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Organizing Far from Equilibrium: Nonlinear Change in Organizational Fields

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Organizational fields undergo upheavals. Shifting industry boundaries, new network forms, emerging sectors, and volatile ecosystems have become the stuff of everyday organizational life. Curiously, profound changes of this sort receive scant attention in organization theory and research. Researchers acknowledge fieldwide flux, emergence, convergence, and collapse, but sidestep direct investigations of the causes and dynamic processes, leaving these efforts to political scientists and institutional economists. We attribute this neglect to our field's philosophical, theoretical, and methodological fealty to the precepts of equilibrium and linearity. We argue that ingrained assumptions and habituated methodologies dissuade organizational scientists from grappling with problems to which these ideas and tools do not apply. Nevertheless, equilibrium and linearity are assumptions of social theory, not facts of social life. Drawing on four empirical studies of organizational fields in flux, we suggest new intellectual perspectives and methodological heuristics that may facilitate investigation of fields that are far from equilibrium. We urge our colleagues to transcend the general linear model, and embrace ideas like field configuration, complex adaptive systems, self-organizing networks, and autocatalytic feedback. We recommend conducting natural histories of organizational fields, and paying especially close attention to turning points when fields are away from equilibrium and discontinuous changes are afoot.

Key words: nonlinear change; multilevel change; field configuration; complex adaptive systems; research methods

This essay addresses the problem of social scientific investigation of discontinuous changes occurring within organizational fields. We begin by taking stock of current approaches to the study of field-level change. We summarize major lines of research in institutional theory, note accomplishments, identify questions left unanswered, and more significantly, highlight crucial questions that are yet to be asked. We contend that the failure to ask these questions is blocking our progress in conceptualizing and researching discontinuous field-level change.

We attribute this situation to pervasive assumptions of equilibrium and linearity. Both theories of organization and prevailing research methodologies are infused with implicit and explicit equilibrium assumptions. Even more fundamentally, we argue that beneath mainstream social scientific theories and research methods lies what Andrew Abbott calls "a general linear reality"—a set of deeply held causal beliefs that treat linear models as representations of the actual social world (Abbott 2001). This amalgam of mutually reinforcing beliefs, theories, and methods honoring the notion of equilibrium has, we claim, blocked the investigation of a family of interesting problems of great practical import.

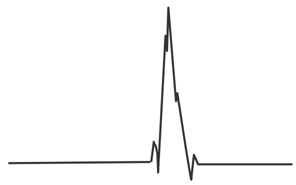
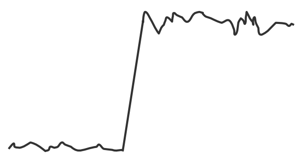
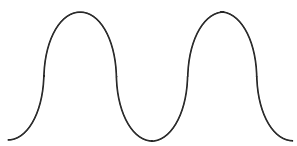
What might a social science of organizing-away-from-equilibrium look like? This question is so challenging that we can only begin to address it in this paper. Instead of tackling it head on, we approach the question indirectly by considering the special problem of studying organizational fields undergoing discontinuous change. We do this by: (1) recounting concrete dilemmas we have encountered in our efforts to study organizations whose fields were in flux; (2) describing philosophical, theoretical, and methodological patches we applied to our prior beliefs, conceptual models, and research designs; and (3) distilling from these experiences some general principles for studying organizations far from equilibrium.

The overall research posture we advocate is coevolutionary, multilevel, contextual, processual, and emergent. Calls for research having these characteristics are plentiful; we offer nothing particularly new here. However, while gathering data from organizations embedded in fields undergoing significant nonlinear change, we have devised some heuristics from which novel and, we believe, useful lessons may be extracted.

Studying Fields in Flux

Over the last 25 years, we have conducted four large-scale field studies in which we set out to study organizations' adaptations to their environments. The first two studies investigated strategic change in hospitals (Meyer

Table 1 The Folly of Assuming Equilibrium While Studying Fields in Flux

Research study	Research baggage	Fieldwide discontinuity	Research bricolage
Study 1: Adapting to environmental jolts (Meyer 1982)	• Equilibrium: has been achieved • Theory: contingency theory ("fit") • Research Design: static/cross-sectional • Unit of Analysis (UOA): organization • Data: survey, interview • Analysis: regression		• Equilibrium: being sought • Theory: adaptation (variation/ selection/retention (VSR)) • Research design: natural experiment • UOA: organization and environment • Data: ethnography, survey, interview, secondary • Analysis: triangulation
Study 2: Responding to hyperturbulence (Meyer et al. 1990)	• Equilibrium: being sought • Theory: adaptation (VSR) • Research design: quasi-experiment • UOA: organization and health sector • Data: survey, interview, secondary • Analysis: pooled time-series regression		• Equilibrium: punctuated equilibrium • Hyperturbulence • Parallel research designs: historical analysis, grounded theory • UOA: organization, network, and sector • Data: survey, interview, secondary, naturalistic observation • Analysis: triangulation
Study 3: Inhabiting an oscillating field (Gaba and Meyer 2005)	• Equilibrium: being sought • Theory: innovation diffusion • Research design: dynamic modeling • UOA: organization and population • Data: secondary • Analysis: event history		• Equilibrium: perpetual disequilibrium • Theory: coevolutionary diffusion • Research design (two-stage): (1) causal analysis of venturing community, (2) dynamic modeling of IT population • UOA: community, population, organization • Data: secondary, interview, naturalistic observation • Analysis: (1) VAR technique for venturing community, (2) event history for IT population

1982, Meyer et al. 1990). The third investigated technology corporations' adoption of venture capital programs (Gaba 2002, Gaba and Meyer 2005). The fourth is an ongoing investigation of network emergence within the nanotechnology investing community (Colwell 2003, Colwell and Meyer 2003).

Testing Models of Organizational Adaptation. Our first three studies began by conceptualizing organizations' environments as stable fields composed of other organizations to which a focal organization seeks to adapt to increase fitness and improve its alignment. Alignment was explicitly theorized as a result of the variation-selection-retention mechanism posited by evolutionary theory (Campbell 1969). What we failed to recognize was our tacit assumption that social systems exist in the state of equilibrium—or at least gravitate toward a quasi-stationary equilibrium.

However, amidst data collection for each of the three studies, we encountered field-level change of sufficient magnitude to utterly discredit the equilibrium assumption. During Study 1, a strike by doctors jolted hospitals (Meyer 1982). During Study 2, government regulators summarily overturned the rules and boundaries of competition within California's health sector (Meyer et al. 1990, Haveman et al. 2001). During Study 3, a boom-and-bust cycle in the venture capital market led corporate venturing programs to oscillate between explosive growth and collapse (Gaba and Meyer 2005).

As portrayed in Table 1, the unanticipated changes we confronted in each of these studies serve to illustrate three distinct forms of nonlinear change in organizational fields: (1) *jolts*—transient shocks that disrupt fields temporarily, perturb their organizational inhabitants, and then subside; (2) *step functions*—changes that sweep through fields, permanently altering features such as structures of competition, habitable niches, or market and industry boundaries; (3) *oscillation*—recurring cycles of field-level expansion and contraction, passing through periods of discontinuity near the zenith and nadir of each cycle.

Some common threads run through these three studies. In each case, we found that it was impossible to understand adaptation moves by limiting our focus to a sample of organizations' actions at a particular point in time. Ultimately, we concluded that as a theory of organizational action, "adaptation" is an unproductive concept in nonlinear settings. When discontinuous changes are rippling through an organizational field, there is no equilibrium to be sought, and the idea of organizational adaptation loses meaning.

In each case, our team scrambled to reconceptualize and redesign the study, and in each case, the research design shifted from cross-sectional to longitudinal data collection. In every instance, the theoretical platform shifted from testing a variance theory to building a process theory (Mohr 1982). In each case, the unit of analysis shifted from focal organizations in exogenous environments, to be replaced by a set of nested units—organizations, that collectively constitute a population, amalgamated into an ecological community, embedded in a changing organizational field.

These first three projects were messy and bewildering. They were sustained only through a frenetic sort of research bricolage. To keep each project afloat we had to drop our familiar research tools (Weick 1996), and work in the moment with whatever materials we could lay our hands on.

Investigating Field Emergence. Our jerry-rigged research efforts in Studies 1, 2, and 3 informed our next field study. In this ongoing research we have set out *deliberately* to examine processes of emergence within an organizational field, focusing on the commercialization of nanotechnology. That is, we take the organizational field in flux as a given and make it a central analytical unit—instead of waiting to be blindsided by disequilibrium conditions and forced into adopting this point of view. We have grounded this new field study in an evolutionary perspective, as we had in Studies 1, 2, and 3. However, this time when packing our theoretical bags we left the concept of adaptation back in the office. We learned from our first three flawed research efforts that adaptation logically implies the operation of the variation/selection/retention mechanism. Accordingly, its utility is limited to settings at or approaching equilibrium. In place of this neo-Darwinian model of adaptation, we are shifting to a complex adaptive systems model positing that order can emerge and persist in the absence of a fixed-point equilibrium.

Surveying the field of organization science, we see a growing number of researchers grappling with volatile ecosystems, emerging sectors, shifting boundaries, and proliferating network forms. We notice that many of our colleagues bring assumptions of equilibrium and linearity to their research settings, as we did. We now believe this conceptual baggage is ill-suited to studying fields in flux. We share our missteps and comebacks in the hope that our colleagues can learn vicariously and avoid our mistakes. However, before recounting and reflecting on these, we discuss the dearth of theory addressing fields in flux and trace the diffusion of equilibrium assumptions from Newtonian physics to organization theory.

A Theoretical Lacuna

The concept of field is central to institutional theory. DiMaggio and Powell define a field as "those organizations that, in the aggregate, constitute a recognized area of institutional life: key suppliers, resource and product consumers, regulatory agencies, and other organizations that produce similar services or products" (1983, p. 148). A large body of work has investigated how institutionalized rules and environments shape organizations' practices, performance, and survival (Scott 2001). This work takes the externality of institutions to organizations as a starting point, presuming that institutions reside at interorganizational or supraorganizational levels (Strang and Sine 2002). Generally speaking, the origins of institutions and institutional change have received far less attention than their impacts on organizations (Powell 1991, Barley and Tolbert 1997).

Davis and Marquis (2005, p. 332) characterize modern organizations as "dense spots in networks of contracts between sovereign individuals." Rather than studying such ethereal units, they recommend "taking the field as the relevant unit of analysis and remaining agnostic about whether it is composed of organizations, individuals, or other combinations of actors" (p. 337). Some recent empirical work has begun to do this (e.g., Thornton and Ocasio 1999, Haveman and Rao 1997, Haveman et al. 2001, Scott et al. 2000). These studies offer accounts of institutional change, shedding light on the role of change agents, triggering conditions, and legitimating processes. However, as Strang and Sine point out, these "accounts of change are not theories because they point to proximate conditions rather than endogenous dynamics" (2002, p. 23). With rare exceptions, scholars assume that organizational fields institutionalize equilibrium states, offering to the theorist trying to understand field-level change and emergence only an infinite regress of higher-level contexts. Hirsch (1997) likens institutional theory's prevailing mindset to a brave new world where top-down forces of stability and order preserve equilibrium.

Equilibrium: From Physics to Organizations

The concept of equilibrium diffused from Newtonian physics to neoclassical economics and then to organization theory—with some definitional reformation along the way:

(1) *Physics*—The state of a body or physical system at rest or in unaccelerated motion in which the resultant of all forces acting on it is zero and the sum of all torques about any axis is zero.

(2) *Economics*—A situation in which economic agents or aggregates of economic agents such as markets have no incentive to change their economic behavior.

(3) *Organization Theory*—A condition in which all acting influences are canceled by others, resulting in a stable, balanced, or unchanging system.

Equilibrium is the bedrock on which modern economic theory rests. Keynesian, neo-Keynesian, and monetary theories share a foundation in general equilibrium theory. Krugman (1996) describes economics as "the study of those phenomena that can be understood as emerging from the interactions among intelligent self-interested individuals" (p. 4). He further maintains that "agents are not only intelligent, they maximize—that is, they choose the best of all feasible alternatives... and when they interact, we assume that [they] achieve an equilibrium in which each individual is doing the best he can given what all the others are doing."

Economic equilibrium, therefore, constitutes a point of rest from which there is no endogenous tendency for any individual, firm, or market to change. Anyone who looks at the world, Krugman notes, "knows that these are extreme and unrealistic assumptions," but because the general equilibrium model is crafted so that economic problems can be formulated in Newtonian mathematics, it has offered economists an "astonishingly powerful way to cut through what might otherwise be forbidding complexity" (p. 6).

Equilibrium theory, accompanied by Newtonian mathematics, has diffused from economics throughout social scientific theory and methodology. Herbert Simon (1947) defined an organization as a system that can exist only when inducements and contributions are in equilibrium; all mainstream theories of industries and organizational fields presume that equilibrium is sought and achieved by firms, markets, and sectors (Bromiley and Papenhausen 2003). However, unlike economists who articulate their equilibrium assumptions explicitly, many social scientists fail to recognize the concept's pervasive influences on their own theories and methods. While social scientists routinely disavow the economist's fiction that human beings are rational maximizers, they rarely challenge the equilibrium assumption. All too often, we forget that equilibrium is also a convenient fiction, and treat it instead as literal truth.

Study 1: Adapting to Environmental Jolts

Research Baggage

Our first empirical encounter with an organizational field in flux was accidental. This paper's first author had just finished gathering questionnaire and interview data from CEOs of 19 San Francisco hospitals to test a then newly formulated typology of organizational strategy and structure (Miles and Snow 1978). Contingency theory supplied the study's conceptual underpinnings, leading to the premise that organizations gravitate toward one of the three stable configurations that Miles and Snow have termed "prospectors," "defenders," and "analyzers." We thought this would occur through the interplay of enactment moves undertaken by an organization's top managers (Weick 1969), and natural selection pressures emanating from its environment.

Then all of San Francisco's hospitals received a severe jolt from their environment. One Monday morning, a malpractice insurance carrier summarily cancelled 4,000 Northern California physicians' group coverage, triggering a month-long physicians' strike that suspended elective surgery, cut patient admissions, and dried up hospital cash flows on both sides of the San Francisco Bay.

Research Bricolage

The doctors' strike created a natural experiment—a serendipitous opportunity to tap the data Meyer had gathered previously, use it to characterize the prestrike attributes of hospitals in the research sample, and then predict how they would respond differently to the exigencies of the crisis (Meyer 1982). However, devising a satisfactory approach for observing these crisis responses posed a methodological dilemma. By jolting each hospital far from equilibrium, the strike invalidated existing organizational response programs and stimulated unorthodox emergent responses. To observe and understand these responses, it seemed essential to adopt an ethnographic approach, becoming deeply immersed in ongoing events within one hospital. However, to systematically compare responses across the entire sample, it seemed essential to conduct structured interviews, distribute questionnaires, and gather budgetary data to gauge the strike's effects.

There was little time for reflection and none for recruiting additional colleagues to help gather data. The hospitals' leaders had already begun sizing up the situation and forming their first responses—and these initial assessments and actions seemed certain to constitute extremely important data.

The methodological dilemma was resolved through improvisation and compromise. An abbreviated form of each methodology was invoked in a sequence proceeding from naturalistic observation, to comparative analysis, to theory building.

(1) *Brief Ethnography*. The prime objective was to get into the field immediately and find out what was happening. Using the notion of theoretical sampling, one hospital displaying each of Miles and Snow's three stable strategic configurations was chosen. A technique for doing "brief ethnography" was concocted on the fly. "Methods of gathering data were varied deliberately. These included telephone calls, prearranged interviews conducted in offices, and impromptu conversations in hospital corridors and waiting rooms" (Meyer 1982, p. 517). One method that proved especially valuable was to haunt hospital cafeterias and join conversations among groups of residents and nurses who were taking lunch and coffee breaks. For three weeks, Meyer moved between the three hospitals, spending about half a day at each one before moving on.

(2) *Structured Open-Ended Interviews*. The next step was to build on the brief ethnographies by securing comparable data across the entire sample. A standard set of questions was developed to assess the strike's impact

and hospitals' responses. These questions were posed in face-to-face and telephone interviews with CEOs of each of the other 16 hospitals. The interviews were conducted immediately after the strike ended, and CEOs' responses allowed detailed reconstructions of sequences of events occurring before, during, and after the jolt.

(3) *Content Analysis*. Quantitative outcroppings of key constructs were then educed from content analysis of the CEOs' interview responses. For instance, numerical indicators of forewarning and organizational learning were constructed.

(4) *Archival Data*. The jolt's financial and operational impacts and outcomes were assessed by gathering archival data that contained prestrike budgetary projections of hospital occupancy, staffing, and cash flows. This enabled calculation of variances between projected values and observed values, both during and after the strike.

(5) *Analysis and Reporting*. Analysis proceeded by triangulating between the study's observational, interview, and quantitative data. In the article reporting the study (Meyer 1982), a narrative approach was used to recapitulate the research process—beginning with brief ethnographic accounts of adaptive responses of the three CEOs, adding detail gleaned from analysis of interviews, conducting simple statistical analyses of baseline and archival data to tap antecedents and consequences, culminating in the construction of a process model of organizational adaptation to environmental jolts.

Meyer's original conceptual framework was another casualty of the nonequilibrium conditions he encountered. When he entered the field, Meyer assumed that "fit" or "alignment" would exist. That is, he presumed that each hospital had already come to terms with its environment by locating and carving out a local equilibrium, placing itself in alignment with its enacted environment. In the face of disequilibrium, it became imperative to model the realignment process explicitly. Meyer turned to organization theory in search of a process model of organizational adaptation. He synthesized one (Meyer 1982, pp. 520–521) by combining "the stimulus-response paradigm and the variation-selection-retention mechanism (Campbell 1969) in proposing that when jolts emanate from environments, organizations select and interpret stimuli according to theories of action (Argyris and Schön 1978) encoded in prevailing strategies and ideologies (Miles and Snow 1978, Starbuck 1982)." These filtered stimuli were posited to then elicit responses that exploited caches of slack resources.

Drawing Lessons from Jolts

Studying the hospitals' responses to an unexpected shock had taught us two kinds of lessons: (1) lessons about the effects of the phenomenon itself and (2) lessons about conducting research during episodes of disequilibrium.

Three Lessons About Jolts. First, we found that jolts expose the power of shared beliefs and ideologies. In shaping organizations' responses away from equilibrium, these factors exerted more powerful influences on the hospitals' behavior than the organizations' prior structures, domains, or stockpiles of slack resources (Meyer 1982). Second, we saw that jolts create clandestine opportunities for change. They disrupt organizational programs, create panic, energize members, mobilize advocates for change, and legitimate unorthodox moves. Third, we found that jolts offer teachable moments to organizations. They constitute natural experiments not only for organizational researchers, but for perceptive organizational actors as well.

Lessons About Research—Dropping Your Tools. Meyer's study of the physicians' strike showed that there are advantages to studying organizations whose fields are in flux, but to capitalize on these opportunities, researchers need to drop their familiar research tools. We distilled the following rules of thumb from the study: (1) Get out into the field right away: Jolts' initial impacts and social actors' initial responses provide especially rich data. (2) Raise the study's level of analysis: If your focus is on individuals, reframe your unit of analysis as individuals in groups; if you are doing a comparative analysis of a large sample of organizations, augment your research design with other methods allowing you to observe the larger environment in which these organizations are embedded. (3) Narrow your scope of observation: Use theoretical sampling (Eisenhardt 1989) to select promising exemplars for intensive observation. (4) Shift your orientation from explaining variance to apprehending process (Mohr 1982): This will entail gathering more observational and qualitative data, switching to more longitudinal and temporally sensitive methods, and attending to unfolding events instead of verifying enduring relationships (Van de Ven and Poole 1990).

Study 2: Responding to Hyperturbulence

Sometimes researchers get second chances. In 1987, 12 years after the doctors' strike had jolted San Francisco hospitals, Meyer returned to the scene of Study 1 to begin a 4-year longitudinal field study of organizational change and redesign. He embarked on this new research expedition in the company of five esteemed colleagues, assisted by two doctoral students, and supported by extramural research funds.[1]

Research Baggage

Best of all, this time we knew what to expect. The team's ambition was to test and extend the process model of organizational adaptation to environmental jolts educed in Study 1. CEOs of 30 hospitals supplied baseline data for Study 2 and agreed to participate in a series of eight structured interviews between 1987 and 1991. We designed Study 2 as a quasi experiment to exploit naturally occurring environmental changes. Metrics for tracking environmental change were developed. Our plan was to "observe hospitals' antecedent states, wait for the environment to change, watch the hospitals adapt, and then evaluate the effects of various antecedent-adaptation combinations on subsequent levels of performance and on the hospitals' final configurations of strategy, structure, ideology, and slack" (Meyer et al. 1993a, p. 70). We hoped to augment our earlier findings about adaptation to environmental jolts by observing adaptation to gradual and incremental environmental changes as well. A peer reviewer of our grant proposal asked, "what will you do if the environment does not change?" We assured the reviewer that our prior fieldwork convinced us that the health care sector would not remain in equilibrium long.

Be Careful What You Ask For. And so we set out, confident in our ability to capitalize opportunistically on environmental change. Once again, we failed to anticipate the type and the intensity of change that we encountered in the field. Instead of adapting to secular trends or environmental jolts, in mid-1987 the California health care sector was undergoing a cataclysmic upheaval—a step-function-like change that outstripped top managers' understanding and overwhelmed their organizations' adaptive capacities (Meyer et al. 1990, Scott et al. 2000). These changes were nonlinear—they breached boundaries that had partitioned health care into a medical subsector dominated by physicians, an insurance subsector dominated by financial institutions, and an acute-care subsector dominated by hospitals.

Early in our first wave of interviews with hospital CEOs it became obvious that Study 2 was skating on thin theoretical ice. The conceptual framework developed in Study 1 treated organizations' adaptation to environmental jolts as equilibrium-seeking responses, but the California health care sector was not in equilibrium in 1987, and it remained far from equilibrium throughout our four-year study. So we found ourselves scrambling once again to locate new theory and patch up our research design. Having abandoned the presumption of static equilibrium, we had carefully designed and instrumented our research to track the behavior of organizations adapting in pursuit of a quasi-stationary equilibrium.

Our problem, recognized only much later, is that we still were encumbered by our allegiance to variation/selection/retention as the underlying mechanism of change. More fundamentally, we were mired in the conceptual assumptions of "general linear reality" (Abbot 2001). In the next section, we interrupt our tale of the field to summarize this pivotal idea.

Transcending General Linear Reality

In an incisive essay, Andrew Abbott (2001) explains how Newtonian mathematics has subtly shaped sociologists' thinking about how society works. The general linear model (GLM), Abbott notes, has spawned a family of formidable and effective analytical techniques that have reshaped empirical research and achieved methodological hegemony within the discipline. However, to use GLM methods "to actually represent social reality one must map the processes of social life onto the algebra of linear transformations" (Abbott 2001, p. 39). The general linear model makes one particular variable linearly dependent on a set of antecedent variables up to an error term: $y = Xb + u$.

Statistical assumptions, originally adopted to estimate this equation, have been recast as philosophical assumptions about how the social world works. The resulting mindset, which Abbott calls "general linear reality" (GLR), consists of deeply held causal beliefs that treat linear models as representations of the actual social world. To paraphrase anthropologist Edmund Leach (1964), the world becomes a representation of our methodological tools, not vice versa.

Abbott unpacks six fundamental assumptions embedded in the GLR mindset: (1) *Fixed entities with changing attributes*: The GLR social world consists of stable entities (the researcher's units of analysis) that have changing attributes (variables). (2) *Monotonic causal flows*: Linear interactions between attributes cause outcomes, themselves measurable as attributes of the stable entities. Causes are assumed to flow from large entities or attributes to small ones and to remain equally effectual over time. GLR does not allow small things to cause big things, or ephemeral attributes to cause enduring attributes. (3) *Unequivocal meaning*: A given attribute of an entity is presumed to have one and only one causal meaning, at least in one study. (4) *Irrelevance of sequence*: The order in which things happen does not influence the way they turn out. (5) *Casewise independence*: Although the independence of cases and variables are commonly seen as statistical assumptions, they also beget conceptual presuppositions, even though this flies in the face of the common observation that attributes of social entities tend to cluster in configurations, archetypes, or gestalts (Meyer et al. 1993b). (6) *Independence of context*: A final presupposition of GLR is that "the causal meaning of a given attribute cannot, in general, depend on its context in either space or time. Its effect does not change as other variables change around it, nor is its causal effect redefined by its own past" (Abbott 2001, p. 56).

In data analysis, there are ways of relaxing some of these assumptions without abandoning the GLM. Interaction terms, controlling for collinearity and correlated errors, temporal dummy variables, and instrumental variable estimation are familiar examples. However, as Abbott points out (2001, p. 58), "although the GLM itself can handle a few interactive effects or temporal dependencies when used with suitable care, GLR as a way of thinking has a harder time with them."

The upshot is that the GLR mindset and the kindred concept of equilibrium have jelled into a self-sealing constellation of theoretical, methodological, and philosophical assumptions. At this point, we return to our account of Study 2 to illustrate some difficulties that this constellation can create for organizational researchers.

Back to Bricolage

Although we did not grasp the root causes of our struggles in studying the hyperturbulent health care field of 1987, we did recognize the need to drop our tools again and resort to bricolage. A search of the theoretical literature on field-level change turned up two promising articles. Astley (1985) proposed "punctuated equilibrium" as a mechanism whereby new organizational populations are born and old ones die—so we substituted "punctuational change" for "equilibrium-seeking" in our assumption set. McCann and Selsky (1984) had offered the concept of "hyperturbulence," a condition in which environmental changes exceed the collective adaptive capacities of inhabitants—so we jettisoned the variation/selection/retention version of organizational adaptation and substituted hyperturbulence. In making these substitutions, we had shifted our attention from changes occurring within organizations to changes occurring throughout an organizational community. We began thinking of organizational evolution as proceeding at an episodic rather than a gradual tempo, producing discontinuous rather than continuous change. "Instead of replacing their ancestors through a steady process of transformation, new populations diverge to coexist alongside their ancestors until the latter are suddenly extinguished" (Astley 1985, p. 230).

After repacking our theoretical bags with these ideas, we turned to the matter of research design. We abandoned, with deep regret, our elaborate quasi-experimental design, and fell back to grounded theory as a methodological frame for observing the processes driving the community's evolution. We added new questions addressing community-level changes to our interview schedules, recruited new informants who occupied community-level vantage points, and returned to the field for our next wave of data collection.

Lessons from Hyperturbulent Fields

Study 2 continued for three years, and so did the hyperturbulent conditions. We left the field sadder but wiser, having learned some lessons about hyperturbulent fields, and others about studying organizations far from equilibrium.

Three Lessons About Hyperturbulence. First, Study 2 highlighted the multilevel nature of change processes. The study's initial design took the organization as its principal unit of analysis, but experiences in the field led us to add two more inclusive units—the industry and the interorganizational network. At each of these levels, we encountered discrete systems changing according to their own dynamics. At the organization level, top management teams formulated strategies intended to align their hospitals with industry conditions. At the industry level, boundaries shifted and were breached as rivalry intensified. At the interorganizational level, "competitors" were drawn into networks of symbiotic relations that overlaid competitive relationships with collaborative and sometimes collusive ones. Although change processes at these different levels were distinct, events on one level influenced events on other levels.

Secondly, Study 2 instructed us on the connection between the direction of causation and levels of analysis. As noted, the study's original ambition was to observe organizations seeking equilibrium, a pursuit that at least tacitly accepted the GLR premise that little things do not cause big things—causality flows from the contextual to the specific (Abbott 2001). A logically related premise holds that little things *change faster* than big things. "Variables at lower levels of analysis [usually] change at least as rapidly as variables at higher levels, and often change a good deal faster" (Freeman 1978, p. 343). Normally, we expect individual humans to be capable of changing faster than organizations; we expect organizations to change faster than the populations they constitute; we expect organizational populations to change faster than the ecological communities they inhabit. Indeed, these expectations are a logical necessity for equilibrium seeking on the part of adaptive agents. However, Study 2 demonstrated that this presumed hierarchical ordering to rates of change is not immutable. When social systems are far from equilibrium, changes unfolding at the level of a particular field, market, or organization can outstrip rates of change at either lower or higher levels. Our Study 2 fieldwork certainly demonstrated that nonlinear changes within fields can outdistance organization researchers' best efforts to understand them.

Finally, Study 2 taught us that nonlinear changes engender collective action. The primary manifestation was the swift formation of interorganizational networks. Clusters of hospitals linked up and took collective action, and in so doing, they shaped their future environments. Intraindustry linkages superimposed cooperative relationships on competitive relationships, and ushered in entirely new strategic options. Transindustry linkages joined unlikely organizational partners in symbiotic relationships that breeched and redrew industry boundaries. As resources grew scarcer, networks turned into social enclaves, and the collective action of networked members depleted the resources available to nonmembers. Organizational collectives influenced the context that influenced them, so they played an active role in processes of emergence. Collective interpretations and actions in a field away from equilibrium create the conditions that the actors subsequently encounter.

Studying Organizations Away from Equilibrium.[2] In accord with the canons of general linear reality, as graduate students each of us learned to partition the research process into sequential stages: conceptualizing, designing, observing, analyzing, and reporting. During the conceptual and design stages, researchers are enjoined to make choices that will remain in effect throughout the inquiry. They are directed, for instance, to identify theoretical models, select units and levels of analysis, specify dependent and independent variables, choose sampling frames, and so forth. During the subsequent stages of observation, analysis, and reporting, these parameters are immutable. To change them on the fly could contaminate data or be interpreted as scientific fraud. Stigma attached to "post hoc theorizing," "data mining," and "dust-bowl empiricism" are handed down from one generation of GLR researchers to the next.

However, as Study 2 progressed, first one research design parameter and then another slipped the shackles of experimental control and started acting like a variable. Efforts to keep our sample of hospitals intact, for example, were beleaguered by mergers, acquisitions, exits, and CEO turnovers. Sample attrition would force us to discard hard-won data and introduce nonresponse bias should the organizations or informants who dropped out differ in analytically important ways. So, we worked hard to maintain cordial relations with our CEO informants, and when successions occurred, we worked doubly hard to "reenter" by building rapport with the incoming CEO. Despite these efforts, the study's organizations and informants varied over time in ways that introduced unknown biases. The lesson is that samples are moving targets when organizations are far from equilibrium. The GLR assumption of *fixed entities with changing attributes* must be relaxed, allowing researchers to invent designs treating organizations' beginnings, endings, alliances, amalgamations, and boundaries as variables rather than as parameters (Abbott 2001).

Other research principles were violated in our study as well. One transgression involved violating the GLR assumption of *unequivocal meaning* by using different theories of causation at different times to explain the same phenomenon. In a sense, theoretical models took on the role of dependent variables in our study: "Are relationships observed between variables X and Y at T_1 better explained by natural selection, boundedly rational choice, or mimetic isomorphism? Which causal model offers the best explanation at T_2, T_3, and T_4?" Another infraction arose from our practice of periodically changing the operational definition of the variable "organizational performance" as industry restructuring altered the *meaning* of effective performance. In effect, the question, "What is your dependent variable?" became an empirical question, not a theoretical one. This underscores the seriously debilitating effects of the GLR assumption of *independence of context* when organizational fields are in flux.

Much like our CEO informants, we found that the burst of changes punctuating the industry equilibrium created paradoxes and violated assumptions of the GLR schemata we used to frame and interpret our worlds as social scientists. Like our informants, we were forced to act first and think later, as we struggled to discover the implications of our actions and the meaning of the data they had elicited. Sometimes researchers justify code-of-conduct violations such as these by labeling the enterprise as "exploratory" work intended to "build grounded theory." However, even this avenue was closed to us because as Karl Weick sympathized (1992 personal communication) "you can't build grounded theory while the ground is moving."

Study 3: Diffusion of Innovation in an Oscillating Field

Our third study (Gaba and Meyer 2005) investigated the diffusion of an administrative innovation. We set out to study how venture capital (VC) practices originating in small private VC partnerships had diffused among the population of large information technology (IT) corporations. IT corporations adopt VC practices by establishing corporate venture capital (CVC) programs. CVC programs are housed in dedicated subunits charged with making direct minority equity investments in portfolios of high-potential start-up ventures. These programs emulate private VC practices and processes, and corporations typically justify their adoption on strategic grounds. Top managers say they expect CVC programs to complement or substitute for in-house research and development activities—in effect, outsourcing R&D and opening a window on disruptive new technologies (Mowrey 1999). Corporations often find it difficult, however, to replicate venture capital practices and outcomes—many of these practices are encoded in tacit knowledge, and some clash with corporate systems and cultures (Gaba and Meyer 2005).

Research Baggage

Our intended unit of analysis was the information technology corporation. However, as noted above, Study 2's hyperturbulence had driven home the need to track changes at multiple levels, and to be on the lookout for collective action emerging within the organizational community in which a focal organizational population is situated. Accordingly, we began a second, concurrent study to inform us about the context of corporate venturing.

A Natural History of the Venturing Community. We approached this second project as a sort of "natural history" of the venture investing community. As characterized in the organizational ecology literature, this community is analogous to an ecosystem inhabited by interdependent populations of organizations displaying distinct forms (Baum and Singh 1994). Using grounded theory (Strauss and Corbin 1997) and historical methods (Isaac and Griffin 1989), we categorized the social actors in the community and studied their habits, interactions, and coevolution. We used two methods. The first was a fairly straightforward analysis of secondary financial data and business journalists' published articles. Our second method, which we regard as more innovative, was to infiltrate swanky networking conferences where denizens of the venture investing community gather to exchange business cards and best practices. Between 2000 and 2004 we attended a series of seven "corporate strategic investing" conferences that attracted representatives of all of the major social fauna that comprise the community ecosystem: corporate investors, venture capitalists, investment bankers, angel investors, journalists, accountants, and lawyers. By pursuing a campaign of shameless ingratiation, we persuaded the conference organizers to waive stiff registration fees, house us at posh conference hotels, provide lists of registrants, and audiotape conference programs for us. As luck would have it, our series of corporate venturing conferences spanned a period when an unprecedented spike in venture capital investing was followed by an unprecedented drop. Thus, we observed and documented a period of rapid adoption of corporate venturing programs, followed by a period of rapid decline and discontinuance.

Modeling the Diffusion of Corporate Venturing. However, while pursuing our community-level "natural history," we were hard at work on the original project that focused on CVC program adoption by IT firms. This study was grounded theoretically in the innovation diffusion literature (Strang and Soule 1998). Drawing on social learning theory (Greve and Taylor 2000) and institutional theory (Tolbert and Zucker 1983), we developed hypotheses predicting which corporations would adopt next as this innovation spread. We obtained a sample of 253 IT corporations, and tapped a wide range of archival sources to construct a longitudinal data set covering the years 1992 to 2000 (Gaba 2002).

Having hypothesized dynamic relationships, we selected event history analysis (Strang and Tuma 1993) as our analytical method. To isolate the impacts of theorized valuables on adoption, we followed standard research practice by measuring a number of potentially confounding variables and including them in the analysis as controls. As noted above, we had conceived of CVC as an administrative innovation adopted by IT firms to pursue strategic objectives. VC investments can, of course, generate lucrative financial returns as well. It is conceivable that IT executives pay lip service to strategic objectives, but actually adopt CVC programs in pursuit of VC-like financial returns, hoping that start-ups' initial public stock offerings or acquisitions will multiply their investments many times over. Accordingly, to isolate firms' strategic motives for adoption, we included control variables indexing the current level of venture investment, the number of VC-backed initial public offerings (IPOs) and stock prices on the NASDAQ—proxies for the fluctuating financial allure of venture capital investing.

When we ran our first event history models, the results were disappointing. We obtained large coefficients for control variables indexing current VC activity—which we had introduced merely to isolate strategic aspects of corporate venture investing. Coefficients for our hypothesized variables, on the other hand, explained little variance in adoption. Moreover, exploratory analyses showed that the parameters of our models were highly unstable over time—when we evaluated the antecedents of adoption separately for the early and late years in our time series, the results implied that entirely different causal processes were afoot.

However, while we puzzled over these results in the spring of 2000, the economy's Internet and telecommunications sectors were collapsing, taking both private and corporate VC investing down with them. As noted above, our ongoing "natural history" afforded us front row seats on this disintegration. Based on striking differences we had observed in the community during the rise and fall of venture investing, we concluded that it was very likely that different causal processes had been at work within the IT population at different points in time between 1992 (when venturing activity was at a low point) and 1999 (when it peaked).

The bad news was that our research team had been walloped once again by a field-level change. The good news was that the "natural history" project we had running in the background provided insight into the forces at work and the meaning of the players' moves. In fact, as part of the venturing community project, we had located and begun collecting the economic and financial data that would turn out to be invaluable in importing the "venture capital cycle" into our theory, data set, and analysis. Ultimately, this saved a laboriously constructed research project from winding up on the scrap heap.

Back to Bricolage: Regrouping with a Two-Stage Research Plan

We dropped our tools, unpacked our bags, and took stock. Among our conceptual trappings we found assumptions that clashed with the phenomena under observation. Upon inspecting our methodological paraphernalia, we realized that we would have to dismantle our research design and rebuild it.

Equilibrium seeking had once again proved a debilitating presupposition: We had regarded CVC programs as variations that would be selected and retained by IT corporations, propelling the IT population toward a new competitive equilibrium. However, our naturalistic observations seemed to show positive feedback processes locking the venturing community into perpetual disequilibrium (Prigogine and Stengers 1984). Our GLR presumption of *independence of context* compounded this problem: We had thought it possible to carve the IT population out of the venturing community and model the intrapopulation diffusion of CVC programs in isolation. However, observation convinced us that this population-level process was overwhelmed by the community-level VC cycle, and that diffusion had to be modeled at the level of the larger community.

When we designed the diffusion study originally, we believed that we had taken field-level temporal instability into account by controlling for the level of venture investing and the number of VC-backed IPOs. In doing this, we were unwittingly buying into the GLR assumption of *monotonic causal flows*. Specifically, our analytical strategy presumed temporal and spatial homogeneity, that is, we assumed all IT firms would display similar propensities to adopt CVC programs, irrespective of time and location. However, our naturalistic observations disclosed nonmonotonic temporal and spatial influences, exogenous to the IT population, apparently tied to field-wide expansion and contraction in VC activity.

In sum, we concluded that CVC diffusion could not be understood without broadening the scope of observation to encompass the larger set of organizational forms and populations that make up the VC community. We reconceptualized CVC adoption as a form of diffusion that occurs within this coevolving community, is subject to highly cyclical levels of aggregate activity and emerges from the interplay of nested social units. Because we were convinced that community-level dynamics strongly affected population-level diffusion, we realized that we needed to begin our analysis at the community level and then model our way down. Therefore, we broke the project into two discrete stages.

Stage 1: Coevolution of the Venturing Community. We began with a causal analysis of the coevolutionary dynamics that we hypothesized would link the VC investing behavior of two distinct organizational populations: private VC firms and IT corporations. We defined coevolution as occurring only when direct or indirect interactions of two evolving units produce an evolutionary response in *each* unit (Roughgarden 1996). Unlike a single population's adaptation to an exogenous environment, coevolution among two or more populations can produce reciprocal evolutionary responses that either thwart these adaptive changes or magnify their effects in mutualistic interaction.

Our core research questions were straightforward: Are the levels of venture investment undertaken by the two populations causally related over time? If so, are the causal processes (a) unidirectional or reciprocal, (b) instantaneous or lagged, (c) equilibrium-seeking (evidenced by deviation-absorbing negative feedback loops), or perpetually in disequilibrium (evidenced by deviation-amplifying positive feedback loops)?

For each population, Gaba collected time-series data measuring aggregate monthly venture investments in the IT sector between 1992 and 2001. She obtained other data indexing the NASDAQ stock market's monthly performance, the aggregate monthly value of new venture-backed initial public stock offerings, and various control variables.

To bring these data to bear on our research questions, we had to abandon the GLM, and replace it with some form of analysis where (1) interdependence can vary over time, (2) the direction of causality cannot be established a priori, and (3) feedback effects can be modeled. We settled on an analytic technique that has only occasionally been used in organization science—the vector autoregressive technique (VAR) developed in the time-series analysis literature (Vandale 1983, Hamilton 1994). VAR models are forecasting tools widely used to estimate complex, interdependent systems of variables. They can go beyond the usual dependent-independent variable dichotomy to uncover causal relationships, and to investigate how shocks affect the dynamics of a system of related variables. Full details of our application of VAR and its results are reported elsewhere (Gaba 2002); here we just summarize selected findings.

Lessons About Coevolution. Our VAR results strongly support the hypothesized coevolutionary relationship between private and corporate venture investing. The findings suggest that investments made within each population move in cyclical patterns. These cycles affect each other, and they are, in turn, linked to movements in the capital markets. We found evidence for a positive feedback relationship between private and corporate venturing, with IPO and stock markets driving the investments of private VC firms.

The picture that emerges looks like this: Buoyant stock and IPO markets stimulate venture investment deals within the private VC population. Positive feedback effects between the two populations—private and corporate VC—fuel a boom in venture investing with corporate and private venture deals having mutually enhancing effects on each other (Gaba 2002). However, positive feedback is inherently unstable and drives the system above its long-run sustainable level (Baum and Singh 1994). With too many dollars chasing too few quality deals, a crash is inevitable. During the downturn, the positive feedback loop works in the opposite direction, accelerating the decline in investment.

Stage 2: Diffusion in the IT Population. These findings led us to completely reconceptualize our study of CVC program adoption in the IT sector. Initially, we had approached diffusion as an *intra*population process, reasoning that IT corporations' adoption decisions were based primarily on observing, learning from, and imitating their IT peers. Our analyses of venturing community dynamics led us to begin thinking about diffusion as an *inter*population process, one where CVC programs jump from the VC population into the IT population.

Our reformulated model (Gaba and Meyer 2005) proposes that CVC program adoptions are coproduced by organization-level factors and community-level factors. We still expect a corporation's proximity to Silicon Valley's VC cluster, age, and prior experience in venturing to affect its adoption decisions. Now, however, we expect these factors to be drawn together into causal webs that change in response to swings in the community's aggregate venturing cycle. Instead of monotonic relationships, we hypothesize threshold effects (Granovetter 1978, Schelling 1978) as movement through the venturing cycle triggers shifts between different causal regimes, changing the profile of the corporation that is most likely to be the next to adopt a CVC program. Our analyses suggest that as the community crosses a series of thresholds, the profile of the IT firm at greatest risk of adopting will change. It appears that there is no single tipping point, but multiple thresholds, each of which reconfigures the operative causal model to exert stronger bandwagon pressure.

Lessons About Diffusion. Study 3 teaches several interrelated lessons. These concern *cross-level analysis, endogenous changes*, and *time-dependent causes*. The cross-level analysis lesson is that field-level dynamics can govern population-level dynamics in shaping organization-level action. Our redesigned study of CVC diffusion investigated the connection between macro-(venturing community-level) and meso-(IT population-level) processes in driving micro-(IT firm-level) adoptions. We found that in this particular setting, changes associated with the macro venturing cycle trigger nonlinear shifts between different population-level diffusion processes. In effect, the history of the IT population is partitioned into distinct causal eras by the VC cycle (Isaac and Griffin 1989). Swings in this cycle propel the IT population across thresholds that abruptly change the causal texture of CVC diffusion.

A related lesson is that organizational communities can be transformed by bottom-up, endogenous processes. Prior researchers have usually attributed community-level transformation to exogenous events like changed regulatory regimes or technological breakthroughs. Our work shows that organizational communities can also be transformed by endogenous processes. We conclude that the instability of the VC community arises mainly from endogenous forces welling up from the interdependence of the corporate and the private VC populations. Past research on coevolutionary dynamics has been preoccupied with population-level birth and death rates, paying far less attention to the mechanics of the underlying processes generating these events. These studies implicitly take an equilibrium view of the community ecosystem, ignoring its variability over time and space. However, at least in oscillating fields, the central dynamic of community-level change is not adaptation toward optimal fitness, but a more complex coevolutionary process.

In short, we verified Weick's (1996) speculation that researchers carrying heavy GLM tools move too slowly and with insufficient agility to keep up with rapidly coevolving fields. For many of us, GLM tools and GLR assumptions have become fused with our identity as scholars. This makes these tools almost impossible to drop, preventing us from seeing and studying the myriad of situations to which they do not apply.

Study 4: Applying Our Lessons in an Emerging Field

In 2002, two members of our group flew to Palm Springs to attend the world's first nanotechnology investing conference. Fueled by over a decade of federal funding of basic research, scientists had been working down at the submolecular level, atom by atom, creating materials and structures with unprecedented physical, chemical, and biological properties. Between 1995 and 2001, the number of nanotechnology patents issued in the United States tripled. This stockpile of intellectual property began sparking the formation of start-up ventures aiming to commercialize it.

An emerging consensus held that sooner or later, nanotechnology would catalyze a lot of new economic activity. Like all emergent sectors, nanotech confronted an amorphous market riddled with technology and business risks. No dominant designs were locked in, no standards had been set, few products had moved beyond proof of concept, and in 2002, potential profits lay well in the future. The embryonic "nanobusiness" sector had no incumbents, no accepted set of social or commercial rules, and no governance structure.

Nonetheless, in February 2002, the sector's upside potential lured 264 people to Palm Springs to attend a first-ever "Nanotechnology Investing Forum." Would-be players converged from different occupational and organizational worlds—entrepreneurs, university researchers, scientists from national labs, technology transfer professionals, federal funding agency administrators, venture capitalists, and corporate business development specialists. Miscellaneous camp followers were also on hand—accountants, consultants, headhunters, lawyers—and two organizational scientists.

This conference marked the beginning of our fourth study. It is a real-time investigation of the emergence of an organizational field. We hope to learn something about how new economic sectors materialize, how they acquire structure, and how they evolve. We left the conference convinced that the key to each of these processes was the formation of social networks. "Networking" was the event's raison d'être, and we came to regard this conference as a microcosm of the emerging field—a sort of "Sim-City" for the nanotech investing community. How, we asked, do networks of social relations, informal partnerships, and strategic alliances bring order to a chaotic rabble of individual agents?

Research Baggage

Planning for Study 4 was shaped by our past misadventures and methodological bricolage. We were tired of unpacking our bags out in the field, abandoning our research tools, and improvising. The foray for Study 3 into an oscillating field had shaken our ontological commitment to equilibrium and linearity, so at the start of Study 4, we resolved to drop these assumptions altogether—and confront the fundamental indeterminacy, novelty, and disequilibrium that accompany field emergence.

We traveled light. Theoretically, we selected two core concepts that resonated with our initial observation of the nascent nanotechnology sector: *field structuration* and *complex adaptive systems*. The concept of field structuration (Giddens 1979, DiMaggio and Powell 1983, Scott et al. 2000) references field-building processes wherein social actors construct increasingly coherent patterns of interaction, information sharing, mutual awareness, and shared governance—precisely the processes we had seen unfolding at the nanotech investing conference. Complex adaptive systems (CAS) models offer a powerful new set of conceptual and methodological tools for modeling nonlinear interactions (Anderson 1999, p. 216). Their four key elements are "agents with schemata, self-organizing networks sustained by importing energy, coevolution to the edge of chaos, and system evolution based on recombination"—features that squared dead-on with our observation of the emerging nanotech field.

We opted for a two-pronged research design. First, we are once again pursuing the natural history approach we developed in Study 3. This allows us to track the emerging field at multiple levels, cataloging actors and their interdependencies while remaining agnostic about which units will ultimately prove most compelling and research relevant. Second, having noticed that many interpersonal social relationships forged at the nanoinvesting conferences that we attended were precursors of formal interorganizational alliances, we are systematically mapping this alliance network from its inception.

A Natural History of Nanotech Investing. In our role as naturalists of the social system, we are exploring the institutional infrastructure forming to promote and exploit the commercial opportunities afforded by nanotechnology. We are taking an unabashedly social constructionist stance. Unlike economists who regard technologies, markets, and industries as preordained outcomes of the material conditions of economic competition; we view these entities as collective products of intentional human agency. From our perspective, organizations, markets, industries, and technologies emerge as shared beliefs harden to form strategic recipes and competitive norms, as boundaries are negotiated and dominant designs are selected, and as collective cognitions converge to create a sense of shared identity. Only when this field structuration project has run its course do start-up ventures, industry structures, market segments, and technological regimes assume an aura of concrete reality.

Our naturalistic methods are modeled after those we developed in the corporate venturing study. We are collecting ethnographic data by attending nanobusiness conferences, audiotaping formal programs, conducting open-ended field interviews, and plying related field research methods. In addition, we are gathering archival data in the form of periodicals, online newsletters, and proprietary databases. To date we have attended six nanobusiness conferences. We have dubbed these "field-configuring events"—which we define as settings where people from diverse social organizations assemble temporarily, with the conscious, collective intent to construct an organizational field. These events are microcosms of nascent technologies, industries, and markets. They are places where business cards are exchanged, networks are constructed, reputations are advanced, deals are struck, and standards are set.

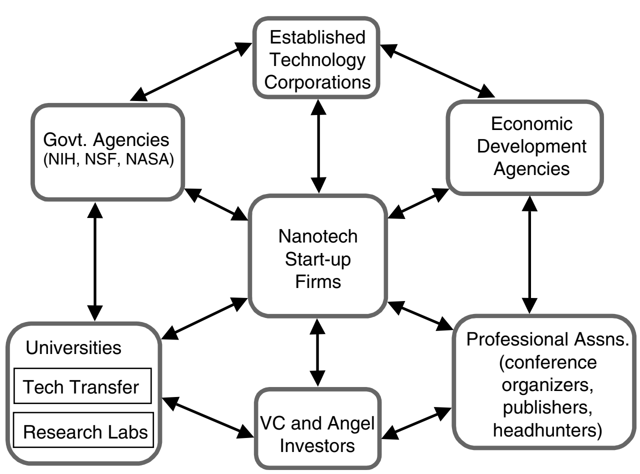
Institutional entrepreneurs (DiMaggio 1991, Fligstein 1997) are becoming a particular focus of our natural history. A spate of trade associations, regional economic agencies, networking organizations, and social movements are actively engaged in field-configuring projects targeting the nanotech sector. These aspiring institution builders have diverse motives, strategies, and objectives. Most have been established since 2000, and many participants are refugees of the collapsing dot-com and telecommunications sectors (illustrating the recombinatory processes posited by CAS models). Institutional entrepreneurs compete for turf, members, meaning, and media attention, while collaborating in promoting societal legitimacy for nanotechnology and financial support for its commercialization. Working as natural historians has convinced us that these field-configuring events afford great sites for observing how "networks of relationships at one level are embedded within and articulate networks at other levels" (Kilduff and Tsai 2003, p. 66).

Mapping the Nanotech Alliance Network. Social network analysis is a well-established technique in organization science, and it came as no surprise to find that most prior network studies anticipated equilibrium and assumed GLR. Most social network researchers scrupulously avoid emergence, electing to study established groups, organizations, or fields. Indeed, most avoid dynamics, examining network structure at a single point in time. Nevertheless, network analysis provides a powerful methodology for analyzing field structuration and tracing the evolution of complex adaptive systems (Anderson 1999, Scott et al. 2000, Chiles et al. 2004).

Having identified the central social actors through our natural history, we proceeded to develop a protocol for mapping the alliance network that is beginning to "structurate" the U.S. nanotech investing community (Colwell 2003). Start-up firms are the central focus. Alliances are crucial to the establishment of nanotech start-ups, serving as conduits for resources, conferring legitimacy, and outsourcing activities lying beyond the competencies of a start-up. To capture the field's emerging network structure, we are building a database that includes all of the organizational forms shown in Figure 1: nanotechnology start-up firms, established corporations, university labs, venture capitalists, federal funding agencies, professional associations, and economic development agencies. By mining proprietary databases and visiting Internet websites (Colwell 2003), we are collecting data on five distinct kinds of ties that link these entities: equity investments, technology licenses, research funding, joint ventures, and research partnerships. We document start-up ventures' network ties from the community's inception in 1990. Our database currently contains 1,004 organizational nodes connected by 2,664 links, along with the date that each node and each tie was established.

Grounding our alliance network study in the organization science literature proved difficult. Most studies

Figure 1 Alliance Networks in the Nanotech Investing Community



taking a network approach to strategic alliances treat the firm as the unit of analysis and firm performance as the dependent variable (Gulati 1999, Gulati et al. 2000). Authors adopt GLR as their ontology and ply the GLM in their empirical analyses. The primary attribute examined in this literature is a focal firm's "ego network"—its set of links to alliance partners. Researchers often regard this network as part of a firm's resource stock—alongside its product lines, brand equity, patent portfolio, and financial capital. The prototypical question is: Does the firm's alliance network generate economic rents?

GLR thinking pervades this line of research—firms are regarded as *fixed entities with changing attributes, monotonic causation* and *unequivocal meaning* are taken for granted, *sequence effects* are ignored, and networks are divorced from their *contexts*. A handful of studies have examined network evolution (cf. Gulati and Gargiulo 1999, Human and Provan 2000), but models adequately explaining the actual formation of social networks are yet to be developed.

In sum, our field's focus has been on networked *organizations*, not on networks *organizing*. At best, findings from these studies offer insight into how alliances forged by well-established firms are associated with performance, but they were irrelevant to the network-forming processes underway in nanotechnology. In this sector, a nanotech start-up doesn't construct an *alliance network*—the start-up's alliance network constructs the *nascent firm*.

We saw that the only feasible unit of analysis for our alliance network project was the entire network, and we concluded that the organization literature offered little guidance. Looking further afield, we noticed that networks were attracting broad multidisciplinary interest as a pervasive organizing principle. Physical scientists were reporting that a variety of complex systems share the same underlying architecture, ruled by basic laws that seemed equally applicable to cells, computers, industries, and society. Barabási and Albert (1999) reported that systems ranging from biological networks (e.g., the protein-interaction network of cells) to human networks (e.g., the World Wide Web) are structured according to two organizing principles: "hubs" and "connectors."

Hubs, Connectors, and Power Laws. Barabási and Albert's network architecture differed sharply from that postulated by the Hungarian mathematicians who pioneered network theory (Erdös and Rényi 1961). For reasons of mathematical tractability, the Hungarians had assumed that network ties form randomly. For the next three decades, most researchers striving to apply network theory to the world unreflectively incorporated this analytical assumption in their analytical models. This random worldview presumed that the "connectedness" of a set of networked nodes (indexed by the number of ties maintained by each node) would be normally distributed. That is, like many other natural phenomena, connectedness measures were expected to cluster around an average, producing the familiar bell curve distribution. In contrast, the distributions of network connectedness observed by Barabási and his colleagues are wildly skewed, conforming to the exponential distributions termed "power laws." Power laws lack the symmetrical peaks and tails of normal distributions. The implication is that such networks consist of relatively small numbers of hubs (nodes with lots and lots of links), along with far more nodes with hardly any links at all. Barabási calls these networks "scale-free" because they have no characteristic scale—small chunks, when magnified, resemble the whole.

Barabási speculates that hubs emerge through "preferential attachment," a nonlinear process arising from positive feedback within self-organizing systems. Power law distributions are the "stationary state of any stochastic process where the probability of an event is proportional to the number of times it has occurred in the past" (Anderson 1999, p. 223). Thus, as networks grow, nodes with existing ties are most likely to attract new ties. Soon, densely linked clusters form around hubs. "Connectors," the second key feature in the topology of scale-free networks, are reminiscent of Burt's (1992) "structural holes." Connectors are nodes linking otherwise isolated clusters, creating the "small world" phenomenon formalized by Watts and Strogatz (1998).

It should be noted that random formation of network ties is an *analytical* assumption, not a *theoretical* one. Few social network researchers spin theoretical stories about links forming by chance, but some pursue GLM analyses that assume they do. Consequently, the networking preferences and choices of human agents "disappear into the magician's hat of variable-based causality, where they hide during the analysis, only to be reproduced with a flourish in the article's closing paragraphs" (Abbott 2001, p. 98).

Building on the work cited above, we are approaching the nanotech investing community as a complex adaptive system, hypothesizing that its structure will display the characteristics of a scale-free network with small-world properties. Our initial analyses support these predictions (Colwell 2003). The links among nanotech nodes are indeed distributed according to a power law, demonstrating the existence of hubs. Our naturalistic observations confirm that this network architecture has not been imposed by a central controller, but is emerging from endogenous characteristics of the network itself. Other analyses document the existence of "connector" nodes that transform the network into a small world, and suggest that the nanotech investing community displays the fractal dimensionality characteristic of a complex adaptive system.

Working Without a Nomological Net. Study 4 is a work in progress, so we can draw no conclusive lessons from the nanotech natural history and alliance network data. Nevertheless, a few observations and conjectures seem in order. First, the project has bolstered our conviction that rigorous research can be done absent the structure supplied by GLR assumptions and GLM methods—rigor does not demand deterministic linear models or statistics describing central tendencies. Second, Study 4 reinforces the merits of multilevel analysis, underscoring the value of marrying a descriptive natural history and a quantitative research design. As it did in our corporate VC study, visiting the nanotech community in the persona of naturalists yielded insight into the nature of change at the field level, and the identities, motives, and roles of key participants. Third, coupling CAS modeling with the structuration perspective offers tantalizing clues about the micro-macro linkages between individual action and social structure. The nanotech investing network appears to be forming in an inherently nonlinear, bottom-up, autocatalytic fashion. Its scale-free architecture is congealing spontaneously, as an unintended consequence of the actions and interactions of a swarm of human agents pursuing a wide assortment of individual-, group-, and organization-level structuration projects. Finally, the alliance network study suggests that, absent equilibrium, observing outliers may be more informative than observing average or typical entities—the handful of nanotech hubs and connectors that we identified probably have much to say about the structure and function of the nanonetwork; the hundreds of weakly connected nodes probably have far less to tell us. "Scientists tend to place too much focus on averages, while the world is full of singularities . . . much of the real world is controlled as much by the 'tails' of distributions as means or averages: by the exceptional, not the commonplace; by the catastrophe, not the steady drip" (Anderson 1997, pp. 566–567).

From this writing, the next steps in our analysis of the nanotech business sector's emergence, structuration, and evolution are themselves in flux. We continue collecting data, working most days in the office mining online databases for new nodes and links to add to our network dataset, but periodically sallying forth to industry conferences to update our natural history of the emerging ecosystem. Plunging ahead without a detailed blueprint for analysis is somewhat unnerving, but the lessons of Studies 1, 2, and 3 stiffen our resolve. If nothing else, they taught us that when studying fields far from equilibrium—where jolts, step functions, and oscillating fields trigger cascades of change—research designs must be evolved, not planned.

Toward Theory and Method for Fields in Flux

The term *heuristics* denotes nonformal methods derived from iterative trial and error cycles that prove useful in solving problems for which no formulas exist. This essay has described methodological heuristics devised while studying organizations inhabiting fields that were far from equilibrium. We collected data from hospitals, technology firms, and nanotech start-ups—embedded in organizational fields undergoing jolts, step-function shifts, oscillation cycles, and emergence. Initially, we viewed these organizations through the lens of GLR, presumed equilibrium, and employed research methods predicated on the GLM, but the nonlinear dynamics we encountered forced us to discard our GLR mindset and drop our GLM research tools.

Early on we made piecemeal adjustments. Conceptually, we replaced assumptions of stationary equilibria with assumptions of equilibrium seeking; we updated our evolutionary frame, sidelining adaptation through selective retention and substituting community-level coevolution. We revamped our research designs, switching from variance modeling to process modeling and from quasi-experimental designs to natural histories. Methodologically, we shifted from ordinary least-squares regressions positing normally distributed data to time-series analyses, interaction terms, and panel studies with fixed-effects models. These patches were only partly effectual. We continued to lurch from one methodological crisis to another. During undergraduate training in economics, each of us had received full immersion in the general equilibrium model, and this may partly account for the slow ascent of our learning curve.

It is a truism that you can only get answers to the questions that you are willing to ask. We suspect that we avoided asking questions about social systems far from equilibrium because they violated dogma about rigorous methodology. Fields in flux are unappealing research settings for GLR scholars. Like earthquake victims, researchers steeped in equilibrium assumptions usually run for cover, wait for the tremors to stop, and then return cautiously to sift through the debris. GLR research may document probable causes of discontinuities, and perhaps enable their prediction—but it cannot elucidate the nonlinear mechanisms that actually drive such change forward. We suspect that prevailing research designs have obscured the fact that some of the most significant variations are distributed across time rather than across organizations, and stem from nonlinear changes impacting entire organizational fields.

By the beginning of Study 4, it dawned on us that our recurring bouts of conceptual befuddlement and methodological bricolage were arising from tacit equilibrium assumptions and the GLR mindset. Exposure to the literature on complex adaptive systems (Anderson 1999) taught us that order in a system does not demand or imply equilibrium because systems can exhibit self-organizing behavior and evolve naturally toward order without ever reaching a steady state. Reading Abbott's papers (1990, 2001) and studying network analysis convinced us that measuring variable attributes of fixed entities (the GLR approach) was impossible in the emerging field of nanotechnology, and that charting the formation of links between interacting agents offered a more promising approach.

Nonlinear change occurs when a response is neither directly nor inversely proportional to its cause. Themes invoked by the notion include process, emergence, and ongoing, perpetual novelty (Arthur et al. 1997). In our own studies of organizational fields, we described nonlinear changes as jolts, step functions, oscillations, and emergence. Others have used the terms turning points (Abbott 2001), thresholds or tipping points (Granovetter 1978), phase transitions (Prigogine and Stengers 1984), increasing returns (Arthur 1989), and network externalities (Katz and Shapiro 1985).

However, while acknowledging nonlinearity, organization science has privileged stability over dynamics, and incremental change over discontinuous change (Haveman et al. 2001). Many scholars have abstracted away nonlinear interaction to protect their equilibrium assumptions and achieve analytical tractability. Yet organizational scientists live in a world where geopolitical upheavals and technological convergence accelerate the emergence of new states, fields, and industries that replace and recombine elements of their predecessors. As our field grapples with these phenomena, we hope that recounting our experiences will help others see disequilibrium and nonlinear change as natural and ongoing rather than as exceptional and episodic.

This is not to foment a revolution. Our intent is not to invalidate GLM analyses, but to complement them. Order can emerge from both linear and nonlinear processes, and persist in both equilibrium and disequilibrium states. Nonlinear assumptions and CAS models are not suited to all research problems and settings, but we believe that new perspectives embracing nonlinearity and disequilibrium can help the field begin mapping episodes of upheaval that have proven intractable within the GLR perspectives that have long dominated the study of social systems.

Concluding Thoughts

This special issue aspires to advance the frontiers of organizational science by reinvigorating and redirecting theory and research. Reflecting on our own forays into organizational fields in flux, we offer the following recommendations.

(1) *Take Time Seriously*. This means conducting longitudinal research, of course, but truly groundbreaking studies will be informed by more nuanced temporal theorizing about cycles, pacing, and event sequences. Data collected at one point in time to index variable attributes of fixed entities can only lead to accounts of stable organizations in static environments. In designing future research, assumptions should be chosen to

reflect empirical facts, not to make models tractable or to defend researchers' professional identities.

(2) *Conduct Historical Research*. We advocate studies that situate social phenomena in their historical contexts. Historical analysis directs attention to turbulent periods that usher in new epochs. History embraces nonlinearity axiomatically, whereas sociology and organization theory evoke equilibrium-based accounts of incremental changes within epochs.

(3) *Treat Research Designs as Experimental Prototypes*. Rigid research designs become a liability when studying social systems that are far from equilibrium. We propose treating designs as renewable licenses rather than fixed constitutions. In specifying units, variables, samples, and even theories, we prefer choices that are temporary rather than permanent, correctable rather than correct, discoverable rather than known.

(4) *Incorporate Nonlinear Concepts in Evolutionary Theorizing*. We envision a new division of labor between linear and nonlinear thinking. The former is preferable when systems are adapting toward equilibrium, the latter during times of system upheaval and emergence. When fields are in flux, we encourage theorists to entertain evolutionary models that go beyond the variation/selection/retention mechanism to embrace notions like coevolution, CAS, field configuration, network formation, autocatalytic feedback, niche evolution, and emergence.

(5) *Design Multilevel Research*. Nonlinear systems cannot be understood without conceptualizing and studying them at multiple levels. At any level of analysis, order can be an emergent property of the interactions of agents at a lower level of aggregation (Anderson 1999). Organizations are entangled in an ecology in which one agent's actions help construct another agent's environment, generating forces that connect social structures at different levels.

(6) *Study Systems in Flux*. Our field has stolidly avoided studying organizations and fields during the turning points when discontinuous changes are afoot. Such nonlinear conjunctures fall between the cracks of our GLR empirical studies. The time has come to capitalize on social systems in flux. Studying nonlinear change at multiple levels over time will, of course, require a profound change in organizational research methods, which Andrew Pettigrew (1987, p. 655) described as "ahistorical, aprocessual, and acontextual."

(7) *So Study Your Guidebook and Check Your Baggage*. Lastly, we would urge our colleagues to reconnoiter carefully before embarking on a study of an unfamiliar organizational field. Learn the context before you go, and make sure you have packed the appropriate gear. Like post-9/11 air travelers, scholars venturing into fields far from equilibrium would do well to carefully check for sharp-pointed GLR tools stuffed absentmindedly into their carry-on bags.

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Endnotes
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[2]This section draws on arguments developed in Meyer et al. (1993a).

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