still not win an election. How could Mitt Romney not win? All rich people on earth wanted this guy to win. It would be wrong to think there's a straightforward correlation or relationship between spending and political outcomes. Money is like a flame for moths. The politicians are going to head to it, but I don't know that that translates into immediate policy impact of the sort that they'd like, but...I don't know.

Mark: Generally correct, but here's the deal. First of all, money is a necessary condition to play in poli-

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tics, but as Jerry suggested, it's not a sufficient one. You need big donors to get in the game, but that doesn't guarantee that you'll win. There's never been any clear evidence that money influenced election outcomes. There's never been any clear evidence that people in Congress voted in accordance with the wishes of the people who contributed to them, and it's not because there's not a high correlation—they're very highly correlated. If you're a congressperson from a dairy farm district in Wisconsin and there are representatives of that industry who are giving you money because you're the local congressman, well obviously you are going to vote in the interest of those dairy farmers. But you would do that anyway. They're giving you contributions because you are the local congressman, but you'd be crazy not to support their interests, because they are important constituents in your district. So then then the question is: what do these campaign contributions buy? The primary thing they buy is access. Number two, there is evidence that members of Congress will respond in ways that are not publicly obvious. "I can't vote against this bill, but I can water it down behind the scenes." "I can't oppose this publicly, but I can kill it before it gets onto the floor." So what happens is what Pepper Culpepper calls quiet politics. In areas where there's not a lot of publicity, there tends to be a stronger relation between the donors and what the legislators are actually doing in response to that. When things become more publicly known, then they have to worry about what the public in general thinks and then the relationship tends to disappear. But it has been very difficult to find causal relationships in this area of analysis because a lot of it would have happened anyway. And yet, Romney didn't win. And Sheldon Adelson didn't get his wish.

Jerry: And Meg Whitman, and Carly Fiorina...

Mark: But they wouldn't have been in the game at

all if they hadn't had the money to start with.

Editors: Speaking about corporations, elites, and politics, let's return to your books. For Mark, our question is whether it is realistic to call for enlightened self-interest on behalf of corporate leaders in the absence of the structural conditions that made that enlightened self-interest possible. In other words, is it possible to have an enlightened self-interest today without the conditions that were generated by the contingencies of the post-war moment? And Jerry, the question for you is: What is it about that old powerful elite that gave rise to, what Carl Kaysen originally dubbed, the "soulful corporation"?

Mark: The answer to your question is probably not. This raises the question, why did I call for enlightened self-interest without the structural conditions to begin with? The answer is that I am not as pessimistic as you might think. What are we going to say, that there is absolutely no hope whatsoever? You are right that theoretically, my argument suggests, exactly as you put it, that the reason they acted with enlightened self-interest had to do with the structural conditions that were prevalent at the time, that they had to deal with organized labor, and they had to deal with a state that had a high degree of legitimacy, and they knew that the public was supportive of the New Deal, an activist government, and protecting the general population. They had to work with that. In their ideal world there would have been no labor unions, no regulation—except for the regulation they wanted—and no Social Security or Medicare later on. But, in the world they were dealing with they had to make their peace with it and ultimately they figured out it was in their interest for the population to have money in their pockets so they could buy what was being produced. Nowadays we don't have those conditions and they've wiped out all sources of opposition. The government has been completely de-legitimized, organized labor has been completely smashed. The only place it exists at all is among public employees whose interests ap-

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pear to be directly opposed to the public who pays their taxes to support them, and therefore they get no sympathy from people. So the idea of organized labor, as with government, has been completely discredited in the United States. And there is no social movement opposing the power structure, wherever it is. There is no viable social movement at least. There was a little bit of a twinkle with Occupy Wall Street, but that seems to have fizzled. It did have enough of an effect that even Republicans are talking about inequality, but it has had no policy impact at all at this point. So there are no structural conditions that would suggest or give any reason for the leaders of the corporate community, even if they wanted to, to exercise enlightened self-interest. So why would I ever call for it? The answer is: we are going down the tubes with global warming right now and I don't see anyone in a position to do anything about it except the people who actually have the power to do it which is the heads of what's left of large corporations, the state, and people who answer to them. Somewhere along the line they have to realize that it's their planet too. Basically what I was doing in that last paragraph of the book was appealing to them. If only they would buy this book and read it, they would start to exercise the enlightened self-interest that has been missing for the past four decades.

Editors: But doesn't your book say that even if they wanted to take you up on their plea for enlightened self-interest, they would be incapable of that action?

Mark: Well part of the plea is not only to have the will to do it, but the willingness to organize. Again, something they are not very good at now, even in their own interests.

Jerry: Do you feel that there is a generational shift? Like that the Silicon Valley average CEO is a 27-year-old white guy that quit college and wrote some stupid app in his dorm room and is now a billionaire?

Mark: As opposed to a 60-year-old white guy?

Jerry: They are a different crew. Enlightened self-interest in a 27-year-old white misogynist—

Mark: There have been three generations. First you had the Reginald Jones generation—

Jerry: He was the CEO of GE in the 1970s—

Mark: Well before that in the 1950s and 1960s you had the Charlie Wilsons: Charlie Wilson from General Electric and Charlie Wilson from General Motors. They even had the same middle initial I think. They were both Charles E. Wilson, and one was called Electric Charlie and the other was called Engine Charlie. No relation. And these guys were statesman-like, and they believed that, "Yes, we as elites in society have an obligation..."—not that they were altruistic, or even particularly liberal, they just acted in enlightened self-interest. They felt that their paychecks came from the fact that people have money in their pockets and they can therefore buy our cars and washing machines. Basically that generation got wiped out by the takeover wave of the 1980s (and I think Jerry's book and work does as good a job as any describing that whole process, and I have a chapter where I talk about it). And what replaced them was a group of cowboys. So you had "Neutron" Jack Welch, and "Chainsaw" Al Dunlap, and people like that. And they were heroes of Wall Street because they managed to inflate or increase shareholder value. And they did so in ways that were decidedly not enlightened, at least by traditional standards. So this was a new generation of swashbuckling, slash and burn CEOs who got a lot of publicity, whose sole purpose was to raise the company's stock price and to hell with everything else. What you're talking about is yet another generation now, like Zuckerberg and these 20-year-old guys with billions of dollars in Silicon Valley. And they're different still because on some issues the Silicon Valley crew has

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been relatively liberal since the early 1990s. And one of the sources of that was, prior to that they were Republicans like everyone else, and Ronald Reagan had appointed the head of Hewlett Packard, John Young, to head a commission to study the possibility of developing an industrial policy in the United States. In the 1980s there was a lot of talk about the fact that we were getting wiped out by the Japanese, and maybe we needed an industrial policy, to have the government focus on certain industries and help them the way some East Asian countries had. So Young put together a serious report that said we should invest in job training, education, and R&D (the story is in the book), but Reagan's people blew it off. Later, he tried to get George Bush Sr. to respond to it, but he got nowhere. So he, and John Scully, the head of Apple at the time, and 29 other Silicon Valley executives took the proposal to Bill Clinton, who was an up and coming candidate for president who had made a connection with some of the Silicon Valley entrepreneurs, and had given some evidence that he was interested in emerging technology. Clinton expressed his support for what he was doing and overnight a group of 31 Silicon Valley executives signed a letter stating their support for Bill Clinton and the Democrats, and they've been liberals ever since. Now, they are more liberal on social issues than they are on economic issues. This is not the Al Dunlap, Jack Welch generation. The jury is still out on this group. They are kind of libertarian in a way that suggests that they probably aren't going to become the Committee for Economic Development of the 21st century. I'm not sure about that. But Warren Buffet seems more socially conscious than these Silicon Valley guys, at least on economic issues.

Jerry: I want to pick up on something Mark said that might address the soulful corporation question. As you were describing it you mentioned generations, and I wonder if we overlook, or don't pay enough attention to the fact that the Depression had

an incredible effect. Prior to the market crash, a huge number of Americans had invested in the stock market and lost their shirts. It took decades for people to get back into the market because it just looked like a giant casino. People said, regulate the hell out of Wall Street because they are dangerous, and it is a giant casino. There wasn't going to be generation of Wall Street types having a big influence. So you have the Depression and a set of regulations followed by the war. If you wanted to induce patriotism in an entire generation there is nothing like a world war, that is an existential threat to your nation, where the government in effect takes over the operation of industry and turns car factories into tank and airplane factories, and sets wages and induces a set of bureaucratic rules all over industry. That's the kind of universalizing, nationalizing, experience that makes companies patriotic and think that there is something bigger than the Ford Motor Company or General Electric, that they are part of the arsenal of democracy defending us from fascism. So that's a pretty unique experience. And then you had an entire generation coming back from the war, being accustomed to taking orders, and fairly high levels of hierarchy and thinking that's normal and efficient, that's the way that we won the war. So there are a lot of factors that made soulful corporations, and hierarchies, and men in the grey flannel suit seem plausible then that would be really hard to recapture now. People have not had that experience of a national, existential threat that would create that same level of unity. The notion that you would incorporate in Bermuda for tax reasons would have been utterly inconceivable in 1950. You get people who have not had that experience, who are used to globalization, you're not likely to get that same level of national orientation or soulful corporation—the notion that you have obligations as a corporate citizen.

Mark: I completely concur with that, I just want to add to it. I think there was something about the experience of the generation that was running the com-

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panies in the '50s and '60s that did affect those years.

Number one, as Jerry said, living through the Depression. Number two, World War II was a critical factor not just because of the bureaucratic experience people had, which fit smoothly with the companies that they ran, but the other piece of it was a lot of the big businesspeople during the war actually served in government. While they were serving there, they discovered, "Wow this is not so easy and these people have real legitimate issues they're trying to deal with, and looking at it from this perspective, I can kind of understand where they're coming from." And then when they moved back into their companies they had a renewed appreciation for not only how difficult it was to operate the government but how useful much of what the government did was.

Jerry: That's a deep point. That's a really deep point. Because if you think about peoples' experience with government now, kids will graduate from Michigan Law School claiming that they want to go into public service and they'll go work for the SEC for two years, but it's basically a lightly paid internship where you learn just enough about the way the SEC works so that you can then go to work for a Wall Street law firm and undermine everything that you worked for at the SEC. Government service now is a lightly paid internship that you then use against them, whereas government service in WWII was legit.

Mark: It actually was service. So one possible piece of this story—and people have asked me about this and I have to say, I think there's something to it—is it's the Tom Brokaw, the greatest generation. These are the people who went through the Depression, they won World War II, they came back and they put together a society that—let's not downplay the faults, I mean we could spend the rest of the day telling you about all of the horrible things they did, like

overthrowing democratically elected governments in various parts of the world, for starters. Nevertheless, if you look at it comparatively, compared to where the elites are now, these people did have a certain sense of responsibility, and the belief that they had an obligation to at least not kill the goose that lays the golden egg.

OK, now, to speak to the second point that Jerry made, part of what came out of the Depression was lots of regulation, especially of the financial world, which made banking one of the most secure but also boring occupations on the planet. So they had the whole 3-6-3 rule, which was borrow at 3%, lend at 6%, and be on the golf course by 3 p.m. That was the case until we had the deregulation in the late '70s and in the 1980s when the whole thing just came apart. Reagan's people got in and they staffed all the agencies and they were proponents of this new hot theory, agency theory, which said that, number one, the only thing that mattered was shareholder value, and number two, hostile takeovers and acquisitions of any kind were efficient because they happened. If they hadn't been efficient, they wouldn't have happened. So, they let things run amok and the '80s takeover wave was in part facilitated by that. And all of the financial shenanigans that we've seen in the past 25-30 years were accompanied by the deregulation, most recently Glass-Steagall in 1999.

Now, the first reverse of the Davis-Krippner financialization trend that I've witnessed in the last I don't know how many decades was when General Electric announced the other day that they are getting rid of GE Capital, which had been their biggest source of profit. And the complaint was on the front page of the *New York Times*, three or four stories in the business section. What was the problem? Well, all the banks are having terrible trouble now because of this stultifying new regulatory environment, which is forcing them to have minimum capital requirements, and therefore making their lives less risky, which of

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course is going to reduce the rate of return, which, if it continues at this pace, might actually turn banking into the boring kind of job that it was in earlier years, although I think they're complaining a lot more than is warranted, as they have been accustomed to doing in recent years. But anyway, it was the first time it occurred to me that, wow, these new regulations might actually have some teeth in them. Now, whether it's going to continue remains to be seen, but I thought that was an interesting phenomenon.

Editors: This is a follow-up question for Jerry. You were talking about how people lost faith in the stock market after the Depression. In the Wall Street crash of 2008, a lot of people lost a lot of savings. But it doesn't seem like people have lost faith in capital markets in the same way. How do you make sense of that?

Jerry: That was surprising to me, surprising in a couple of ways. I have a paper with Natalie Cotton-Nessler that we presented in the ASA about eight years ago called "Does Buying a Mutual Fund turn you Republican?" A great rhetorical question. The answer was yes. So between 2000 and 2004 shareholders went very Republican. Bush and certain theorists in the Republican Party were actively recruiting them. They called it the investor class theory. The investor class theory was, when people own shares they start reading the *Wall Street Journal* and believing it. So, like my sister, they start looking at the market going up and down, and then they say, oh my gosh! When Bush announced capital gains tax cuts the market went up and I went to lunch at Red Lobster. It's almost like they grew this extra sense organ to connect them to the ups and downs of the stock market. It's on your phone—you can see what the market is doing right now! Wal-Mart announces they're raising the minimum wage at Wal-Mart stores to \$9/hour, their share price went down 3.2% or \$8 billion dollars. So, oh, all right,

paying people a living wage is bad. Investor class theory says people will make these connections and then they will support policies that are pro shareholder. So if you'd invested \$10,000 in the S&P 500 the day George Bush took office, you'd have \$6,000 the day he left. No one has ever been worse for the stock market in American history. You'd think that investors would say, "Oh this whole Republican thing, I'm not so sure." And they didn't, and it was shocking that shareholders continued to identify as Republican at very high rates. Which we found puzzling. Natalie Cotton-Nessler created a very sophisticated account drawing on the work of Delia Baldassarri at NYU and Amir Goldberg at Stanford, trying to unpack why it is that shareholders did not abandon the Republican Party. Being an investor became part of their identity – it was not just about economic incentives. Also, if it's your 401K, you're kind of stuck with the market you've got. People in 1929 might have owned two stocks, and so when those two stocks evaporated they were kind of stuck. People today don't buy stock. They buy mutual funds or exchange traded funds invested in the broad market. So they lose money, but they don't lose everything. The market goes down 40%, but they don't lose it all. So people kind of stuck with it. You didn't see this collapse going from half the population owning shares to 10%, which is what happened in the Great Depression. Our pension system now puts people into the stock market whether they like it or not, so they weren't really able to escape the vagaries of the market. And Obama has been gold for the stock market. It's been a magical period. If you just sold every time that a Republican started office and bought when a Democrat started office...

Mark: It's almost tripled since 2009...

Jerry: Yeah, it's quite amazing. And so in retrospect, you wouldn't look back and say, yeah I should have sold everything. The market did come back,

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though. I don't know what to do with that.

Mark: I have a short version of the same answer. I think 2008 was different because it wasn't bad enough. If you compare it to the Great Depression, the problem was that not enough people were really severely hurt, and that's why we didn't get a real turn to the left politically like we did in the 1930s. You can't compare 10% unemployment with 25 to 30%, and there were no people jumping out of windows. People lost a lot in the stock market but they didn't lose everything. A lot of people got hurt really badly but not enough.

Editors: Jerry's almost making a false consciousness argument that people still buy into the stock market against their better interests because it's intuitive in certain ways.

Jerry: We don't have a very good counter-narrative.

Editors: Katherine Cramer, at the University of Wisconsin, delivered an interesting talk on campus recently about why people vote against their economic interests. She interviewed a lot of people in rural communities and found out essentially that they look at the cities and they think elected officials are just interested in funneling money toward people in the cities. They don't care about people living in rural communities. Which is actually not really something we think about much, what's happening in these rural communities. It just seemed like another side of it.

Mark: But that doesn't explain why they vote Republican. That's what I've never understood about the arguments that people usually make about why poor white people vote for Republicans. And it's usually, well the Democrats don't really do anything for them. And that's all true, it's very easy to show that. But they're not nearly as bad for them as the Republicans. And so while that might explain a

lack of enthusiasm for Democrats, it does not explain why they then vote for Republicans who are even worse.

Editors: Cramer seemed to be arguing that Republicans are better at tapping into the concerns of rural communities by making moral arguments about social or family values, but also tapping into other concerns, like criticizing Democrats for building a train that doesn't go to their town, for instance.

Jerry: Well, the interpretation of the housing crash—one-third of the mortgages issued in 2007 were second or third or fourth mortgages. The mortgage crash was largely caused by speculators who were buying houses to flip and who walked away from underwater mortgages, and yet the narrative that won is that poor people are buying houses that they can't afford. And it's all because Democrats, in 1977, passed the Community Reinvestment Act forcing innocent banks to give mortgages to poor people. That's insane! I mean one third of mortgages were from people buying second or third houses. I mean that's the smoking gun. It was speculators and banks and not poor people, and yet this narrative manages to win out. I don't know, we're just not clever enough at being evil. We should be teaching a class on this.

Mark: Although to be fair, in terms of your question—Why would people still buy into the stock market?—if the Dow was at 6,500 in 2009 and now it's around 18,000, that's a pretty good reason to like the stock market.

Editors: It seems to boil down to people's capacity to tolerate volatility.

Mark: Yeah. Well that's what they tell most people if you're young. That's what they told me. When I

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came to the University of Michigan I got a brochure that said University of Michigan economists advise you, if you're young, to put 100% in the stock market and just don't even look at it.

Jerry: I think at your age, Mark, you should be buying gold.

Mark: Well yeah, maybe I should be out of it, but I'm not. My dad's 89 and he's still in it.

Editors: You mentioned briefly the 1% as maybe a misnomer, but can you talk more about the relationship you might see between the 1% and the corporate elite. Is it just a political slogan or does it have any analytical use as the corporate elite maybe ceases to be as relevant?

Mark: That's a really good question. I've thought a lot about that. So, there's never been a perfect correspondence between wealthy individuals and heads of corporations. And so for example, in the early 1980s when Reagan became president, he instituted these huge tax cuts for individuals that were highly unequal. They were 10% across the board, but the same percentage cut yields far more additional income the higher your income is (because the rates were higher at that point) so he immediately ran up huge deficits and within two years Reagan and the Business Roundtable had a fight because both of them agreed there had to be a tax increase, but Reagan felt the tax increase had to be on business and the Business Roundtable felt the tax increase had to be on individuals. And that made sense, right. Reagan wanted to protect his constituents: his wealthy constituents who had elected him, and small businesspeople were paying personal income tax for their companies; and the businesses were trying to represent their business interests. What's interesting about that though is that the CEOs of those companies—now maybe their incomes were nowhere near

what they are today but they were still at the top of the income distribution; these were wealthy people. And yet they were willing to advocate increased taxes that were going to disproportionately affect them individually, to protect their company interests. And probably on some level because they thought it would be good for the economy as a whole. So then we flash forward to the present, and, number one, CEOs now are making enormous amounts of money. Most of it is soft money, i.e. stock options, and it's not that their salaries per se have gone up but their compensation has gone way up—but this has come at a price. I would argue that they have much less autonomy today than they did in the earlier days. So it's not as if they're just getting the extra money, but the money they have has now vaulted them into the category of the super wealthy, at least for the heads of the biggest companies.

The differences between the super wealthy and the corporate interests are more fused today than they probably were in those earlier days. I think what happened during George W. Bush's presidency is that the Roundtable was still upset about deficits. Jerry and I were at a talk by John Castellani, the head of the Business Roundtable at the time, at the Detroit Economic Club in April 2004 and, it was a talk on outsourcing—how it's a great thing and we should all support it. But, in the middle of the talk—unscripted, because it was not in the published version that ended up on their website—he went off on this tangent, railing against the deficit, how terrible it was, wanting to rein it in. But during that soliloquy, there was not one word about George W. Bush's tax cuts having had anything to do with the deficit. So, unlike 20 years earlier, when that same organization—the Business Roundtable—came out for a tax increase on individuals, here it is now 2004, and there's not a word. So when I was writing about this, I couldn't get anyone in the Business Roundtable to

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explain it to me. I tried to figure out, why they were no longer willing to support a tax increase. I don't know the answer for certain, but it appears that when George W. Bush instituted his personal tax cuts, the businesses were upset and said: we want business tax cuts too. And Bush said to them, hold off on those, support me on this, and I'll help you out later. And they went ahead, and then a couple years later W. delivered. There's a man of his word—he pushed through the business tax cuts. My theory is that there was a kind of quid-pro-quo there; unstated, not necessarily a formal agreement. He promised to take care of them if they took care of him. He kept his promise, and the Business Roundtable then advocated making the Bush tax cuts permanent. What it meant was that they could complain about the deficit, but were no longer in a position to blame tax cuts for it.

Jerry: I think that the 1% is a stratum and not a class, and a class can't be based on income alone. My favorite theorist, Mark Mizruchi, distinguishes between a class-in-itself and a class-for-itself. A class-in-itself is based on factors such as the source of income. Ownership of the means of production puts you in a class-in-itself that gives you a set of interests that you have in common with other people, maybe opposed to the aristocracy—they have power and wealth based on the ownership of land which is ancestral and can't be sold. The new men have income on the basis of ownership of the means of production rather than land itself. So you can imagine that the basis of wealth these days might not map onto that same thing as it did in prior eras. Owning a factory gets you bupkis today. I can rent a factory and make a TV, and rent a brand name like RCA or Westinghouse, some defunct company, Frigidaire, or something that's long gone. Put the name on it and become the biggest TV brand in the U.S. (Vizio is my favorite example for everything). So it's trivial to

rent factories and have high revenues, and you can rent the means of production just as readily. So if the basis of wealth and power is something other than owning factories, then what is that thing? That's the project for sociology or class analysis today: to figure out, what is that basis? The billionaires today don't own any factories; they write some stupid app in their dorm room and suddenly they're rich. They write the best-selling video game ever. Flappy Birds—the most addictive phone game ever (not to be confused with Angry Birds)—was written by this guy in Saigon in the equivalent of his basement. And everybody loved this game, it was huge. The guy is making tens of thousands of dollars a day, and then he pulled it off the market because it was too addictive for people. So, the people that become overnight billionaires are the ones that write WhatsApp and sell it for \$19 billion. That's insane! That's far more than starting a new car company or owning a factory. People are getting rich for crazy reasons that don't map onto the reasons that we're familiar with, which is owning the means of production. It's some other thing that's harder to map onto. I don't know what to make of that: identifying what are the class interests? What is the class-in-itself?

Editors: Jerry, if owning the means of production is no longer the basis of wealth and power, are you suggesting that we've somehow entered into a post-capitalist society without even realizing it?

Mark: Ralf Dahrendorf returns, 50 years later. This time he might be right.

Jerry: Well, it's certainly post-industrial, but I don't know about post-capitalist. It certainly seems capitalist in tooth and claw, but the materialist basis isn't the one we know well.

Editors: With 50% of households involved in the stock mar-

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ket, is this the right-wing road to socialism?

Jerry: No.

Mark: It is still capitalist insofar as you can generate income and wealth on investment rather than production or creation of a tangible commodity.

Jerry: I guess the whole basis will fall apart. I can out-morose even Mark Mizruchi, because I think the valuations today make no sense at all. A company like Zynga that makes Farmville: why should that be a public corporation, when a guy in Saigon can write a better game in his basement? So I think we're using the structures of old-school organizations and old-school corporations to do things that aren't why we had corporations in the first place. We had corporations because they invested in long-lived assets like factories, railroads, stores, and so on. We're using that same organizational apparatus for video games, Facebook, Twitter: businesses that don't really require long-lived assets, that can use rentable assets. The non-morose version would be: if it costs next-to-nothing to start a company, then you don't need much capital, you don't necessarily need investors. It could be some form of collective ownership or municipal ownership, rather than the person who happened to have enough money to build the factory.

Mark: Except the problem is, it's certainly a possible outcome, but it's very far from being a logical outcome. There are so many other possibilities that are so much less happy. Why do you need collective anything? One person could invent something that generates enough wealth for that person to control the entire world.

Jerry: The problem is you don't get to actually control the entire world. The people who wrote WhatsApp sold their business for 19 billion, but there's ten apps that do exactly the same thing.

There's nothing really special about it. Kids can just say, "I'm gonna switch to Snapchat instead of WhatsApp"—it's pretty trivial. So they don't really control some rare and important resource; it gives you money but it doesn't necessarily give you power. There was a lovely article in *The Nation* saying, why don't we just have worker-owned Uber? Why don't we have Ann Arbor local Uber, and it's, you know, three dozen people who decide to band together and write their own damned app? How hard can it be to write the Uber app? There's nothing really special there. So you could certainly see possibilities for democratic transformation using that technology. It might have taken the Rouge plant and a lot of capital investment to make cars cheaply. But Zynga? Twitter? Uber? Anybody can do that.

Mark: So let me amend my earlier point. It's not a matter of having control over the world. It's a matter of whether you need anybody besides yourself to operate your firm. So you write the code for some new system, you become a multi-billionaire, who needs collective ownership? OK, maybe you can't control the world, but you generate a lot of wealth and you don't need anybody else. Part of the problem—and this is something we've discussed—is, it's not clear that any companies today need anybody to work for them. And so, the biggest firms right now—Google, Facebook—they have no employees. If you compare their capitalization to that of the behemoth companies of the mid-twentieth century, and then you look at the hundreds of thousands of employees that those companies had, versus what? 4000 or 2000 employees?

Jerry: Twitter has 3,700 employees and a market capitalization of 34 billion (I looked earlier today since I'm writing something about this). Kroger has 400,000 employees, and about the same market capitalization as Twitter, and a hundred billion dollars in sales. It's pretty crazy. I don't think that can keep up forever.

Mark: So where exactly are people going to get employment in the future?

Jerry: Universal basic income.

Mark: Should we should shut off the recorder [in jest]? This is where we need a system where employment is not pegged to profit. Because, basically what you have is a situation where we can generate everything we need without having everybody working for it. If that's the case, why not generate all the products we need, then distribute them? If there's no work for people, that's great. We don't have to do as much work. They used to have a name for this—which I'm not going to say—it started with an 'S.'

Editors: A specter is haunting this conversation.

Mark: It made a certain amount of sense then, but it probably makes more sense now. If I were an economist, I would try to understand why the contemporary economy isn't one big Ponzi scheme (if, in fact, it isn't). I have no conception of how the world has managed to maintain any semblance of economic coherence when so little of it is based on the production of tangible commodities that have genuine value. So much of it is simply converting currency or money from one form of exchange to another. And, that's the basis of much of the wealth that has been generated by the financial world. Maybe if they're going to clamp down on that, it'll force things to shift to production. The latest I've heard is that since they can't do sub-prime loans for mortgages anymore, they're doing them on used cars. That's the new frontier.

Jerry: With electronic devices that can prevent them from starting if you don't make your payments.

AUTHORS MEET CRITICS:
*THE EMERGENCE OF
 ORGANIZATIONS AND
 MARKETS*
 BY JOHN F. PADGETT AND
 WALTER W. POWELL

Editors' Note: The following is an expanded version of a panel discussion held at ASA in August 2014.

*JAMES MAHONEY
 NORTHWESTERN UNIVERSITY*

In *The Emergence of Organizations and Markets*, John Padgett and Woody Powell outline an extremely important agenda: they seek to develop new tools for understanding and explaining the emergence of new organizational forms.

Explaining true novelty in organizations—or true novelty in anything else—is one of the more difficult but more worthy undertakings that social scientists can pursue. It is especially worthwhile if the pursuit is undertaken in conjunction with empirical analysis. And while the theory chapters of this book are weighty in their own right, most of the book consists of empirical chapters that seek to explain emergence across quite diverse substantive topics.

At the heart of the book is a new framework for analyzing the emergence of new organizational forms such as these. The framework combines insights from social network analysis with insights from biochemistry, especially the biochemistry idea of autocatalysis. This is a fresh synthesis. The complaint about network analysis has always been the complaint about structural approaches more generally: it lacks a mechanism of transformation. It is not good at explaining change, much less emergence. This book seeks to overcome this structuralist bias and

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thereby allow for the explanation of emergence.

The key move that this book makes is to appropriate ideas and concepts used to explain the origin of life in order to make better sense of the emergence of organizations and markets. The analogy is quite interesting, and it goes beyond previous efforts to use evolutionary ideas from biology for the explanation of organizations. If currently influential evolutionary approaches to organizations draw heavily from the discipline of biology, this book draws more heavily from the discipline of chemistry.

For this reader, there is good news and bad news to report about this synthesis of network theory and biochemistry. It is mostly good news. One core piece of good news is that the approach has inspired the authors to develop some quite interesting and quite useful mid-level mechanisms of organizational genesis. In particular, the list of eight mechanisms of organizational genesis in chapter 1 is extremely helpful. These eight mechanisms are presented on pp. 11-26, and they make up the heart of the usable part of theory. I am not going to discuss all eight of them, but focus on just three of them.

One mechanism is *transposition and refunctionality*. This mechanism is the movement of a practice from one domain to another, and its repurposing to fit into the new domain. This is innovation in the sense of “a new purpose for an old tool.” This is the most important mechanism in many of the empirical chapters of the book.

As an aside, this mechanism also appears to be the main mode of theory invention used by Padgett and Powell—that is, they are transposing existing ideas from chemistry into the domain of organizations and sociology.

In presenting this mechanism, the authors set an

agenda of research for others to take up. Some of the questions their framework inspires are the following. What kinds of agents are best at transposition and refunctionality? What kinds of organizations or environments are more likely (or less likely) to experience refunctionality. What are the normative implications attached to this mechanism? When will transposition help organizations meet their *goals* versus undermine their goals?

The next mechanism I want to discuss is called *incorporation and detachment*. This occurs when a part of one network is inserted into another network without detaching from its original network. You can think about this as two Venn diagrams that partially overlap. In fact, the book makes excellent use of just these kinds of Venn diagrams.

The agenda introduced by this mechanism in part involves exploring how learning and information dissemination occur in organizations. The mechanism suggests that once one network has partially penetrated another, it can spread new ideas to the penetrated network as well as bring back new ideas to its own network. What we need are hypotheses about the kinds of organizations that will allow for incorporation and detachment. Scholars need to ask: under what circumstances are we likely to see incorporation and detachment?

The book’s theory explicitly brings in ideas of power and conflict, as can be seen in the mechanism of *purge and mass mobilization*. With this mechanism, the upper ranks of hierarchies are purged, and the bottom tiers are raised up to take their place. Stalin did this with the Great Terror.

Here the movement of ideas and new organizational forms can occur within a given organization or network. New organization emerges by eliminating old forms of organization and allowing marginalized actors

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to remake the organization. It is a kind of revolution from within. The key initiating source of the change is actor who carries out the purge of the top. But really the key source of emergence is the marginalized actors who rise to the top after the purge. They bring the new organizational modes with them.

Again, this mechanism sets an agenda of research: What kinds of organizations are susceptible to purge and mass mobilization? Is it possible that purge and mass mobilization will end up reproducing prior organizational patterns? Said differently, when will purge and mass mobilization will produce higher degrees of invention and innovation?

As a reader, I had some more general questions that I wanted to ask the authors. One concerns the relationship between this book's theory and field theory. The diagrams in this book often specify domains that might be thought of as fields. For example, in the discussion of purge and mass mobilization, there is a diagram of the Great Terror. In the diagram, on p. 22, one field seems to be the economy and another is the Communist Party. How do the authors feel about situating their theory as a kind of field theory?

Second, the networks in the diagrams tend to break things down into domains such as political, kinship, economic, military, and religion. I imagine that the kinds of domains or networks that one thinks are important will be heavily influenced by other theoretical considerations, such as whether one is a Marxist or not. Does the theory in this book have any advice for telling us how to determine the relevant and most important domains in a given substantive area? Would it be possible for two scholars to whole heartedly embrace the approach of this book but completely disagree with one another about the sources of innovation and invention in the same empirical setting?

Third, I wondered if the authors would be willing to say something about the relationship between this book and the earlier Powell and DiMaggio edited book, *The New Institutionalism in Organizational Analysis* (1991). Is this book about emergence, whereas the earlier book was about stability and change? Does the new framework in this book have things to teach us about the issues explored in the earlier book?

For me, the bad news regarding the new book is that the material on biochemistry, including even the core concept of autocatalysis, is rough going for social scientists. Autocatalysis is a bit like the concept of complexity: it is an umbrella label for something very important, but also something very hard to pin down in any exact way. Getting a handle on the concept is a bit like holding a ball of mercury. The concept is formally defined on p. 8 as follows: "autocatalysis can be defined as a set of nodes and transformations in which all nodes are reconstructed through transformations among the nodes in the set." The definition is not bad or wrong, but it is just hard to wrap one's mind around it, in the same way that it is hard to wrap one's mind around many definitions of complexity. Crucially, one does not have to understand the biochemistry roots of this argument to appreciate the basic Padgett and Powell model of economic production. The model is basically as follows: Firms are containers of skills. Skills are rules. Skills change products into new products. Trade involves the movement of products through firms, which can change skills. This model is useful for understanding the coevolution and co-constitution of products and organizations.

Moreover, one certainly does not need to have any background in chemistry to use and apply many of the key tools offered in this book. I think the eight mechanisms in chapter one are the core of those tools. The next step for the rest of us will be to develop further generalizations about how those mecha-

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nisms work in certain settings to stimulate innovation, invention, and emergence in organizations.

KATHERINE STOVEL
UNIVERSITY OF WASHINGTON

The Emergence of Organizations and Markets is a fascinating and challenging book. Drawing inspiration from chemical models of autocatalysis, the bulk of the book presents a series of careful and dynamic analyses that trace how interlocking institutions can lead to reproduction, innovation, and invention in organizational form or substance. Unfortunately, the historical knowledge necessary to evaluate some of the case studies, and the biological vocabulary that provides the foundation for the modeling sections, are beyond the knowledge base of all but a few social scientists. Nevertheless, the book offers an exciting set of ideas, concepts, and examples that have the potential to push the study of networks and organizations in important directions. My comments in this short essay are intended to highlight several ideas that captured my imagination while reading this book, and to identify some of the more provocative threads that I believe merit additional development in subsequent research.

I begin with what I consider the book's mantra, "**In the short run, actors create relations; in the long run, relations create actors**" (p. 3). This insight, which can easily be traced to the relational sociology of Harrison White and his students, summarizes the powerful autocatalytic foundation for Padgett and Powell's approach to the study of the emergence of organizations and markets. The key insight here is that while actors meaningfully orient their behavior toward others, actors are, profoundly, the product of past relations—both those they may have personally been involved in, and other relations and systems of relations in which they and others are embedded. This reflects an important tra-

dition in sociology, if one that is missing from much of our contemporary scholarship.

In fact, a quick perusal of what passes for sociology in much of the discipline treats actors as endowed with sets of characteristics (attributes) which—in many cases—have no history and whose meaning is unproblematic. Of course a few branches of sociology emphasize the constructed nature of all social material; together, these two poles remind us of the old over- and under-socialized 'man' debate. And so Padgett and Powell, like Granovetter before them, bring networks to the rescue. Yet whereas Granovetter emphasized the consequences of variability in network density, Padgett and Powell, echoing White, emphasize the temporal dimension of the problem.

For Padgett and Powell's mantra to drive a vibrant research agenda, it is necessary to move beyond treating it as an assertion, and consider instead a series of contextually specific questions that can be empirically verified. The chapters in this book provide some nice illustrations of how to do this, though many of them are a bit less connected to the core insight than one might like. At a more collective level, we should also begin to pose a set of more general questions about the relationship between actors creating relations and relations creating actors. Perhaps one of the most obvious questions is, what sort of time scale constitutes the short run, and what is the long run? Does the appropriate time horizon vary by setting, or situation? More sociologically, we must consider what we mean by relations creating actors. How do we know when actors are changed by their network? Most contemporary network methods still focus on measuring the presence and absence of ties, and these methods are quite poor at capturing changes in the salience or meaning attributed to interactions or relationships. (At the same time, if we simply choose to

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impute changes in meaning/value/salience as a result of change in structure at some aggregate level, we may miss the processes by which cognition and symbolic communication actually change.) More qualitative strategies for understanding values, aspirations, and orientations might help, though such methods have proven difficult to effectively integrate with network structure in the cross-section, let alone over time.

My second observation is that the book offers a **network version of Weber**, in the sense that it emphasizes the transformative consequences of the intersection between spheres or domains of social life. Yet where Weber defined spheres of life substantively, here domains are reflected in (often self-sustaining) networks. In both approaches, an important source of organizational transformation is the collision between different spheres, collisions that may lead to adaptation, to importation, to inclusion, to homology, and so on. Most centrally for Weber, and for much of this book, is the essential feedback between political and economic activities, though the chapters organized by Powell expand this to include the modern educational realm.

Embedded in this insight is the notion that spheres (or domains, or networks) when stable may have a 'logic' and that interaction across spheres frequently interrupts the existing logic. Of course this language is not the language of Padgett or Powell; rather, it is the language much more familiar to students of organizations and institutions. And yet it seems that it is imperative to continue to specify, in particular contexts, how network structures generate and reproduce logics—where logics may be both material and symbolic. By carefully specifying the relationship between networks and logics, then we might begin to think more systematically about what happens when particular domains collide (and why some domains are likely to collide).

Some issues to consider on this topic: First, how central is the symbolic content associated with a domain (or a network)? In human systems collisions frequently trigger efforts to repair or replace the symbolic capital of networks—a process that no doubt impacts the sort of actors the network produces. So it seems that we need to attend to how these intersections of spheres impact networks at the symbolic or linguistic level as well as at a more material level. And second, are domains really that distinct in practice? As Padgett has previously helped us all appreciate, actual relations and institutions are rarely cleanly situated in one Weberian domain. When relations are multivalent, opportunities for borrowing and transposition may abound. However, the imperative of theory is that we offer more than a laundry list of possible mechanisms, and rather specify (or even predict?!) likely consequences of particular sorts of intersections. One way forward might be attempting to link particular logics with mechanisms as introduced in the book. For instance, networks that sustain a logic of complementarity may contain the sort of anchoring brokers that facilitate innovative, rather than transformative, borrowing.

Building on the idea that there is further room to theorize the conditions under which particular mechanisms operate, it strikes me that there are also opportunities to identify (possible) affinities between specific contextual or network/structural characteristics and particular mechanisms. In the opening chapter, Padgett and Powell briefly note that certain network structures might be more vulnerable to change than other structures, but they do not take the next step and consider how types of structural vulnerability might intersect with particular types of mechanisms. The closest Padgett and Powell come to explicitly linking network structure with a specific mechanism of origin is in chapters 9 and 10, which document transformations in the Communist party in Russia and China. In each in-

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stance, the crucial network feature is a dual hierarchy that facilitates the process of purge and subsequent mass mobilization. Yet it seems there is great potential for further development of the relationship between other network features and mechanisms of change.

Returning to the issue of how symbolic goods play a role in emergence, I found the book's emphasis on **categorization** to be particularly significant though still somewhat underdeveloped. It is well recognized that in relatively stable systems, shared approaches to categorization and classification are crucial for regularly getting things done (for instance, overlapping categorization schemes allow actors to find trading partners). In chemical systems producing shared categorization schemes is relatively unproblematic, since physical structures of molecules dominate. Yet classification and categorization are more complex in social systems where they involve cognition and language, phenomena that are less disciplined by material demands than in chemistry. Subtle (or not-so-subtle) shifts in classificatory rules within a population of actors may shift the value of particular inputs (or outputs), a mechanism that may well turn out to be the link between actors making relations and relations making actors. When commonly accepted categorization breaks down—often through endogenous drift or collision with other networks—the emergence of new forms is more likely. This process is nicely demonstrated in the series of chapters about the emergence of the biotech field, where resolution of classificatory incoherence differentiated regions in which biotech emerged from those where it did not. Yet because a key difference between chemical reproduction and social reproduction is symbolic language, it is imperative that we focus our microscopes on how symbolic shifts occur, and when they have transformative capacity.

Finally, I would like to briefly discuss the relationship between Padgett and Powell's project and the **analytical sociology** work spearheaded by Peter Hedström. Both of these approaches rely on mechanisms and agent-based models, but with very different orientations. For Hedström and his followers, mechanisms are by definition specified at the micro/individual level, whereas the mechanisms identified by Padgett and Powell operate at the network, or meso-level. This makes sense, as analytical sociology tends to embrace the methodologically individualistic contention that explanatory accounts must make sense in terms of individuals' motivations. And yet if actors (and, presumably, their motivations) are fungible, then insisting on anchoring causality in actors' motivation may miss the important action.

Similarly, the role of agent-based models differs greatly between these two approaches. Whereas Padgett builds small and highly stylized models that emphasize the consequences of structure and interaction rules, Hedström's newer efforts at agent-based modeling rely on population-level registration data that contains variable-like data on masses of individuals. At its best, this latter approach allows analysts to describe mechanisms that are consistent with macro-level patterns, though it sheds little light on how the mechanism operates—let alone why one social arrangement might break down or be replaced by another. Padgett's work, in contrast, follows the model put forth famously in Schelling's tipping model (and further developed in the complex systems world), whereby analysis of the dynamics of a simple interaction model can yield great insight the emergence of new and stable patterns.

While the differences in approach are striking, I worry that both rest on a laundry list of mechanisms generated in a rather *ad hoc* way from case study. Very little attention is paid to how mechanisms re-

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late to one another, when a particular mechanism will become operative, or if there are key organizing principles (e.g., balance theory, hierarchy, or status orderings) that underlie the stabilizing or transformative effects of the family of mechanisms. In terms of the utility of agent-based models, there is great debate about how data intensive should agent-based models be. I am not convinced that models need be so rooted in detailed registration data, but I do think that while working within the complex systems framework it is imperative that all model object be well specified, and that the number of moving parts be tightly coupled to either theory or an empirical puzzle.

In summary, I view this as an important book that offers a needed corrective to the variable/attribute centered approach that dominates much of American sociology. That said, I think that the long-term impact of this book depends on the extent to which others find ways to extract and develop some of the powerful ideas embedded within the dense pages. Luckily there have been several engaging discussions of the book already, which provide an excellent resource for those seeking entrée into Padgett and Powell's way of thinking about organizational change. In order for these ideas to move beyond the 'trust me' phase, we need to focus on how to consistently operationalize the many concepts introduced here, and on how to measure relevant quantities precisely. For students looking for dissertations, I see great payoff in projects that will empirically evaluate some of the book's core insights across multiple contexts.

BRAYDEN KING
NORTHWESTERN UNIVERSITY

Reading this book, I was taken back to my junior year in college when I had organic chemistry in the mornings, one of the required classes for premed students.

In the afternoons I sat in classes for my sociology major, including a complex organizations seminar where I read for the first time DiMaggio and Powell (1983) and Padgett and Ansell (1993). The tug-of-war for my attention was no contest. Isomorphism and Florentine political intrigue pulled me over to their side with little resistance, and I subsequently dropped the awful idea of becoming a medical doctor and tossed organic chemistry aside. And so here I am a few years later, reading a book by two of the scholars who lured me away from the natural sciences and suddenly I'm in the world of chemistry again. I came over to their side to get away from chemistry and somehow it found me again!

Holding the authors in such high esteem, I approached this book and the criticisms I will make of it with a bit of trepidation. As I see it, this book is the product of careers' worth of thought, theorizing, and painstaking analysis. Padgett, Powell, and their collaborators deserve praise for producing a big book at a time when we see fewer and fewer books such as this in sociological research. And I mean "big" in both a figurative and literal sense. Anyone who has had to tote this densely-packed book along with them on summer road trips, like me, will know just what I mean. But it's also a book that grapples with big ideas—perhaps the biggest problem that faces organizational and political sociologists.

Most of our theories are quite good at predicting/explaining stability and reproduction, but the real mystery is where novelty comes from. Why and when do new organizational forms emerge? How do new institutional arrangements get created? The real strength of the book is reorienting our gaze to the early stage processes of organizational and institutional genesis—when new forms are created through recombination and the transformation of relations between actors. Despite the big question, the answer they provide is elegant. Individuals and

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organizations can be quite cognitively/culturally simple and still produce technological and organizational complexity due to simple rule and role switching across multiple networks and accessing rich environments that sustain multiple skill combinations. This view takes much of the invention out of the hands of the actors and into the process through which structural folds in overlapping roles/domains lead to recombinations and transformation of the nodes in a network.

The other strength of the book is the rich collection of case studies and the empirical diversity of those studies. Padgett, Powell, and their co-conspirators take us all over the globe and to different historical time periods to observe transformative moments. Examples of organizational genesis include the birth of Tuscan merchant banks out of Roman Catholic Church organization, the creation of the joint stock company during the Dutch revolution, the transformation of markets in post-Communist Russia, and the creation of hybrid life science joint ventures out of the university. If you thought you were simply getting a theoretical overview with no additional empirical analysis in this book, you thought wrong. The book's chapters are detailed and precise in their analytic approaches, assembling data in elaborate graphics, tables, and charts to illustrate the relational and organizational transformations at the heart of their stories. It's really a beautiful book to look at.

Now, let's turn to what I see as the major weaknesses of the book, the biggest of which is the analogy upon which the theoretical framework of the book is based. The book turns to chemistry for analogies to understand social life. This is an attempt to distance us from biological analogies that emphasize competition and selection but that do not offer much guidance in understanding the process of speciation, i.e., creating novel forms. The key concept is autocatalysis—the idea that change occurs

through a self-sustaining process of reactions among nodes in a network. In chemistry, autocatalysis applies to chain reactions among elements that come into contact, leading to changes in the very product that was a part of the initial chain reaction. In social life, Padgett and Powell define autocatalysis as the transformation that occurs to all nodes in a network due to changes among certain nodes in the set. Usually autocatalytic networks are characterized by self-repair but in certain situations, where nodes overlap with other networks or where nodes are put to new uses, autocatalysis leads to the transformation of the entire set and consequently to the birth of a new social life form.

As social scientists we often use analogies from biology or chemistry to clarify and to focus our attention on processes and dynamics that would otherwise go unobserved. I asked myself two questions as I read this book: 1) is this a useful analogy for clarifying the creation of novelty? And 2) can we create from this analogy a more general theoretical framework about the origin of social life/novelty? I'm skeptical that the analogy of autocatalysis does either very well.

The analogy doesn't clarify. Instead it obscures the very processes they seek to understand. Once we get past the initial definition of autocatalysis, the book introduces a flurry of concepts, only a few of which seem directly tied to autocatalysis: structural folding, transposing, migration, and of course the more common concepts from social network analysis. If you've followed the works of these authors, or that of David Stark, you're probably already familiar with many of these concepts. The analogy of autocatalysis bears a heavy burden in trying to unify all of these concepts in an overarching framework. In all of the cases, especially the empirical chapters about biotech and life science firms, it was not apparent what value the analogy added. At times I felt

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like I was reading two books, and perhaps this reflects some tension in the writing process as well. The first book takes autocatalysis quite seriously and tries to theorize it as an actual process that we can observe directly in social life, and the other book is really interested in the mechanisms whereby novelty emerges. In these mechanisms-focused chapters, the concept of autocatalysis seems almost copied-and-pasted into arguments, rather than being their source of argumentation. Perhaps this added-on appearance reflects a more fundamental problem with the analogy. We don't need it, and it gets in the way of the analyses themselves.

Second, does autocatalysis generate new theoretical expectations or mechanisms for understanding the emergence of social life? I would say that it does not. Once we move down a level of analysis to the actions of the nodes themselves, the language of chemistry becomes pretty useless. One reason for this is that humans are not chemical elements; they are thinking, feeling actors. Autocatalysis does not generate a particular hypothesis about when nodes transform and when they do not. We need something more to explain why and when to expect novelty.

And this is where mechanisms come in. The book lists eight of them, but there is no reason to think that we should be limited to just eight. As I understand it, mechanisms provide a way to bring energy to autocatalyzing systems. They are the node-level actions that inject a system in stasis with new energy that leads to transformation across nodes. To the point of the book, mechanisms are where genesis and novelty creation occurs. But the mechanisms don't follow logically from autocatalysis; rather, they are unique to empirical situations and vary by context. Some, like robust action, are derived from the existing literature on organizational genesis and others the authors arrive at inductively. The mechanisms end up being the primary causal explanations of novelty in their

narratives. The problem with relying on mechanisms is that it doesn't really add up to a theory. Is a theory based on mechanisms really a theory at all?

I was struck throughout the book with the similarities to Charles Tilly's project of explaining change in political actors. Like Tilly, Padgett and Powell draw on network analysis to explain how identities transformed over time, leading to new kinds of actors and action repertoires. We can see some similarities to his accounts of the creation of new types of political actors—e.g., revolutionaries turned statesmen. Like Padgett's story about the creation of new types of economic exchange and politics in Florentine markets, Tilly saw similar changes in political repertoires in both Great Britain and France and claimed that they were the result of reconfiguring relationships into new network forms.

At the end of his career, Tilly weaned himself from overly-structural theoretical arguments about changes in actions, which led to his embracing of mechanisms. Tilly's *Dynamics of Contention* book with McAdam and Tarrow (2001) is illustrative of this approach. I would offer the same criticism of Padgett and Powell's book that many people at the time made of the dynamics of contention approach. Although identifying mechanisms is important to theory development, they are not by themselves a theory of anything, especially when they emerge inductively from the examination of historical case studies. Each historical case study seems to require a different set of mechanisms to explain how/why autocatalysis happened. Mechanisms may be universal but they are apparently limitless in number. How can we create a real theory of autocatalysis when it occurs through so many pathways or is contingent on so many different mechanisms?

A more fruitful approach, perhaps, would be to begin with a different set of premises. It is possible

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that we could arrive at the same mechanisms if we started with a bottom-up theory of novelty creation that took more seriously the human mind, motivations, interests, and struggles for power and status. I was surprised at how many times, as I read their chapters, these sorts of issues lingered under the surface. This more bottom-up, human approach wouldn't necessarily neglect the role of relations, but rather it would put the actor more squarely in the middle of creating and reconfiguring those relations.

Autocatalysis is attractive because it allows for the possibility of individual actors as an element in change and stabilization processes, but without having to carry over any of the baggage of psychology, decision-making, or emotion that distinguishes human actors from chemical compounds. Nevertheless, inevitably when we begin reconstructing stories about how a particular historical case unfolded we can't resist returning to the human-like properties that actors in these stories exhibit and inevitably shed some of the uncomfortable stiffness of the chemical analogy.

For example, consider the mechanisms of refunctionality, conflict displacement, and incorporation. All of them depend to a certain degree on the calculations and motivations of the actors involved, the need to consolidate power and to maintain one's status position. The mechanisms derive from human and collective motivations to dominate, or at least to not be dominated by another group. The mechanisms do not derive from the process of autocatalysis as much as they are the transforming energy that ignites a change in a set of nodes. But without an understanding of the psychology and group dynamics of the nodes, you would never understand why in these situations, the nodes (read: humans) chose the particular strategy of action that they did.

I would like to take the theoretical machinery from

this book as it describes actors as concatenations and retheorize it from the bottom up. On a more micro-level, I think there is more to be gained from incorporating the human mindset and passions into the creation of novelty. Consider the work of literary theorist Harold Bloom (1973; 1975), who I have always considered a sort of network theorist due to his emphasis on relations among literary figures. He argued that novelty stems from misreadings of past works of important literary figures. Misreading involves, first, borrowing from a predecessor—taking an idea that resonated in some way with your own understanding of the world—and second, reappropriating that idea, or willfully misinterpreting it, as a way to set yourself apart from your peers and predecessors. Through misreading, authors and poets both build on their literary forbearers but also distinguish themselves from those forbearers, and if the misreading is drastic enough, create something entirely novel. There is no biology or chemistry in this explanation at all, but yet it is squarely focused on how motivations and relations are intertwined and continually transform one another. In some cases, the motivation leads to an intended outcome, but in most cases novelty is an unintended byproduct of a local struggle with one's peers and predecessors.

Let me end by praising the book's emphasis on novelty. To me creating something novel is at the heart of innovation and ultimately invention. I think one of the biggest takeaways from this book is to challenge us to consider new methods and theories for studying the creation of novelty. Padgett and Powell set us on the right path for uncovering new analytic and methodological tools for understanding this important outcome. Despite my misgivings about the chemical analogy, the weight of this big idea book will make it an influential tome in building a sociological understanding of novelty.

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AUTHORS' RESPONSES

WALTER W. POWELL
STANFORD UNIVERSITY

I want to thank Kate, Jim, and Brayden for their thoughtful and thorough remarks, which are much appreciated. We also want to thank the audience, which has turned out in large numbers at 2:30 at the last session on the last day. This is quite gratifying. Now, Kate refers to the book's argument as a network version of Max Weber, and Brayden compares the book to Charles Tilly's efforts in *Contentious Politics*. I am half-tempted to say thank you, and let's all go for a beer. That is very nice company to be in.

One of the questions asked by Brayden, as well as many others, is why chemistry? Why did we turn to chemistry for assistance in thinking about novelty? Can't we use ideas directly from sociology or literary theory? At the outset, fourteen years ago, we did not have our sights set on chemistry. We began a multi-year search reading a wide range of disciplines to see how scholars in different fields thought about the production of novelty. The "we" included John and myself, of course, but many others participated in our workshops at the Santa Fe Institute—Charles Sabel, David Stark, Doug White, Brian Uzzi, Bruce Kogut, Julia Adams, Lis Clemens, and Dan Carpenter, to name only a few. We also included many of our current and former students, and we were fortunate that Walter Fontana, Doug Erwin and Sanjay Jain, fellows at the SFI, joined with us.

There were many possible candidates. We read work

in science and technology studies, most notably Peter Gallison's powerful *Image and Logic*, and related work on boundary objects. We looked at evolutionary game theory, as well as the so-called new Schumpeterian economics. There were numerous people at Santa Fe interested in power laws and the intersection of physics and computational social science, so that work received our attention. There was also emerging work in evolutionary and developmental biology. As we read these various texts and discussed them at great length, we looked for ideas that were fertile. John and I had a mutual commitment to pico-level historical data, and the close analysis of biographies and careers. For us, biography is a structure producing mapping. Some of you will notice that the book is dedicated to Harrison White, and some of Harrison's best work drew on polymer chemistry, especially his ideas about wheeling and annealing. So work in chemistry on the origins of life had considerable appeal.

Our core theoretical commitment was to multiple networks. We simply are not the people that most of our theories suggest; people are bundles of different interests and identities, which change at different points in time and in different places. Most social scientists adopt an interest-based or identity-based view of the world. But people are multi-functional concatenations of different roles, which are often conflicting. Roles have interests and roles have identities, but we have to see people as bundles of divergent interests and identities, from which they toggle back and forth. If we see people as mixtures of roles and purposes at different times and spaces, that leads to analyzing multiple networks and their folding, rewiring, and disbanding through time.

So for us, autocatalysis is not chemistry, it is life, and it is fundamentally social. Autocatalysis helps us with our larger theoretical ambition that we are pur-

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suings in our continuing work—a general theory of development that operates at multiple levels and has different rules, speciation, and selection at those different levels.

Some of you may have noted that the cover of the book is a photograph of a cross-section of fossilized stromatolites. These were bacterial colonies formed 3.8 billion years ago, not long after the earth cooled. Stromatolites were the first life form, and are the earliest physical record we have of the origins of life. They were created out of a unique combination of an acidic ocean, a cooling earth, and mineral formations of serpentine structures from hydrothermal vents, which created a reactive environment where nascent RNA formed and life began. For us, the problem of emergence requires a focus on when flows of different elements intersect. The core question, then, is when do flows of networks become self-reinforcing or self-reproducing? Catalysis makes a project happen faster. Autocatalysis suppresses the noise of the surroundings, and more catalysts are created. This chemical view that we transport into the social world led us to think about how the coupling of roles in one domain reproduces relations in another, and to ask when the breakdown of authority in one domain might trigger change in another.

Kate Stovel asks a very good question, “How do we know when actors are changed by their networks, and how do we study this?” The mantra of the book is, of course, in the short run actors make relations, but in the long run, relations make actors. At the core of this view, which is fundamentally autocatalytic, is the idea of the network construction of persons through their biographies. We are searching for the transformative consequences of the intercalation of different spheres of life. This leads to an entirely different view of networks, not only as pipes and prisms, but as things that do transformational work. In this sense, what we are looking for—biography,

politics, culture, social influences, the economy—is what passes through networks. Movement, not variable-centered frozen attributes, but networks through time.

How do we know when actors are changed by their networks? In John’s and my joint work, we had these remarkable moments in which we saw similar events, seven centuries apart, representing this kind of flow. In Renaissance Florence, as families tried to cement relations with rivals, they did so through the exchange of daughters and sons-in-law. We found the same phenomena in the contemporary life sciences, as molecular biology developed in its early days. Senior scientists traded graduate students and post-docs, cementing research programs and particular kinds of approaches. Similarly, we found compelling evidence in archival materials. A wonderful example came in comparing letters of credit from the early 1400s with licensing letters written in the early 1970s. I won’t do the long quotations here, but a short version is illustrative. A Florentine letter typically went, “Mio caro amico, because we have so many friendship, economic, and family ties in common, let me give you this loan as a gift. Perhaps down the road we can even become brothers and form a partnership.” (A gift here did not mean “free;” it meant business as reciprocal gift-exchange. See Padgett and McLean, *Journal of Modern History*, 2011, for more details and evidence.) A comparable letter from the Stanford University Office of Technology Licensing to a Bay Area startup biotech firm would read, “My Dear Colleague, Because of the many scientific and personal relationships between scientists at our university and your company, we do not believe it feasible to license this new recombinant gene technology to you. Instead, we propose to allow you to use it for free, but in the event a new medicine is eventually developed, we would ask for 3% of the royalties from that product.” (See J. Colyvas and W. Powell, “Roads to Institutional-

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zation,” *Research in Organizational Behavior*, 2006, for more details and evidence.) In both cases, such letters were very exclusive. A standard business letter would be sent, for example, to an established chemical or pharmaceutical company, asking for an annual payment.

In both Renaissance Florence and the early days of Silicon valley, the realms of social relations—family and academe, were repurposed into business relations, transforming the business, AND eventually flowing back to transform both the family and university science. Seeing these letters side by side, five hundred and fifty years apart, was quite an extraordinary moment. But it is not only multiple network data or archival data that can answer the question of when people are transformed by their network relations. Mario Small’s ethnography (*Unanticipated Gains*) of day care centers and hair dressers suggests how acquaintances get re-purposed to take on the roles of family members, and in so doing such crossings alter the character of hair dressing salons and day care centers in inner cities.

Jim Mahoney asked about the relationship of this project to my ‘orange’ book, *The New Institutionalism in Organizational Analysis*, that Paul DiMaggio and I did back in the early 1990s. More generally, many people have asked about the relationship of our work to field theory. In a very important sense, the Powell and DiMaggio book, along with Theda Skocpol and Peter Evans, *Bringing the State Back In*, were exemplars for John and me. Both of those books defined a research program, set an agenda for future scholarship, and have had healthy audiences. We aspired to do something comparable. But our new book is quite different from *The New Institutionalism*, and in some respects from field theory as well. The imagery of field theory is very much one of force fields from physics, and it carries a strong sense of alignment. You see this imagery when Bourdieu talks about a social field as like

a football field, or the pitch, or when Fligstein and McAdam talk about fields with the analogy of nested Russian dolls. Our project is different, although we appreciate very much the insights from these scholars and they were among the materials we read in our search. (As one illustration, Bourdieu’s notion of the habitus, or embodiment, has at its core a social learning model that suggests mastery of a small set of principles. He talks a lot about how skill is inscribed in play, and his image of European football is apt. If these skilled players had to think about what they were doing, it would disrupt the game. That is a beautiful illustration of flow.) Our project is constructivist too, but from the bottom up, not fixed things but things that are changing. We’re interested in how micro-level interactions generate a sub-strata that is independent of its micro-origins. So rather than see networks like physical networks, and as fixed, restrictive forces, we want to think in terms of networks of possibilities. The term that Walter Fontana and others at SFI use is evolvability. Thus inconsistencies or cross-purposes are important for us. We are also much more mindful of how much innovation comes from people trying to hang on to what they have. Perhaps I learned this insight from John, it comes from a famous Italian novel by Giuseppe Tomasi di Lampedusa, *The Leopard*, which has a central theme that if we want things to stay the same, we have to change. So we too are constructivists who think about the social construction of persons, of categories of actors, and habits of mind. Rather than seeing domains as set and fixed, and institutions as top-down forces, we follow network flows to point us to which domains are the necessary objects of study.

Jim Mahoney likes our idea about the topology of the possible, but he wants to know what kinds of things can be recombined. That’s a great question, one I have spent years thinking about. One way I approach it is to think about what considerations

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never appear on the table. So if we go back to the 1970s and 1980s and the dawn of the molecular biology revolution that created the biotech industry and the eventual transformation in both the pharmaceutical industry and university science, there were a number of organizational models that you don't see in the historical record. No one talked about turning the university into a factory for mass production of monoclonal antibodies. The older model of Bell Labs—that is of a large firm having an autonomous R&D unit—seems to have become discredited. No U.S. firms thought very deeply about this. Few hospitals were willing to take on the task of becoming research-driven entities. And at the time, none of the early venture capital firms imagined themselves as incubators. So the creation of the small science-based start-up firm—with a campus-like atmosphere and some modicum of freedom for scientists to explore, which many of you will recognize as now typical of startups in all fields today, was an unexpected result of amphibious scientists hedging their bets by keeping one foot in the academy and the other in this novel, risky world creating new kinds of companies. Our approach leads us to focus on these amphibians, who travel between different domains, and can reshape extant organizational forms for new purposes. The agenda, for both John and me, is to analyze these rare moments when border-crossings rebound to transform their domains of origin.

Let me move more quickly to several of the other comments.

Jim asked what kinds of agents are best at transposition and refunctionality. Can we develop any hypotheses about when transposition will help organizations meet their goals versus undermine their goals? In current work, Kjersten Whittington and I are trying to think about what kinds of organizations can be anchor tenants, and whether such anchors are always benevolent or whether they can be malevo-

lent. We are also interested in what types of people are likely to be amphibians. In the scientific world we find that there are several kinds, either high-status university scientists, younger foreign scholars educated in the US, or frustrated middle managers in mainstream technology companies. What is common across them is they have very different time horizons than do their peers. I have started a project with Kathia Serrano-Velarde (University of Heidelberg) looking at the flow of academic scientists from computational social science fields into the social media industry. This seems to be a case of transposition and detachment at the same time, as their linkages back to the academy are being severed.

Several of you asked about our list of mechanisms and I take your question to be: by what principle is our list coherent? Is it exhaustive? Here I plead exhaustion rather than exhaustive. These ideas emerged from many, many years of work. Is it a complete list? Of course not. And perhaps it is even too long, as several might be combined. What we are trying for is a way of understanding the various processes by which multiple views can become stapled together, to offer an explanation that is adequate at the level of human meaning. Perhaps people would find the word process more palatable than mechanism, as the latter raises questions about our connection to Peter Hedström, James Coleman et al and more instrumental conceptions of human agency.

I want to close with a suggestion for the many younger researchers in the audience. One simple little idea that John and I often emphasize is that we need much more attention to verbs, rather than nouns. Most social science thinks about nouns, fixed things that you can attach a label to. Rather than labeling people, products, or institutions, we want to encourage people to use verbs and ask how these things come into being. Where do categories of

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thought and categories of actors come from? More attention to flows, we believe, will deepen and enrich social science.

JOHN PADGETT
UNIVERSITY OF CHICAGO

Like Woody, I want to begin by sincerely thanking our three commentators-cum-critics, Brayden King, Jim Mahoney, and Kate Stovel, for their engaged and constructive reflection on our work. It is gratifying to see such thoughtful people take ideas seriously and appreciatively, whether or not they agree with our conclusions. All three of them have noted that ours was a “big book” in more than one sense. On the one hand, it is almost 600 pages, oversized with double columns, physically heavy even in paperback because of the care that Princeton University Press put into reproducing our 108 color diagrams. On the second hand, the range of topics covered in our book is almost ridiculous: (a) three chapters on the origins of life on earth, including simple chemistry models by us and others about that process; (b) four chapters on the emergence of capitalism and state formation in Europe, focusing on the cases of Italy, Netherlands and Germany; (c) four chapters on the fall of Communism in the Soviet Union and China, and post-Communist reconstruction in Russia and Hungary; and (d) six chapters on contemporary Silicon Valley, biotechnology and the life sciences. Scott Boorman in his review indeed called our book four books in one. And finally, it is “big” in the sense of trying to develop theory about a phenomenon not much analyzed or even discussed in the social sciences—namely, the emergence of novelty, in particular the emergence of novelty in “actors,” be those people, organizations, markets or states. The task assigned to our three reviewers, in other words, was not a simple or an easy one. They deeply deserve the thanks they receive from Woody and me.

The comments of the three critics are not the same, but they overlap and are compatible in many ways. Rather than create redundancy by replying to each of the critics separately, I will proceed in my response by abstracting four questions that I think they all share, even though they emphasize different ones: (1) Why chemistry?; (2) Where is agency?; (3) Where is culture?; and (4) How to turn all this into researchable normal science? My reply will be organized into these categories.

Why Chemistry?

[the question most emphasized by Brayden King]

Chemistry—and in particular the chemistry-based idea of autocatalysis—is used in this book in four ways: as a metaphor, as a formal model, as one-half of the answer to the question of the emergence of novelty, and as a theoretical framework for organizing our empirical work on historically dynamic networks and biographies.

As metaphor, I would insist that the contribution of “chemistry” to our book is profound: it deconstructs apparently solid objects into reproductive flows. In my talks, but not in the book, I often use the example of my nose. To me my nose appears solid and stable enough. But to a chemist my nose wasn’t there a few years ago. Every cell and atom in it has died and been flushed in that time, only to be replaced and reconstructed afresh by new cells and atoms. Why does my nose seem the same in spite of the underlying chemical reality of its continual flux? Because it is an autocatalytic system, that’s why, whose nodes in interaction (and not only nodes within the nose) reproduce the nodes. Autocatalysis is the chemical definition of life¹.

Like chemists, we recommend that social structures be conceptualized processually as regenerative vortices through time. In saying this, we are saying

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nothing more than that social systems are a form of life and should be recognized as such. Of course, social systems are more complicated in all sorts of ways than amoeba. We are not denying that obvious truism.² But at the existential level of understanding why social systems exist at all, it is more insightful as a first cut (I claim) to contemplate what we have in common with lowly amoeba than to fixate egotistically on how “superior” we like to think of ourselves as being. More narrowly on the point of understanding novelty, a number of our critics have pointed out that autocatalysis by itself is insufficient for explaining novelty, even in our own empirical cases. That observation is correct, but that is not our argument. Our argument is that autocatalysis and multiple networks together are necessary to understand the emergence of novelty. Neither alone is sufficient; both, working together, are necessary. In our theory and in all of our cases, novelty at the level of invention³ is produced by transpositions and recombinations of multiple networks. “Evolution” in our framework is not the recombination and selection of genes (or pseudo-genes like “memes”), as it would be in sociobiology. It is the recombination and selection of networks⁴—more specifically of the relational practices that comprise and generate networks. Where does autocatalysis fit into this multiple-network story? Multiple networks in the traditional SNA approach are too static; there is no motor driving reproduction, much less evolution, in an exclusively topological analysis. For us autocatalysis is that requisite motor. “Multiple networks” for us are coarse-grained representations of multiple autocatalytic systems, which overlay and interpenetrate one another. (Perhaps more specifically, networks are the historical residues or “reifications” of prior autocatalyses that have been inscribed into the “memory” of the present.) Therefore when we say “transposition and recombination of multiple networks,” that is just our short-hand way of saying “transposition and recombination of multiple auto-

catalytic systems.”

The fact that each autocatalysis by itself leads to reproduction and stability, not to novelty, explains why the combination of internally self-regulating systems, when they become forced into contradiction or ambiguity through permutation (“historical contingency”) frequently generate episodic or punctuated change⁵—just as Stephen Jay Gould argued long ago. I appreciate Kate Stovel mentioning my formal agent-based models of production autocatalysis in chapter 3 of the book. Not too many sociologists are going to zero in on that. I take as a great compliment her comparison of my models of autocatalysis to the tipping model of Schelling, for indeed, quite similar to Schelling, my motivation for modeling is not to mimic reality, which for me means Florence—a goal I eschew because I know too much about Florence to insult her like that. Rather the purpose of modeling is to develop stylized logic machines that are capable of generating implications that were not intuitively obvious to their author. Examples in that chapter were my models’ conclusions/hypotheses about the evolution of altruism as autocatalytic repair and about the impact of stigmergy (feedback between social networks and the physical environment) on the evolution of selfishness. For present purposes, the most pertinent derivation from those models was that autocatalysis itself evolves toward multiple networks as chemistries become more complicated (namely, as transformational interaction possibilities increase). Out of a primordial soup of increasingly diverse interactions, multiple overlapping autocatalytic systems (a.k.a. multiple networks) emerged and differentiated in my agent-based models, even as they overlaid each other and stayed linked at multiple junctures. Perhaps others before me have concluded this in different language, but I would like to be remembered in part as someone who derived Durkheim’s “differentiation of domains” simply out of chemistry.⁶

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Brayden asks “why chemistry? why not literary theory?” or some other “more human” version of social constructivism. The comparative advantage of chemistry as a metaphor is that it immediately grants one access to a powerful and deep set of findings and models, at the cutting edge of science today, which one can use to help develop testable hypotheses about generative process and (evo-devo network style) evolution. But in no way am I opposed to literary theory. If literary theory can deliver payoffs like that, I say “bring it on.” Pragmatically I am all ears; insights can come from anywhere. The problem in the social sciences is simply that I don’t see many (any?) tools for addressing (or even asking?) Woody’s and my core question about the emergence of novelty. Hence one is forced farther afield, like chemistry or literary theory. Until literary theory comes through to deliver the empirical bacon, however, I will continue to plumb for insight potential homologues between biochemical processes of classification and hybridity and social-science processes of cognition and multivocality.

Where is agency?

[the most common question I have received from many, many sources]

Our answer to this question is always our mantra: In the short run, actors create relations; in the long run, relations create actors.⁷ In other words, in any short-term time frame where individual actors can be presumed to stay fixed, Powell and I are methodological individualists—albeit more of Simon’s “bounded rationality” variety. Since most of the social-science literature is methodological individualism, however, we choose to emphasize the longer-term side of this intertemporal feedback across multiple time scales, where our theory is more original. To study novelty within the conceptual frame of life is to yank our individualistic minds out of their naturally egocentric gestalts toward the larger chain reactions of (transformational) flows into which all of our (heterogeneous) minds are

linked. Our empirical case studies are littered with people who made a difference—Stalin, Mao, Bismarck, Cosimo de Medici, Deng Xiaoping, even Pope Urban IV (you’ve never heard of this last guy, but I guarantee that he too made a difference). Some might even say that our case selection is in fact biased toward “Great Men.” To lower one’s voice and intone the chant of AGENCY, however, is to completely miss the central point of our case studies. No matter how shrewd these historically important actors were—and unquestionably all of them were as smart as they come—the complexity of the systems in which they were enmeshed vastly exceeded their comprehension, much less their control.⁸ For every success we can cite in their biographies, we can and do cite failures.

Two points are crucial in all of our case studies: (1) The consequentiality of “agency” lays not at the node of action/choice but downstream in the chain of reactions that unfolded from that choice. In our cases, the particular feature that over and over again made these chain-reactions both consequential and unpredictable at the same time was the catalysis of new interests and actors downstream, nonexistent at the moment of choice.⁹ (2) The historical sources of any real actor’s “agency”—that is, of any real actor’s motivations, alternatives, and cognitive conceptions—do not come from our own imaginaries as analysts. They come from that person’s learning within his or her own biography. Since that person’s biography was constructed in turn by the social networks that reproduced through him or her, the history of the evolving system is itself inscribed into the micro as well as macro forces of its own transformation. All pieces for novelty and change are there in the path dependent present; the almost unfathomable trick is how do they fit together, feedback, recombine, and tip through their interdependence.

Thus I respond to Brayden’s plea for holding on to

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the human in the following way: You misunderstand us in thinking that we wish to abolish the human, turning everything into chemistry instead. That is far too literal a reading of what we are up to. In fact we want not to eliminate agency at all but to endogenize actors—by situating their emergence and evolution within learning from their own histories (both macro and micro). In other words, we want to open up the solid-object black box of agency, to look inside and to see how its components are moving through time, thereby constructing the “objects” we call actors, both at the time scale of biographical time and at the time scale of historical time. History is not separate from individuals; history works through and within individuals.¹⁰ What does this theoretical perspective imply for our particular operationalization of agency? Consistent with our theory of three types of socially distributed autocatalysis¹¹ flowing through people, thereby bringing them to life, our “actors” are conceptualized as composite sets of practices of three types: (a) production rules or skills, (b) relational protocols of how to form ties, and (c) linguistic-cum-cognitive categories or symbols. The main things left out of this characterization are purposes or goals

We make two points about that addendum—(1) goals are features of roles, not of individuals,¹² and (2) goals are our conscious (and thereby our limited) perceptions of the paths we are on. Thus at the most micro level we are “practice theorists”—in alliance, as far as micro foundations go, with Bourdieu and with Dewey-esque pragmatists. Our main complaints about these fellow travelers are that Bourdieu is too top-down when he turns to causality¹³ and that pragmatists are so transfixed by creativity and flux as to be inattentive to macro stability. Were these weaknesses to be fixed, however, there would be much room for fruitful dialogue, which we welcome, between them and our own network autocatalytic approach.

Where is culture?

[the question most emphasized by Kate Stovel]

On this criticism, I mostly plead guilty. “Linguistic autocatalysis” is how our framework conceptualizes that multivalent (to the point of being vague) word “culture.” This way of approaching culture emphasizes the living reproduction and reconstruction of words through conversation and action, and implies considerable fluidity and lability of language (and by implication conscious cognition) in active use. At the level of theory, in other words, we are open, not closed, to the topic of culture—especially when that can be represented empirically by semantic networks that can evolve.¹⁴ The reason for the relative lack of delivery, in the Padgett and Powell book, on this side of our theory is that linguistic change was not empirically observed to be an important causal driver in any of our case examples of organizational emergence, no matter how frequently linguistic change appeared as a lagging correlate.¹⁵ In our cases, transposition and recombinations of biographies consistently seemed to be more consequential for organizational emergence than did transposition and recombinations of words.

That does not mean that other cases could not be found that illustrate better the leading, not the lagging, causal role of linguistic autocatalysis. Bill Sewell in particular has been persuasive in tracing the causal impact of linguistic autocatalysis¹⁶ in driving the French Revolution. We simply need more cases like that to help us better to make the connection between linguistic autocatalysis and production and biographical autocatalyses. In the meantime, I have an agent-based-modeling project (with Jon Atwell at the University of Michigan) to model and explore the early evolution of communication and language—mostly at the level of social insects and animals—within autocatalysis models of production and biography. I welcome collaboration on this important outstanding issue.

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How to turn all this into researchable normal science?
[the question raised mostly by Jim Mahoney]

In the empirical cases in the volume, eight cross-network mechanisms of organizational genesis were discovered inductively: (1) transposition and refunctionalization [Renaissance Florence and contemporary biotech], (2) anchoring diversity [life-science industrial districts], (3) incorporation and detachment [medieval Tuscany], (4) migration and homology [early-modern Netherlands], (5) conflict displacement and dual inclusion [nineteenth-century Germany], (6) purge and mass mobilization [Communist Soviet Union and China], (7) privatization and business groups [post-communist Russia and Hungary], and (8) robust action and multivocality [Cosimo de' Medici and Deng Xiaoping]. Jim Mahoney found this to be the best and most useful part of the book; Brayden King complained that a list of mechanisms does not a theory make.

Mostly Mahoney urges us to take the next normal-science steps. Understandably he wants to know when our various organizational-genesis mechanisms are more likely to be employed. And understandably he wants to know what the transformational consequences of those mechanisms are likely to be under various circumstances. Woody and I can't argue with these reasonable questions, because in fact they are also our own questions to ourselves. The challenge is that we don't yet know all of the answers. Our hope is that Padgett and Powell will not be alone in searching for these answers. Others, with different application domains in mind, are more than welcome to join us in parallel research to try to find the answers.

In lieu of answering Jim's questions as directly as he would like, I will confine myself here to specifying the outlines of what an "answer" would look like within the autocatalytic-network framework. The first complication in analyzing open-ended evolving systems is scientifically to define what 'prediction'

means in the study of historically contingent processes. Physicists and economists for the most part understand prediction to mean "convergence to equilibrium"—although the best of them recognize multiple equilibria and hence indeterminacy in their theories. "Convergence to equilibrium" will not do, however, for analyzing open-ended evolving systems where the rules for interaction change, because equilibria are calculated by iterating fixed behavioral and (especially) interaction rules. I don't want to go into an elaborate philosophy-of-science detour at this point, but I argue and hopefully demonstrate in the book (especially in chapter 9) that the best that scientific theories of open-ended evolution can ever do is to understand/derive the "trajectory space" of finite potential futures latent in a structure, rather than to predict exactly which historical path a social or a biological system will "choose."

Darwin thought similarly: his image of history was a branching bush. Given the complexity, contingency and stochasticity of actual history, Darwin never fooled himself into predicting that this critter or that would evolve. Understanding the structure of the branching bush was enough for him—which was good enough for him to change the scientific world.

How can our theory move toward our own goal of predicting or more modestly postdicting evolutionary trajectories [roads available], even if not of predicting actual histories [road taken]?

Compared with comparable discussions of speciation and organismal novelty that you can find in the evolutionary biology literature, the distinctive contribution of our own social-science-inspired approach is "multiple networks." In discussions with my biology and chemistry colleagues, multiple networks are what they find interesting and new—not autocatalysis, which they know already. [What is new to them is old to us, and vice versa.] All of the

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“organizational genesis mechanisms” alluded to by Jim are various processes of combining multiple preexisting social networks into something relationally new. Given this, the three moving parts in our theory are (a) “multiple preexisting social networks” [analogous to initial conditions, or to probabilistically predisposing IVs], (b) “processes of combining” networks [the causal dynamic or motor], and (c) “relationally new” [the DV]. I will discuss each of these in turn, starting with the DV.

(a) “DV:” Relationally new. Ultimately defining an organizational case (or any type of case) as “novel” is a matter of historical sensibility and needs to be justified explicitly on those contextual grounds, not in the abstract. However, Powell and I do distinguish between “innovation” and “invention”—the former being a new object in its context, the latter being a new autocatalytic network that produces and reproduces that object. “Innovation” in our view (and more importantly in our cases) derives from transpositions of products, practices, people or language across autocatalytic domains. “Invention” in our view (and more importantly in our cases) derives from tipping from one autocatalytic network to another—often within domains, but occasionally more radically across domains, thereby refiguring those domains. Innovations (like biological mutations) are not really random; they have a “directed evolution” or “topology of the possible” pattern to the stochastic stream of them. This derives from the structure of multiple-network overlay or embeddedness through which they flow. Even if nonrandom innovations are “a dime a dozen”; that is, they are voluminous, stochastic, and of high frequency. Sort of like quantum flux in our theory. Important perhaps to the short-term destiny of the carrier of that innovation, they are mere “perturbations” from the long-run perspective of the multiple network system itself.

The real DV in our book is invention—namely, that small number of innovations that changed not just

the local site of their use, but the broader topology of “ways things are done” in which they are embedded. Think industry evolution, not product evolution. Spillover, feedback, and tipping are the core network dynamics that need to be documented, to establish that our DV of “invention” has occurred. Having identified and process-traced a candidate “invention,” the explanatory task becomes to understand what caused that original innovation to percolate through and to alter the multiple networks that sustain it. It also means to locate a control-group case, which is “close enough” according to some criterion, where nonetheless something different happened.

Building, testing and extending theory to us means doing careful, historically contextualized, and parallel case studies. An easy and lazy count of “adoption rates” won’t do. This is because explanatory theory to us is about dynamic processes and generative mechanisms, not about correlations. [Not that the latter could not be a useful step toward the former. A statistical estimation equation, no matter how sophisticated, is never itself a theory or even an explanation.] If such intellectual labor limits the speed of our own theory’s adoption, then so be it. We care more about the long-run anyway.

(b) “IV”: Multiple Networks. Social network analysis as it is currently practiced was not as helpful to us as an outsider might think. There are the usual sociological criticisms about SNA being “too static” and “too reified.” We agree with those criticisms, but feel that our own work and that of others is starting to make those complaints out of date. The weakness I am referring to instead is the focus of contemporary SNA on single networks, not on multiple networks. Ever since Harrison White and his blockmodels left the field, no one seems interested any more in measuring how multiple networks overlay and interpenetrate. SNA today is infatuated with big data and big networks, not with thick data and rich networks. That

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will make its future progress in the field of history even slower than it is now.

I don't have an immediate solution in mind for this problem with my subfield. But for Padgett and Powell to move in the direction that Jim Mahoney wants us to go, we need better tools for characterizing in a systematic way our IV as well as our DV. Looking to chemistry (in particular to evo-devo) and to their carefully studied metabolic and genetic regulatory networks might once again prove to be a source of inspiration, but perhaps that is asking too much. At very least they (unlike us) are onto the concept of catalysis, which lies at the heart of the issue of multiple-network intertwining.

(c) Causal motor: "Processes of Combining (and Reproducing)." Our critics are right to say that our eight organizational-genesis mechanisms were inductively derived from our cases, not deductively derived from some abstract model of autocatalysis. That does not mean that we have some rigid epistemological stance against models in favor of history, because we also use formal agent-based models. But it does mean that there is nothing fixed and magic about our number of eight; no doubt more multiple-network recombining or folding mechanisms will be found in the future. And it probably also does mean that even the mechanisms we have found eventually will be shown to be decomposable into more primitive operators that our histories have assembled into the collective "strategies" we see.

Let me defend, however, the value of induction, especially when the scientific goal is to study generative process, not static correlation. I will do so through two example mechanisms taken from my own research—multivocality and robust action, and incorporation and detachment.

Multivocality and robust action: It is true that my first study of Cosimo was a search for theory through nar-

rative, not a "test" of some preexisting theory. That is also true of Obert's and my study of Bismarck in this book. It is also true of my analysis of Deng Xiaoping in this book. It just so happened, however, that these three cases inductively turned out to be members of a family—the "multivocality and robust action" family of organizational genesis. The contents of their histories and the content of their IVs and DVs are radically different, but they were similar in process. All three were cases of brokering or stapling together not just different multiple networks but contradictory multiple networks, more or less at war with one another. Oligarchs and new men in the case of Cosimo; democracy and autocracy in the case of Bismarck; and reform faction and the army in the case of Deng. Previous dynamics in these cases had already demonstrated that simply throwing these multiple-network IVs into the pot was not sufficient to generate anything stable, much less new. The mechanism or process itself of multivocality and robust action was crucial to the outcome—the "DV" details of which were quite different in any case (to wit: Renaissance elite in the case of Cosimo, German federalism in the case of Bismarck, and successful economic reform in the case of Deng.)

The methodological point here is that patient induction and comparison of carefully constructed rich case studies is another route to constructing theory. Potentially induction is even a more fruitful route than statistical IV-DV correlations if the goal is to understand process and history.

My second example of induction is my other mechanism of "incorporation and detachment." When I wrote the Padgett and Powell book, it is true that this mechanism really was just a generalization from a case of one—the case of medieval Tuscan merchant banks. I also did another case study—of early-modern Amsterdam, where the stock market and joint stock company were invented. These both

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were not “examples of a preexisting theory” for me; they were just fascinating cases where for sure I could see “organizational invention” going on. I came up with a different tailor-made mechanism for Amsterdam, which I infelicitously labelled “migration and homology.” It was not until the plane ride out here yesterday to the ASA, however, that I realized inductively that these two are also members of a processual family. Amsterdam’s “migration and homology” really is just “detachment and incorporation,” with Tuscany’s “incorporation and detachment” sequence reversed. This is because in Amsterdam first there was a religious war (the Dutch Revolt) that detached vast population flows of Protestant merchant from the south of Spanish Netherlands and of Catholic merchants from the north of Spanish Netherlands. And then there was the massive incorporation of Protestant merchants from the south into northern governmental federations like Holland in order to make war through global trading. The unintended result was a brand new organizational form, the joint stock company, which inserted the more advanced mercantile skills and trading networks of the southerners into the regulatory crystallis of the northerners. This shrinks our eight mechanisms into seven, with variants in each family. Let’s hope that future research continues this evolution in understanding.

Having just now perceived this homology—of process, not of IVs and DVs—I have much work to do in order to move toward “if, then” generalizations of the type that Jim is asking for. In our rush for scientific rigor, however, let’s not forget that the patient inductive comparison of carefully done case studies was much as part of Darwin’s scientific method as was his occasional flash of theoretical insight from Malthus. Research-design courses in our home universities have far to go in teaching our next generation of students, as well as us, how to reason about and how to study causal process inductively, not just

how to test pseudo-deductive hypotheses with IVs and DVs. There is no reason that we should prohibit ourselves from opening the black box of causal process to look carefully inside.

NOTES

1. As discussed in chapter two, cellular enclosure and evolution are sometimes layered onto this chemical baseline definition, to produce more expansive definitions of life. But everyone agrees that chemical autocatalysis is a foundational component in the biochemical definition of life.

2. As far as how we conceptualize social systems to be more complicated than low-level chemical forms of life, we discuss three forms of social autocatalysis: (a) production autocatalysis, where products are reproduced through transformational (“technological”) relations among products, (b) biographical autocatalysis, where people (specifically the production and relational practices they carry) are reproduced by social relations among people, and (c) linguistic autocatalysis, where words and other symbols are reproduced through conversational relations among words and symbols. While there might be other things as well, we thereby make the claim that economy, social networks, and language are three prominent examples of social forms of life. 1. As discussed in chapter two, cellular enclosure and evolution are sometimes layered onto this chemical baseline definition, to produce more expansive definitions of life. But everyone agrees that chemical autocatalysis is a foundational component in the biochemical definition of life.

3. See the book for our distinction between innovation and invention. To be simple-minded about it, “innovation” is change in the nodes; “invention” is change in the reproductive networks that construct the nodes. “Dime-a-dozen” innovations either spill over into their surrounding reproducing network to expand into inventions, or they do not, in which case pre-existing autocatalyses mostly select them away (although not entirely if they are incremental enough).

4. A similar move in evolutionary biology to make evolutionary theory more “networky” than the traditional population genetics is called “evo-dev” (i.e., the evolution of development). In biology circles, we are in alliance with evo-devo. The Social Science Research Council recently has created a new Working Group on History, Networks and Evolution, under my chairmanship, to explore commonalities and differ-

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ences between biological and social-science conceptions of network evolution.

5. Although sometimes of course they can lead to implosion and collapse, as they did for Gorbachev, unlike Deng. See my chapter 9 in the book for a detailed analysis of how network autocatalytic theory explains the divergent responses of the Soviet Union and China to the same reform program.

6. Perhaps that should not surprise us so much, because even amoeba have “differentiation of domains.” This idea, I would argue, is a processual analogue to the more object-oriented concept of modularity. (I duly note, however, that the great Herbert Simon to his lasting credit defined his “nearly decomposable systems” operationalization of “modules” in network and frequency/energy of interaction terms. In spite of his brilliance, Simon missed the implications for evolution of multifunctionality.)

7. Stovel is right to mention the Harrison White lineage of this mantra. The book is dedicated to Harrison.

8. “[Agents in the book] were part of but did not control the explosive events they stimulated... If ‘agency’ means induced intent and learning, then fine. But if ‘agency’ means the capacity to foresee and control complex chains of consequences, then no. Autocatalysis does not deny individual agency; it just endogenizes that as one time scale in life, interpenetrating with others” (Padgett and Powell, p. 60).

9. This is why in times of turbulence, like our cases, rational choice is stymied: even the set of actors to strategize against has changed.

10. This is not inconsistent with “Coleman’s boat.” It is just that his hypothesized downward arrow of causation—from macro to micro—is rarely theorized in his rational choice tradition, where the upward arrow—from micro to macro—reigns supreme.

11. See footnote 2.

12. Cosimo de’ Medici as ensemble individual didn’t want to maximize profit; it was Cosimo de’ Medici as banker that wanted to make profit. Likewise, Cosimo as politician wanted power, and Cosimo as father wanted status for his family, not Cosimo as a biological person. When pursuing multiple goals is made consistent by a world that made their multiple outcomes correlated, then it becomes mathematically possible to represent Cosimo “as if” he had a superordinate “utility” function. But when pursuing multiple goals is contradictory, because of zero or even

negative correlation in their outcome variables, then cycles and situational switching behaviorally are observed. The assumption of “as if” maximization then becomes mathematically invariable, because foundational axioms of von Neumann-Morgenstern utility theory are thereby violated. It is logically impossible to maximize cycles.

13. Questions have been raised about the relationship between Padgett and Powell’s [and Durkheim’s] “domains” and Bourdieu’s “fields.” Woody has addressed this already. My two cents are (a) that there is considerable consistency at the micro level in that both autocatalytic networks and fields ultimately are composed of reproducing practices (“habitus” in Bourdieu’s terminology), but (b) that Bourdieu’s “fields” are too exogenous and top-down in conceptualization, because they are founded on metaphors like “gravitational field” and “soccer field,” which require an external force (like the sun or the state) to establish. “Domains as autocatalytic networks,” in contrast, are bottom-up and emergent. This is not to say that “institutional logics” have no place in social analysis, but they should appear, it seems to me, at the end of the emergence causal chain, not at the beginning. Regulation kicks in to maintain autocatalysis after emergence has already unfolded. It was the error of functionalism to mistake the (equilibrating) consequence for the (genesis) cause. I would be delighted if social scientists treated “fields” simply as a shorthand for “autocatalytic networks,” without all of the Foucault control overtones of “fields.”

14. See the book by Paul McLean, *The Art of the Network: Strategic Interaction and Patronage in Renaissance Florence* (2007), for an example of culturally oriented work very compatible indeed with our perspective.

15. “I stand with Harrison White [and with Herbert Simon] in concluding that, our Enlightenment pretensions notwithstanding, mostly we all play interpretive catchup with events, trying to respond to the jaggedness of the unpredictable twists of a vibrant and vast social world far beyond our comprehension.” (P&P, p. 61)

16. This is our label, not his, of course. But from our perspective, that was exactly what Sewell was writing about.

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QUESTION & ANSWER

BRUCE CARRUTHERS
NORTHWESTERN UNIVERSITY

Historically, multiple networks are often compressed. How do you know when something is compressed or not? Wasn't there already a great deal of intermingling of family, business and politics in pre-Renaissance Florence? Are the different domains easily distinguishable? How do you know which domains to analyze for cross-domain study?

Woody Powell (WP): Good question. Of course, different domains often appear intermingled when viewed at a particular point in time. For example, family and business for John's era; science and commerce, technology and finance for the current era. One reason we focus so intently on biography, the network construction of persons, and biographical autocatalysis is this exercise affords a picture of these processes over the life course, revealing how and when domains come into contact with one another, and with what consequences. The scientists in chapters 13-15 attend college, graduate school, work in particular labs, take their first jobs, etc. Along the way they are exposed to different laboratory cultures that tilt toward either public or private science. As they begin their faculty careers, they end up at universities that have very divergent forms of engagement with the world of commerce (think Stanford or MIT vs. Chicago). Such contacts shape both the construction of the person, and the character of the science.

You also asked about the choice of domains to study.

As a historical sociologist, Bruce, you know that the course of personal lives is deeply bound to historical times. Differences in birth year expose people to quite distinct social worlds. Our focus is on the intersection of biography, history, and social structure. We let our inquiry dictate which domains shape people's lives at particular points in time. We are not wed to three domains, and the domains obviously cannot be the same across place and time. Nevertheless, kinship, politics, profession or guild, and commerce seem like useful starting points.

John Padgett (JP): Let me also add something from the modeling perspective. In chapter 3 on agent-based models of autocatalysis, my coauthors and I showed that autocatalytic processes under ALL chemistry automatically self-organize into multiple sets of hypercycles, which we call (simple toy) "domains." However these interwended and interpenetrated naturally in our computer simulations. For example, one outcome hypercycle "domain" might be $\{(1 \gg 2), (2 \gg 3), (3 \gg 1)\}$ whereas a second might be $\{(1 \gg 2), (2 \gg 4), (4 \gg 10), (10 \gg 1)\}$. Clearly the "rule" or "practice" or "skill" $(1 \rightarrow 2)$ is a member of both domains at once. Indeed that rule interconnects the two model "domains." This intermingling is exactly your point. My point in response is that this intermingling is a corollary of autocatalysis itself, once chemistries are enriched enough to permit that. [That is, no such multiple domains were possible, by definition, for the simpler SOLOH chemistry.]

CHRISTOF BRANDTNER
STANFORD UNIVERSITY

Are you set on just eight mechanisms? Are these the right eight mechanisms? More generally, are these mechanisms in the rational choice sense, or generative processes?

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JP: No, we are not set on eight mechanisms. Those were inductively derived from our empirical analyses, to illustrate our general multiple-network perspective on emergence in action. We fervently hope that both we and others will follow in our footsteps to add, combine and shrink this list, as more and more cases are added to collective research on this topic. At the end of my response to the critics, above, I made a plea for induction, as well as deduction, as a way toward the construction of general theory. I even showed there how to shrink our list of eight to seven, by combining “incorporation and detachment” with “migration and homology.” Inductive discovery like this is good, not bad, if the goal of science is biology-like generative mechanisms or processes, not physics-like covering laws.

JEANNETTE COLYVAS
NORTHWESTERN UNIVERSITY

What is the relationship between hypercycles and multiple networks? In your agent-based models, do multiple networks emerge, or do you begin with them? Would hypercycles occur without multiple networks?

JP: From an empirical point of view, “multiple networks” are eclectically defined, depending upon creative data measurement techniques. But from our theoretical point of view, “multiple networks” are the reproducing consequence of autocatalytic processes, as I just said in the previous answer. But that is only the short run “actors create relations” half of the answer. The longer-run “relations make actors” other half is that emergent networks, once they emerge, become “institutionalized”, as others would say. In autocatalytic terminology, reproductive relational flows become stabilized and repetitive through “relational protocols” that crystalize out of previous actions, through increasing the likelihood of successful interactors interacting again in the future. Simple modeling examples in chapter three were stigmergy and Moore space. An empirical example in chapter six was the master-

apprentice guild protocol for making companies. Relational protocols (or “institutions”) reify or “remember” the past and thereby guide, but do not control, the flux of the present.

The core point to remember about our theory, however, is that novelty is not the result of autocatalysis or multiple networks operating alone. Both have to interact for speciation or emergent actors to emerge. A more temporal (and less clear?) way of saying the same thing is that novelty emerges out of the flux of the present confronting the reification of the past, and then tipping (or not) through reproductive feedback.

ANDREW SCHRANK
BROWN UNIVERSITY

What were your criteria for choosing the topics and themes your group discussed at SFI? And how did you decide on the cases that you have analyzed?

WP: There is a personal story here. John and I were familiar with each other’s work ever since we shared a house at Stanford back in the spring of 1992. He was building the data set that led to his robust action paper with Chris Ansell, and I was interviewing Stanford faculty who were involved in the early days of the biotech industry. So we knew each other’s cases really well. And we found we both had an interest in pico-level biographical data, and we were interested in network pathways and trajectories. What wasn’t present yet, at least for me, was a deep appreciation of the feedback dynamics, the reverberations caused by the intersection of different pathways.

At SFI, in the initial years, we assembled different groups each summer of smart people doing different styles of network research. That proved both illuminating and frustrating. We learned a lot but the ap-

proaches were very discordant, and not everyone shared our concerns, which may well have seemed monomaniacal, with what flows through networks and with what consequences. There is a lot of elegant work on the structure and topology of networks, and that is great, but we wanted cases that went beyond mappings or depictions of small worlds to engage with our abiding question of when do we observe novelty. So our cases had to address these questions, and I guess our collaborators had to tolerate John and me asking endless questions.

ANN SWIDLER
UNIV. OF CALIFORNIA, BERKELEY

How would you differentiate autocatalysis from reproduction? The latter produces change but in the same direction, whereas you are emphasizing novelty. What distinguishes the two processes?

WP: Reproduction as you describe it Ann, is replication. It involves the passing on, through either direct efforts or indirectly through diffusion, of existing ideas or practices. To be sure, a certain amount of innovation is the result of failed reproduction, that is, efforts at copying do not always work, and new parts get added, or the thing being copied turns out different when propagated in unfamiliar soil. Eleanor Westney wrote about this eloquently in her *Imitation and Innovation*, an analysis of Meiji Japan's efforts to borrow Western organizational practices, and all the misreading and jerry-rigging that transpired.

Autocatalysis takes these ideas in a fresh direction. Practices or models get transferred, re-purposed, employed in unfamiliar contexts all the time. This kind of innovation, as John has put, is everywhere. On some occasions, such moves have staying power and become locally consequential. This is the world of brokerage, which Ron Burt and others analyze so well. Even less frequently, these innovations have reverberations back into the worlds of their origin, and alter how things are done in their home domain.

Put differently, reproduction is about inheritance and how offspring are influenced. Autocatalysis can transform both the parents and offspring, and create novel biographical and institutional trajectories.

JP: Autocatalysis is reproduction of networks, period. Our point, however, is that there is not one process for stability and another process for change. Both stability and change are rooted in the same reproductive and overlapping autocatalytic processes. Difference in outcome of autocatalytic processes of reproduction comes from the dynamics of feedback: both equilibria and cascades are possible in the same structure, depending on circumstances. The testable implication of all this is our dynamical systems vision that history is a finite number of trajectories latent in any multiple-network structure—what Walter Fontana has called the “topology of the possible.”

TERRAFORMING
ECONOMICS:
THE INSTITUTE FOR NEW
ECONOMIC THINKING
CONFERENCE, 2015

Field Notes From
DONALD TOMASKOVIC-DEVEY

George Soros and a few fellow financier/philanthropists founded the Institute for New Economic Thinking (INET) after the global financial crash to reframe the economics profession toward more plausible models. They hold the economics profession as at least partially responsible for the global crash, a position I share. Thus INET's goal is to transform mainstream economic thought and by extension the

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policy models adopted by governments and central banks. This is an unusual approach to policy and science development, a kind of terraforming from the outside-in. INET has a program of well-funded grants, conferences, and public relations vehicles, including programs for transforming the economics curriculum and for graduate student participation in INET conferences.

This year's conference took place at the OECD headquarters in Paris. The conference is populated by multiple economic Nobel Prize winners, young mainstream and older heterodox economists, policy economists, and a smattering of other scientists from sociology, political science, psychology, neuroscience, and even physics. The final conference dinner was held in the incredibly sumptuous *Palais Garnier, Opéra National de Paris*, included very good food and wine, a speech on the future of Europe by George Soros, and a chamber quartet. Mysteriously, my co-author Ken-Hou Lin and I were invited. Who could resist?

Inequality was the meeting's theme. Nobel Prize winner Joseph Stiglitz and *Capital in the Twenty-First Century* author Thomas Piketty provided joint keynote addresses. Swirling around the conference was an underlying concern about the fragility of the financial sector and the conceptual shortcomings of mainstream economics. Central bankers, bank regulators, and finance crisis first responders were abundantly represented. Conference presentations repeatedly juxtaposed complexity, context, institutions, and social networks on the one hand and the complex real motivations of actors on the other with the elegant, ahistorical, and unrealistic mathematical representative rational actor models central to neoclassical economic thinking, training, and policy tools.

Describing Inequality

The low hanging fruit turned out to be the empirical

description of rising inequality. There seems to be almost no debate that inequality is rising. Stiglitz asserted that mainstream economists now think of inequality as a fundamental aspect of economic analysis, and the profession now recognizes that inequality undermines both well-being and growth. Piketty was less convinced, characterizing mainstream economists as admitting to the trend but not the implications of inequality, much less budging from a growth *ueber alles* normative economic model.

The conference provided plenty of empirical grist for recognizing the centrality of inequality to economic processes. Branko Milanovic, from the CUNY Graduate Center and the World Bank, took a long-term perspective, identifying malign (e.g. war) and benign (e.g. social insurance) institutional forces that reduce inequality. He predicts that inequality will rise in the U.S. because these forces are missing. Five additional mechanisms drive increased inequality in the U.S.: increasing capital income shares, a high concentration of new wealth, a strengthening association between employment and capital income, assortative mating, and—to cement the trends—a greater influence of money on politics. Vamsi Vakulabharanam, from the heterodox University of Massachusetts, Amherst economics department criticized the focus of most economists, including Piketty, on the generic nature of capitalism and inequality, arguing in contrast that the rate of return on capital and wealth concentration are the result of local political economic forces. He explained that in Asia before 1980 there were equalizing trends as countries emphasized labor-intensive growth. Post 1980s, however, socialism waned, urban capitalist professional elites took advantage of the global economic regime to capture national income flows, and inequality rose. In China and India from 1988 to 2010 practically all relative income gains

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went to urban elites.

The most radical talk I encountered was by Andrew Sheng, former central banker (Honk Kong) and current Fung Global Institute fellow. Sheng's message to economists was that markets of all sorts are increasingly concentrated and dominant actors rule. He asserted that the five largest central banks control 68% of world central bank assets and only a handful of major accounting firms, rating agencies, and big banks fill out the system. As a result, there is no free market. Rather, the world economy is a concentrated winner-take-all hierarchy, with central banks the agents of concentrated power. Consistently, Jerry Epstein, also from the heterodox UMass economics department, explored the effects of the U.S. Federal Reserve's quantitative easing practice of buying financial assets from commercial banks in order to stimulate economic growth. His analysis showed that the biggest banks' profits were propped up, but neither lending to households or firms, nor employment grew. In Epstein's model, central bank policy serves the interest of finance because central banks are insulated from political pressures from labor and even industrial capital.

Largely missing from the discussion of inequality was production, workplaces, and the concentration of income among the top 1%. That the increase in inequality in most countries is being produced by changes in firm compensation practices and labor processes was absent from almost all of the panels I attended. One exception was William Lazonick, UMass Lowell, who proclaimed that the most insidious idea that ever came out of economics was the notion of shareholder value. He argued that we must understand productivity, company pay schemes, and capital claims on income to understand inequality. In contrast, University of Chicago Nobel Prize winner James Heckman opined that the way to limit inequality is to increase skills in children when they

are still malleable. No firms there, just a familiar supply side inoculation of the populace.

Challenging Traditional Models

The two most exciting sessions I attended were organized around social networks and psychology and were direct, external challenges to traditional economic models. To make them palatable both were repackaged as uber-scientific, via the labels of econophysics and neuroscience. Here the new economic thinking strategy was to import ideas from other sufficiently respectable scientific disciplines.

Tania Singer, of the Max Planck Institute for Human Cognitive and Brain Sciences, undermined the economic behavioral model of fixed preferences, personal utility, and rational self-interest by summarizing lessons from cognitive science. We are not autonomous actors, but rather we monitor others' motives and emotions and we have multiple malleable motives for action. Multiple motivations—achievement, consumption, power, fear, anger, affiliation, care—are all simultaneously available. Which motivational system is activated depends on the interactional context and is filtered through personal interpretations of that context. Macro-economist Dennis Snower's fairly dramatic reaction to this was to suggest that since economists' assumptions about preferences and motives are wrong, all prior knowledge generated in economics is obsolete. He concluded that what humans are actually good at is dealing with uncertainty, *not* the probabilistic risk in standard economic models. This sounded a lot like symbolic interactionism to me. One economic historian noted in the discussion after the panel that the whole economic model of human behavior as self-interested rational machines was made up, adopted from a 19th century philosophical thought experiment, and never based on a science of human behavior.

TERRAFORMING ECONOMICS

The session built around econophysics focused on network effects in financial markets before, during, and after the 2008 crash. Stefano Battiston, a physicist by training and now a finance professor at the University of Zurich, showed that network effects matter for financial stability. He focused on the post-crash regulatory reforms that increased bank reserve capital requirements but ignored the risk posed by the tight network structure of finance. Given what we know about that network structure, his simulations suggest that if just one of the 22 largest systemically important firms failed, 70% of the financial assets in that system would be wiped out by contagion effects. The current “tightened” regulatory framework thus misses two-thirds of the risk to financial collapse in the system. In the same session, Co-Pierre Georg, University of Cape Town and the German Bundesbank, examined the lending market in Europe, tracking minute-to-minute inter-bank transactions in the days immediately following the collapse of Lehman Brothers and leading up to the bailout of the financial system by the major central banks. What he found was that after Lehman failed, banks continued to lend but switched from long term to overnight loans. Total lending did not decrease. Ironically it was only after central banks provided liquidity that real lending was cut in half. His interpretation was that the banks cried wolf, the central banks took on the risk, and the real economy suffered. Most importantly for INET’s goal of promoting new economic thinking, both speakers were pessimistic that central banks would actually adopt a network perspective on risk.

What’s Next

So what to make of this terraforming project? The economists with powerful new models and empirical work all came from the fringes of the economics profession. These economists recognize the importance of power, context, relationships, and institutions. The strongest source of challenging new ideas came

from outside of economics, particularly from actors with external scientific or central-banking based legitimacy. The high status, insider economists provided less in the way of new ideas: Stiglitz went back to the 19th century preoccupation with land as capital and Heckman to a supply side strategy of expanding skill to explain contemporary inequality (although to be fair, Heckman now has a much more complex notion of skill than in conventional human capital models). Student panels commented on inertia in graduate training and the econophysicists commented on policy resistance to network models of economic processes. Judging by audience reactions, the implications of cognitive science’s more complex and contextual models of human motivation seemed to have the most appeal to the average economist in the room. Perhaps the rational utility maximizing actor assumption will be the first brick in the wall to crumble?

Since this is an economic sociology newsletter, some readers might wonder where economic, organizational, and inequality sociologies fit into new economic thinking. No sociologists were cited or discussed at the meeting (although Rob Sampson had a cameo role), but both institutional thinking and class perspectives on inequality are clearly present at the heterodox fringes of the discipline. Network models of the economy are present as well. My prediction is that if INET (or more likely the next financial crash) succeeds in producing a new economics, the ideas that constitute the everyday currency of adjacent sociologies will be incorporated, and we will no longer be able to base our models on critiques of economist’s thinking. But it looks like these remain challenger ideas for now, not yet incorporated into graduate training or incumbent imagination, and so our practices may remain ours. One change that would be useful for sociology, however, would be to increase our trade with these newly influential, if not yet central, het-

erodox economists, cognitive psychologists, and ecophysicists. We have much to learn from each other.

The other thing we should be doing is submitting grant proposals to INET.

MEMBER PUBLICATIONS

Tim Bartley, Sebastian Koos, Hiram Samel, Gustavo Setrini, and Nikolas Summers. Forthcoming (June 2015). *Looking Behind the Label: Global Industries and the Conscientious Consumer*. Indiana University Press.

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30% off discount code: MASA30

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Victoria Reyes. 2015. "Legacies of Place and Power: From Military Base to Freeport Zone." *City & Commu-*

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CALL FOR SUBMISSIONS

Call for submissions—Work in Progress blog

The Work in Progress blog, of the Organizations, Occupations and Work section of the ASA, invites submissions (800-1,200 words) on all topics related to organizations, occupations and work, broadly understood. The primary purpose of the blog is to disseminate sociological findings and ideas to the general public. Articles should be accessible and jargon-free, written like a New York Times op-ed.

We will publish summaries by authors of all monographs related to organizations, occupations and work. Additionally, we invite proposals for three types of article: research findings, news analysis, commentary. Interested authors should send a proposed title and topic (one paragraph maximum) to Matt Vidal (matt.vidal@kcl.ac.uk). The WIP Editorial Team will decide whether to invite a full submission.