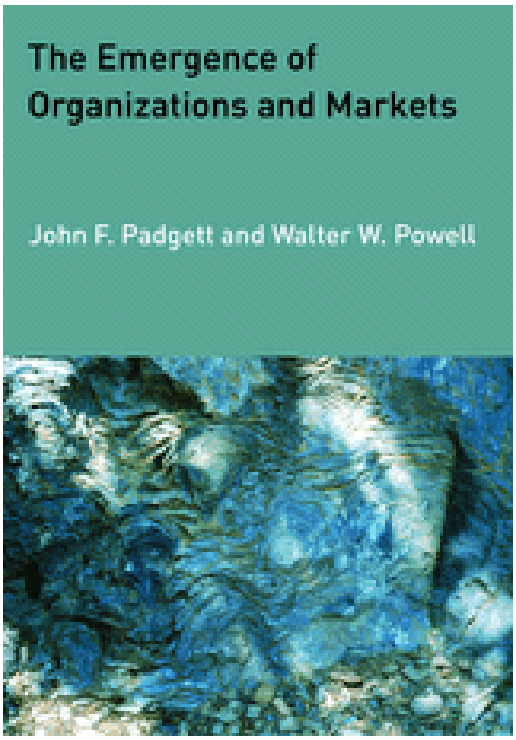
Author Meets Critics Session

American Sociological Association
August 2014
San Francisco, CA



The Emergence of Organizations and Markets: An Agenda-Setting Book

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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 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 refuntionality. 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 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-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 mechanisms work in certain settings to stimulate innovation, invention, and emergence in organizations.

Comments on *The Emergence of Organizations and Markets*

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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 tradition 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 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 Powell or Padgett; 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 instance, 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 under-developed. 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 vary 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 relate 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 on Padgett and Powell, *The Emergence of Organizations and Markets*

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 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 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 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 forebearers, 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 Meet Critics Session – ASA 2014 Meetings, for *The Emergence of Organizations and Markets,* Comments by Woody Powell

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 worked 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 pursuing 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 Stovell 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 auto-catalytic, 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 Institutionalization," *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 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 malevolent. 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 (U. 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 Hedstrom, 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 thought and categories of actors come from? More attention to flows, we believe, will deepen and enrich social science.

Padgett response to ASA critics: Brayden King, James Mahoney and Katherine Stovel

(revised version: October 5, 2014)

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 life.[1]

Like chemists, we recommend that social structures be conceptualized processually as regenerative vortexes through time. In saying this, we are saying 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 that 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 invention[3] 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 networks[4]—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

[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.

[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 differences between biological and social-science conceptions of network evolution.

just our short-hand way of saying "transposition and recombination of multiple autocatalytic 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 change[5]—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 autocatalytric 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]

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.

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[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 decomposably 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.)

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 inter-temporal 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 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

[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." (P&P, 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.

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 autocatalysis[11] 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 causality[13] 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.

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[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 inviable, 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."

Where is culture? [the question most emphasized by Katherine 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 autocatalysis[16] 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.

How to turn all this into researchable normal science? [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 refunctionality [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.

[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.

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 'chose'.

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 "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 non-random, 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) ”IVs”: 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 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 P&P 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 narrative, 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 P&P 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 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.