

Introduction: Silences, and Beginning to Fill Them

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This collection is about how organization comes into being. We mean the word “organization” in the common sense of whole enterprises, the narrower one of formal initiatives within established firms, and the modest one of developing new means of getting old tasks accomplished. We even mean it in the relatively expansive sense of firms cooperating with one another. The collection is, more broadly, about creating order and reshaping it, both within and among firms: it is about what comes between entrepreneurial ideas and actually functioning enterprises and specific activities. This is a subject of obvious interest and significance, its curious neglect in various academic literatures notwithstanding.

The neglect is striking. Even as interest in entrepreneurship has blossomed—in the American economy, in the curiosity of the general reading public, and in the instructional demands of undergraduate and MBA students—there is startlingly little to read about actually getting things organized.¹ The number of studies of entrepreneurial finance has exploded; an essentially sociological literature seeking to link founder or founding team attributes to outcomes is extensive; and courses abound which help students to develop and hone business plans. Recognizing and creating attractive business opportunities are currently among the most active areas of publication in the managerial “A-journals” (the relatively small set of high social science scholarly periodicals which most management academics read and in which ambitious junior faculty strive to publish). To be sure, there are many more conventionally

¹ It says something that one of the best remains Tracy Kidder, *The Soul of a New Machine* (Boston, MA: Little, Brown, 1981), written at a time when 32-bit mini computers were the *dernier cri* in technology and IBM Selectric typewriters were not only the dominant means of text production in business workplaces, but would continue in production for another half a decade and more.

economic analyses of entrepreneurship and industry dynamics and of the effects of entrepreneurship on innovation and overall economic growth. But literature on representative, or even salient, aspects of the means to and challenges of actually developing institutions, including new firms, remains extremely sparse; and creating more of it is apparently not a widespread priority among entrepreneurship academics.²

This relative silence is not confined to entrepreneurship studies. The scholarly literature on organization theory (a more established field, the other obvious home for such research, and a much more empirical undertaking than its name may suggest) is vast. But it too generally investigates ongoing entities, rather than moments of birth, early development, or transition and their associated difficulties.³ This is even true of the considerable subliterature deriving from James March's celebrated article on exploration and exploitation in organizational learning.⁴ The literature on the so-called "liability of newness" deriving from Arthur Stinchcombe's 1965 contribution to the first *Handbook of Organizations* is largely descriptive and retrospective.⁵ The works of the population ecology school are also far too abstract to be particularly helpful.⁶ This continuing oversight in mainstream organization theory is no less surprising given that most new and young firms fail: ongoing organizations are not an unbiased sample of the organizational population at any time.⁷

² Much of this literature describes and analyzes datasets based on samples of surviving firms. On sampling bias concerning one set of relevant questions, see e.g. Tiantian Yang and Howard E. Aldrich, "Out of Sight but Not Out of Mind: Why Failure to Account for Left Truncations Biases Research on Failure Rates," *Journal of Business Venturing* 27(4) (July, 2012): pp. 477–92. There are some panel datasets constructed to at least minimize this problem, the most well-known examples of which are the Panel Study of Entrepreneurial Dynamics I and II databases housed at the University of Michigan (for details on which see <<http://www.psed.isr.umich.edu/psed/home>>). For some representative research deriving from such sources, see the special issues *Small Business Economics* summarized by Peter S. Johnson, Simon C. Parker, and Frits Wijbenga, "Nascent Entrepreneurship Research: Achievements and Opportunities," *Small Business Economics* 27(1) (August, 2004): pp. 1–4 and William B. Gartner and Kelly G. Shaver, "Nascent Entrepreneurship Panel Studies: Progress and Challenges," *Small Business Economics* 39(3) (October 2012): pp. 659–65. The evidence is coarse and apparently unhelpful regarding the process concerns of this text. (The Kauffman Firm Survey (see e.g. <<http://www.kauffman.org/what-we-do/research/kauffman-firm-survey-series/an-overview-of-the-kauffman-firm-survey-results-from-2011-business-activities>>) is superior in some respects but inferior in others.)

³ This is strikingly true of the most recent handbook-like surveys of the field, whose thirty-eight chapters and nearly 900 pages of text give a fairly comprehensive study of the state of play a decade ago. See Joel A.C. Baum, ed., *The Blackwell Companion to Organizations* (Oxford: Blackwell, 2002). The literature overall has not changed significantly in this respect, though there are glimmerings of an exception to the sweeping statement in the text which we will discuss.

⁴ James G. March, "Exploration and Exploitation in Organizational Learning," *Organization Science* 2(1) (March, 1991): pp. 71–87.

⁵ Arthur L. Stinchcombe, "Social Structure and Organizations," in James G. March, ed., *Handbook of Organizations* (Chicago: Rand McNally, 1965): pp. 142–93.

⁶ This literature derives from M.T. Hannan and J. Freeman, "The Population Ecology of Organizations," *American Journal of Sociology* 82(5) (March, 1977): pp. 929–64.

⁷ The conventional wisdom is that 80 percent of start-ups fail. (See, e.g. David Streitfeld, "That's Life at a Start-Up: Jet.com's Strategy," *New York Times* December 28, 2015, Sunday Business

We write as business historians and we see these lacunae as an opportunity. Business history sources sometimes offer essential raw materials in thought-provoking settings. Business historians are thus at least in principle in a position to examine and interrogate them, and to do so using contemporaneous documentation free from the biases that afflict the retrospective, predominantly interview-based, conventional case studies.⁸ Most members of the business history community have substantial institutional incentives to develop and explore such materials. We believe that the right sort of business history, paying detailed and carefully circumspect attention to processes which inevitably play out over time, can provide vivid, compelling, and memorable case studies exposing and analyzing phenomena in this domain. These can be useful to a variety of parties in a variety of ways.

We are hardly alone in holding this belief: a small but increasing number of researchers in the United States and Europe demonstrably feel the same way. The approach we will take in the remainder of this volume differs from the most prominent recent others, however. The objectives behind the 2006 founding of the journal *Management and Organizational History* can be reviewed in that journal's inaugural issue.⁹ The calls the founding editors cite for what they propose to publish come from outside of the mainstream of management academia.¹⁰ Another important point of reference is Marcelo Bucheli and R. Daniel Wadhvani's *Organizations in Time: History, Theory, Methods* (Oxford: Oxford University Press, 2014). The agenda of that volume involves defending the integrity of historical methods and their value for academic management and organization studies, positions we would certainly endorse. The objective of its most practically oriented elements, however, appears to be to explain to business historians how to present their work in a style more familiar to an academic management audience (potential colleagues, personnel review committees, etc.). We have in mind a different project: addressing live discourses in management academia's classrooms and journals with (and trusting) unfamiliar but cogent evidence developed and

Section, p. 4.) Some academic studies have come to this conclusion, some not. Roger Dickinson, "Business Failure Rate," *American Journal of Small Business* 6(2) (Fall, 1981): pp. 17–25 *inter alia* reviews an extensive earlier literature and makes the point that calculated statistics vary over time and samples. Yang and Aldrich ("Out of Sight") conclude that the studies they reviewed underestimated failure rates due to left censoring of the population being sampled.

⁸ On those biases, see Gartner and Shaver, "Nascent Entrepreneurship," p. 660. Studying specific cases of course inevitably raises questions of generalizability. Historians take great care to identify idiosyncrasies of the particular case—in effect, stratifying the example—to address this concern. We expand on this point in the Conclusion of this volume.

⁹ See Charles Booth and Michael Rowlinson, "Management and Organizational History: Prospects," *Management and Organizational History* 1(1) (January, 2006): pp. 5–30.

¹⁰ See also Peter Clark and Michael Rowlinson, "The Treatment of History in Organisation Studies: Towards an 'Historic Turn'?" *Business History* 46(3) (2004): pp. 331–52.

presented in a style deriving mainly from the evidence itself rather than the conventions of the audience.

The discourse these sources represent makes frequent reference to a historical turn in organization studies. The earlier so-called linguistic turn in philosophy and the cultural turn in academic historical writing were not ambiguous phenomena: they were visible in supply (and, increasingly, demand) in the labor market for junior faculty and eventually in the weight of practice in the working profession.¹¹ As the composition of employed academics in those fields changed, so did the published literature, so that there is ex post no shortage of actual philosophy and academic historical writing in these once new styles: the turn in question was a shift in the direction of actual philosophical and historical writing, not a tide—at flood levels or otherwise—of calls for change. We are of course deeply sympathetic to the increasingly extensive calls from within business history and some quarters of organization studies for more extensive and profound engagement with the historicity of many phenomena of interest in management academia.¹² But the calls for, and claims of, a historic turn in that domain do not appear to us as of this writing to be a phenomenon similar to the turns mentioned above. The tables of contents of mainstream journals and mainstream training seem, a tiny handful of jobbed-out special issues aside, quite unaffected.¹³ One might say that there is a lot of hortatory theory but not much practice yet; and such practice as there is is coming chiefly from the fringes of academic management studies, not the core. A significant change in the mainstream discourse may be coming; and we agree that it would be very desirable. But we see no evidence that it has happened yet.

¹¹ The appearance of post-modernism in a number of fields followed a similar course.

¹² Some writers see the origins in a series of articles by Meyer Zald in the early 1990s coming to a climax for these purposes with his "Organization Studies as a Scientific and Humanistic Enterprise: Toward a Reconceptualization of the Foundations of the Field," *Organization Science* 4(4) (November, 1993): pp. 513–28 (*Organization Science* being an entirely mainline organization theory—and thus academic management—journal) and a paper in that same journal a year later, in the section set aside for invited thought-provoking essays on the state of the field by Alfred Kiesler, "Why Organization Theory Needs Historical Analysis—and How This Should Be Performed," *Organization Science*, 5(4) (November, 1994): pp. 608–20. The number of authors expressing similar views and the specific domains of academic management research to which they refer certainly seem to have grown considerably since then, particularly since the millennium. More extensive exposition of the papers' content and a much more extensive set of references than would be appropriate here can be found in the introduction by Bucheli and Wadhvani and the chapter by Behlül Üsdiken and Matthias Kipping in *Organizations in Time*. (The latter of these in particular gives a thorough review of organization theory developments that led to a longitudinal perspective in certain lines of organizational research. But neither it nor any of the other chapters in the volume, we feel, grapple successfully with the question how to effect change in valorization or practice in mainstream business academia.)

¹³ It would be interesting to follow the extent to which articles in those special issues are or become cited in the literature or methods sections of mainstream articles.

This raises the question of whether there might be other, complementary ways to start—ways not to knock on the door so much as to induce others to ease it increasingly open. That is, to put the matter slightly differently, the project of this volume and the historical essays it presents: the main business of this volume is to present business histories of the emergence of order in new or changing organizations and groups of organizations. This introduction sets up that effort in two steps. First, it explains in a more elaborated way why the sparse extant academic literature might be thought a missed opportunity. Then it develops the concepts—most importantly, that of a routine—and the orientations that both suggest where researchers might look for particular subjects and provide focus and intellectual context for the individual studies that follow. Since these studies are written as history rather than in the perhaps more familiar form of social science literature, the volume concludes with a chapter concerning learning from history—why there is currently so little of it being produced by business school academics, what would constitute sound history (by which we mean good analysis of surviving historical evidence) were they or anyone else seeking to address their audiences to want to write it, how social scientists, with their mainly statistical notions of proof, should think about the value of case studies, and what good historical analysis can accomplish (again, for the relevant populations).

We begin with the silences. The ideal of the entrepreneur and the start-up, attempting to “commercialize” or “monetize” a product or idea (and becoming wealthy through selling the venture) has gripped the imagination of a far larger population than just enrolled business students. But in its emphasis on conception and exit, it is in an odd way incomplete. Sometimes there are actually sales of ventures that are really mere ideas. (More commonly still, consider patent licensing.) But most exits involve in effect selling a company. Potential investors generally want to see some evidence of commercial viability of the idea—that a genuine operating company with some definite prospect of profit earning is possible. This suggests that a crucial aspect of entrepreneurship is starting an actual enterprise.

To appreciate the challenges of this, two key aspects of actual companies must be understood. One is that certain sorts of activities are crucial—even nascent organizations must in one way or another carry them out. Some of these activities are tied fairly closely to the material world. These include design, production (which involves both organizing the supply of elements or components the company itself will not create and directing productive activity concerning the parts created in-house), and distribution. Other activities are not strictly speaking material but are closely tied up with those that are. For example, some material activities can be paid for afterwards, a week or a month in terms of waged or salaried employees, perhaps as long as sixty or ninety days for materials supplies. But actual money rather than credit is

required at some point; and companies that run out of money generally find themselves unable to continue operations. Second, at a more abstract level, decisions need to be made—in the straightened circumstances of most early-stage firms, more or less continuously—about resource allocation, that is, about what scarce cash (and perhaps human capital) should be used to do. And however much members of the organization may feel that they understand what they are about and what the organization is trying to do, from time to time, organizational actors either make strategic decisions or find strategic decisions forced upon them.

The other key aspect is that out of the usual frantic borderline chaos of the earliest days of enterprise, order typically does emerge (typically, it only emerges, rather than being present at the start). There are two sorts of reasons for this. One is the existence of exploitable economies of scale—in the technological sense originally envisaged by economists—in the sort of activities described above. Alfred Marshall framed this in terms of quantities: twice as many input resources, for example, yielding more than twice as many outputs.¹⁴ It has long been recognized that any such relationship has a so-called dual one stated in terms of cost: in this case, the dual to higher outputs with the increasing scale of inputs corresponds to lower unit costs at fixed input prices in the context of increasing overall scale. There are examples of this sort of thing in which the economies come as a matter of engineering physics rather than through managerial intervention. But most examples do involve managers. These generally derive from conscious decisions and plans to use fixed resources more intensively. They survive because the resulting superior cost position generally enables firms that possess it to outcompete firms that do not.

The second reason for emergence is the existence of a more abstract, or at least more strictly organizational, variant of an economy of scale. This is the notion that with experience, it becomes clear how to do things better—faster, smarter, etc. There are tacit versions of these organizational economies, explicit ones, and even ones so explicit that they can in effect be formalized in a division of labor. The improved techniques may even become embodied in the functions of individual tools or machines. One might call its individual-level counterpart “skill.” The organizational counterpart might also be called skill or, know-how: it is the ability of an organization, coordinating internally through formal protocols or tacitly, to do whatever is called for. The business objective in having these is generally the same at both levels: as the central objective of most businesses is to make money, the vehicle for making money in business is providing goods or services potential customers want to buy,

¹⁴ Alfred Marshall, *Principles of Economics: An Introductory Volume* (New York: Macmillan, 1920), pp. 318–19. The notion can also be framed in terms of unit costs.

and there is, generally, more money to be made at larger scales than at smaller ones. Skill or know-how usually facilitates providing what is wanted economically.

Since organizational action is at least in part the action of individual people, it is worth probing a little more deeply here. Some ideas about human nature and conduct, originating with the psychologist and philosopher John Dewey nearly a century ago but recently again in the forefront of discussions, are helpful.¹⁵ Dewey saw three background determinants of how people behave. Revised slightly, they are habit, impulse, and deliberation. Dewey thought of habit as what was most distinctive about humans. However, he distinguished between dead habits, i.e. rote repetitions, and those infused with thought and feeling. Dewey, readers may recall, was a pragmatist. To him, empirical individual habits—those in which Dewey himself was interested—are not inevitably unchanging patterns of behavior. They are at most dispositions and are perhaps better thought of as repertoires rather than actions. They are capabilities that can be triggered and mobilized into action. What triggers when they go on and off is an obviously (and active) subject for psychological research.

The organizational counterpart of a habit in this sense is a routine. It is something the organization knows how to do and *ipso facto* is in principle repeatable. Its identity, however, lies not in the *fact* of its repetition, a behavioral matter, but in the *possibility* of its repetition. There is, as Geoffrey Hodgson has acutely observed, a great deal of blurring of these two senses in the scholarly literature.¹⁶ That blurring represents a category mistake, and one of a sort well known to philosophers since Aristotle.¹⁷ One can say in retrospect that the exposition concerning routines in Richard Nelson and Sidney Winter's vastly influential 1982 monograph clearly has the capability notion in mind; but it was easy to read the text and see the behavioral notion, and perhaps easy to see the behavioral notion and little else.¹⁸

¹⁵ John Dewey, *Human Nature and Conduct: An Introduction to Social Psychology* (New York: Henry Holt and Co., 1922). The re-emergence began with Michael D. Cohen, "Reading Dewey: Reflections on the Study of Routine," *Organization Studies* 28(5) (May, 2007): pp. 773–86.

¹⁶ Geoffrey M. Hodgson "The Concept of a Routine," in Markus Becker, ed., *Handbook of Organizational Routines* (Cheltenham: Edward Elgar, 2008), pp. 15–28.

¹⁷ Aristotle, *Metaphysics*, book IX, chapter 3. (On category mistakes more generally, see Gilbert Ryle, *The Concept of Mind* (London: Hutchinson, 1949), chapter 1.)

¹⁸ Richard R. Nelson and Sidney G. Winter, Jr., *An Evolutionary Theory of Economic Change* (Cambridge, MA: Harvard University Press, 1982). Many subsequent authors have done the latter of these (some without much comment and others much more critically). The leading references are Brian T. Pentland and Henry H. Rueter, "Organizational Routines as Grammars of Action," *Administrative Science Quarterly* 39(3) (September, 1994): pp. 484–510, Pentland, "Grammatical Models of Organizational Processes," *Organization Science* 6(5) (September–October, 1995): pp. 541–56, Martha S. Feldman, "Organizational Routines as a Source of Continuous Change," *Organization Science* 11(6) (November–December, 2000): pp. 611–29, and Feldman and Pentland, "Reconceptualizing Organizational Routines as a Source of Flexibility and Change," *Administrative Science Quarterly* 48(1) (January–February, 2003): pp. 94–118. For the most recent thinking and extensive references, see Feldman, "Routines as Process: Past, Present, and Future," in

Routines are important to firms for a variety of reasons. The heart of the positive ones lies in the fact that coordination is generally, for the reasons given above, a central aspect of why firms exist in the first place. It might in principle be the case that rules and procedures for coordination, and the circumstances in which they should be mobilized, are so transparent that they can be codified and transmitted costlessly. But this rarely seems to be the case. The more common situation is that there are powerful complementarities between unarticulated, and possibly inarticulable, aspects of the behavior of potential actors. It can be possible to write a helpful set of protocols while still being impractical to write a comprehensive one. The actors (or interacting units) themselves, on the other hand, learn to work with one another over time. They get good at it.

Two aspects of this are worth developing. The first is that it is characteristically true of firms going from prototype scale to commercial production scale that they are to a significant extent exploring unfamiliar internal terrain. There is thus more to evaluating potential problems of scaling than just asking whether the individual elements of the business plan and the company's formal infrastructure each can scale. There may be interconnections. Some informal resources—individuals and collectives—may not be scalable, particularly coordination-related resources. Opportunities (and potential problems) may appear at larger scale that are not visible, if they exist at all, at lower scales. Indeed, trade-offs may emerge between production and process-improvement experiments. Recipe book thinking about scaling can be just as pernicious as recipe book thinking about anything else: actual situations can be importantly diverse. The second is that routines, once emerged, can have bad consequences. As Dewey observed, habits can be dead as well as alive.¹⁹ The advantages of everyone knowing what to do in reaction to some stimulus or situation can be outweighed by that collective response not being the best one or even the right one, and those weights can change over time.

Routines in this sense may not be genes exactly, as some remarks in the Nelson-Winter book suggested.²⁰ But that was an analogy and analogies are never exact. Routines in this sense are gene-like, in that they condition expressed attributes and are heritable: as time passes, old people leave the

J. Howard-Grenville, C. Rerup, A. Langley, and H. Tsoukas, eds, *Organizational Routines: How They Are Created, Maintained, and Changed, Perspectives on Process Organization Studies*, Vol. 6 (Oxford: Oxford University Press, forthcoming), chapter 2. This literature is fairly rigorously focused on observable actions and patterns in them. The subject matter of the studies is without question interesting. As will be clear from our text, we differ from Feldman when she writes there that “the definition of routine used in [the Feldman and Pentland] 2003 paper was fully consistent with all previous work on routines”.

¹⁹ Dewey, *Human Nature*, p. 51. See also the discussion at p. 32.

²⁰ For the main exposition “routines as genes”, see Nelson and Winter, *Evolutionary Theory*, pp. 134–6.

firm and new people join, but the firm's routines and capabilities continue. But there is more. In studying firms, routines have another interesting aspect: they can be hard, especially for non-participants, to replicate. This means that the possession of an effective routine for some purpose can generate an asymmetry between otherwise similarly situated firms. Competitively valuable asymmetries can frustrate the ability of head-to-head competition to drive down prices.²¹ Their origins and maintenance are therefore subjects of great interest in Strategy as well as in Entrepreneurship and Organizations discourse; and the emergence of routines is thus a historical subject with some bite.

In a setting in which strictly repetitive activity dominates operations, it is reasonable to wonder how individuals within the organization can exercise agency beyond coming in for work every day. At a high level of abstraction, there are basically two answers to this (and not merely for executives but also for employees relatively close to operations). The first is that agency is inherent in any action, even a relatively routinized one, in which skill or judgment might be required. The second is that even the most routine-bound organization occasionally runs into situations in which the standard, or at least established, set of activities (and even formal objectives if there are any) and ways of doing things come into question. This may be due to appealing new prospects (investment opportunities, potential clients with idiosyncratic but not utterly unimaginable requests) or, alternatively, to a looming catastrophe. But in such moments, it will generally be true that at least some members of the organization will consider—perhaps individually, perhaps collectively—what the circumstances are, what should be done, and how to set about doing it (or at least doing something). One might think of these as moments of strategic intervention, or at least of strategic urgency. The point here is that they do happen.

Everything said here about firms is also true, *mutatis mutandis*, of groups of firms such as industries. The simplest example of this sort of thing is a tight oligopoly in which the firm-level actors come to realize that the profit-maximizing competitive strategy is not competing head to head for a single set of customers, but rather finding a way to stay out of one another's way, sacrificing some economies of scale for minimized competitive pressure. John Hicks suggested nearly a century ago that monopolists prefer a quiet life; and while this remark can be quite misleading, there is a grain of truth to it.²²

²¹ Sometimes competitively valuable asymmetries derive from property rights (patents, for instance, or legal ownership of a scarce resource) or from an entrenched market position (brand equity, for example, or even legally blocked entry). But sometimes they derive from what the firm can do more in this mode of routines.

²² J.R. Hicks, "Annual Survey of Economic Theory: The Theory of Monopoly," *Econometrica* 3(1) (January, 1935): p. 8.

Hence, there are many aspects of the behavior of firms in groups which fall under the account of routines given above.

An assumption behind much of the above should surface at this point, namely that both organizations and actors within them are best approached as in principle genuine agents whose degree of actual agency in any particular period and circumstance is a matter to be assessed and not something to be casually assumed away. Dewey's perspective would be unintelligible without this assumption. But much theoretical and empirical analysis of firms and industries—consider all economic analysis proceeding from assuming free-entry market equilibrium—in effect assumes the opposite. The possibility of agency only has some force if evidence at the actor level is available. The chapters in the volume offer dramatic evidence that this situation sometimes exists. Considered as a methodological assumption, it should perhaps be contrasted to the idea that structures are (i.e. determine) everything. The former is as an assumption not intrinsically hostile to the notion that structures generally exercise an influence. It simply does not rule out other possibilities.

Considering routines illustrates why attending to structure matters. A moment's reflection will confirm that routines (of all descriptions, including routines concerning change) can induce path dependency in organizational histories. Efficiency and profit seeking may be motives in the moment (and conceivably even outcomes over the course of evolutionary competition in the market); but there are usually explicit costs, and sometimes also tacit sources of resistance, to change. As Marx wrote colorfully in brilliant and not excessively theorized remarks in his *Eighteenth Brumaire of Louis Napoleon*: "Men make their own history, but they do not make it as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly found, given and transmitted from the past."²³ Neither firms nor markets, still less entire economies, are ever continuously in a state of equilibrium.

So far, perhaps, so good; but where do routines actually come from? The mystery, after all, lies in the fact of emergence.²⁴ One might think as a general matter that routines have their origins in inflexible aspects of technology, in managerial design (i.e. in intentional action, by individuals or collectives, in original initiatives, or in copying), in institutional rules or norms, and in

²³ Karl Marx, "The Eighteenth Brumaire of Louis Bonaparte," in Robert C. Tucker, ed., *The Marx-Engels Reader*, 2nd edition (New York: Norton, 1978), p. 595. (Marx continues, increasingly colorfully but to the same point, "The tradition of all dead generations weighs like a nightmare on the brain of the living.")

²⁴ On the origins of organizations and markets, see John Padgett and Walter Powell, eds, *The Emergence of Organizations and Markets* (Princeton, NJ: Princeton University Press, 2012), especially their introduction and the chapters in the initial section.

experience (again, perhaps individual and perhaps in one way or another collective). But the idea of probing more deeply into such generalities seems in principle promising. Perhaps the above list—ad hoc as it is—is not exhaustive. Perhaps more detailed accounts will be illuminating of processes and mechanisms. Perhaps they will be pragmatically useful as well, suggesting both ways of proceeding in related circumstances and also ways of proceeding worth avoiding. These thoughts raise questions about how one might learn from an intrinsically evanescent phenomenon and where exactly one might look for evidence. As suggested above, we look for situations in which establishing order—getting things organized—is a matter of great concern. We turn to the particulars of this next.

The following studies are divided into three groups. The first examines emergence of routines in the context of *initiatives* at all levels of aggregation, from de novo enterprises through the development of new capabilities and internal institutions within ongoing businesses, to intra-industry adaptation to new regulatory standards. The second concerns the emergence of routines in ongoing *operations*. This section begins with a study inside a company whose history is famous but, we learn, underexplored. But the section focuses mainly on broad interfirm settings, with one on the development of the interfirm division of labor, another on the dynamics of regulation and practice, and one of contracting relationships per se. The final section homes in on routines in periods of *transition* of one sort or another: in one, of the fundamental technological possibilities of the industry; in the second, of the organization's desire to measure and evaluate what it is doing; and in the third, the development of routines for innovation in problem-oriented knowledge communities.

It may be helpful to give a little more detail in terms of the presenting problems. Chapter 1 in the “Initiatives” section, by Daniel Raff (Wharton School Department of Management and NBER), concerns the earliest years of the Book-of-the-Month Club. The company's basic idea was to bypass established distribution channels and instead reach out directly to potential customers—many living in places too thinly populated to support bookstores and related retail establishments. The founders were experienced and indeed subtle hands at writing direct marketing and advertising copy, though they had only slight experience with managing retail trade or with the enterprise's product category. They had modest initial expectations and in the very beginning their “organization” amounted to themselves, a secretary, and no routines of any sort for carrying out repetitive but essential tasks of day-to-day operations or for responding to economic opportunities or environmental change. From very early on their advertising generated so massive a response that such essential activities as filling orders and even cashing the customers' checks became potentially crippling challenges. Worse, the fraction of

customers who returned books was sometimes large enough that the incoming parcels themselves paralyzed the company's modest midtown Manhattan, two-room walk-up offices. Chapter 1 discusses the challenges that emerged in the company's ramp-up and early growth phases and the various measures the company undertook as time passed and its business grew (like Topsy) to create order and enhance profitability. It relates the measures to established industry norms and practices; and it calculates just how valuable the innovations were in the first twenty years or so of the company's existence.

Chapter 2 in this section considers a more complex case. Margaret Graham (McGill University Faculty of Management and Winthrop Group) explores the historic shift from entrepreneurial inventor to innovating research laboratory at Alcoa, the American aluminum company that dominated its industry for much of the twentieth century. The chapter uses the generation, maintenance, and renewal of routines at various levels as a lens through which to observe this change. If research-generated innovation became the central routine of capitalism in the early twentieth century, how did industrial research translate into routinization of innovation, which in an entrepreneurial context is the ultimate non-routine process? Alcoa is an example worth exploring. As metallurgical knowledge of light metals in general and aluminum in particular advanced, operations in European firms quite rapidly became dominated by the growth of scientific knowledge and the influence of centralized control. Alcoa, in contrast, began and continued throughout the period with relatively autonomous operating units and a craft-style suspicion, on the part of unit managers, of centralized control over process. Various aspects of the firm's product market profile, especially prior to the 1920s, reinforced these tendencies. The chief protagonist in this account is not Alcoa's technical organization as such, but a subset of the organization known as the Committee System. Chapter 2 covers why the Committee System arose, how it evolved, how it functioned, and how it eventually became a bureaucratic, routinized mechanism for incremental innovation, but only after two decades of legitimating, generating, and guiding strategic, grassroots research and development.

In Chapter 3, Martin Collins (Smithsonian Institution, National Air and Space Museum) reconstructs the genesis and implementation strategies involved in Motorola's Iridium project, a venture into global satellite communications. In this venture, Motorola had a context, an idea, and a problem. The Cold War was winding down, and with it military procurement budgets. The company had developed skills and capabilities but needed new products. In the post-Cold War world, however, transnational projects were becoming much more feasible (indeed, the market for them was becoming much more competitive). Motorola's idea was a global personal communications network using satellite rather than ground-based transmission. This was no strictly

technological novelty. But it was fearsomely complex technically and organizationally as well. Executing on the idea—developing working components, a smoothly functioning system, and the business infrastructure required for it to operate successfully as a private enterprise in a huge number of jurisdictions, plus navigating the regulatory environments in all those jurisdictions—required not just coordination among large, physically, and culturally widely dispersed groups and individuals within the company and its contractor community but also—particularly given performance requirements—an unprecedented degree of commitment to process attributes in development and manufacturing. Chapter 3 is from one perspective an account of the creation of a culture and a set of institutions, over and above specific narrow-domain routines, such that this would be feasible, placed clearly in that story’s larger (and formative) times. But the routines were not just about ways of getting organizational purposes accomplished. They embodied modes of thought that sought to connect, at different scales, the complex realities of a globalized world.

The final chapter in Part I, Chapter 4, concerns standards, which are usually understood as static, conservative, and limiting. Lee Vinsel and Andrew Russell (both at Stevens Institute of Technology) present two case studies, each engaging with the notion of organizational routines in a changing environment. The Bell System in the 1920s was a monopoly and may have looked monolithic from the outside. But it was internally at best a federation of many different local companies, interlinked in their potential service provision, ultimately reliant on the same basic patents, but operating with a wide variety of equipment, formats, and practices. Coordinating them technologically, in a period of ongoing scientific and engineering advance, and achieving this at least relatively inexpensively required eliciting information and organizing consensus. By contrast, the American automobile industry, in the time of the environmental movement and the first major regulation of auto engine emissions, was certainly not a monopoly. Its component companies were entirely distinct operationally and powerful political forces. They, rather than the regulators, were the primary source of research and development. When asked, they would say what they could do and what they could not. The federal government wanted much less pollution from production auto engines than the industry said it could provide and wanted progress faster than the industry said was feasible. The regulators elicited much more progress, and much faster, through the capture and deployment of information, the setting of technically feasible performance requirements, and a general stance of leaving the engineering implementation to the companies. The authors see no evidence that the costs were excessive.

The “Operations” section begins with a study of the Ford Motor Company by Damon Yarnell (Dickinson College) in Chapter 5. The company’s first two

“mother” plants in the time of the Model T must be among the most scrutinized operations in the whole of industrial history. But the attention has focused on manufacturing methods and labor relations; and an essential set of activities has gone largely unremarked, and essentially unstudied. The division of labor in manufacturing was indeed a marvel of organization; but without a steady flow of the required materials to the shop floor, the conventional clockwork metaphors would have been an irrelevance and nothing would have worked in any way other than fitfully. Without devising purchasing routines, the explosive growth and dizzying totals of Ford production in the teens and twenties would have been simply impossible. Chapter 5 examines how procurement and related coordination practices worked and evolved, particularly in the context of rapidly expanding demand for a complex manufactured product. It also exposes some of the struggles (between production and administration, culturally as different as they were, and within the production activities themselves) and the maneuvering behind that evolution, particularly in the face of the coordination demands of ever larger scale production under conditions of inconstant demand. Networked coordination and internal flexibility were integral features of Fordism.

The challenges of organization for operations that are temporary rather than ongoing anchor Part II's next two chapters. Enterprises oriented around projects are vastly more common in the economy and business history than is popularly understood; but elements of repetitive practice and skill are crucial to such enterprises' profitability and longer-term survival. A great deal of coordination—perhaps a surprising amount—is required in complex productive activities, in complex projects most of all. Some of this coordination must, as a practical matter, go on across firm boundaries. This transacting does not work as smoothly or effectively as some (e.g. economists) might imagine. It is also helpful to recognize that in a project context, the cross-firm activities are often not so much transactions as they are jointly managed experiments. There are various ways all this coordination could be organized, with varying performance costs. Chapter 6 by John Brown (University of Virginia Department of History and School of Engineering) explores the high-risk course of learning traced by American railway bridge builders in the post-Civil War decades, each project presenting different obstacles and hazards. After 1865, a new industry, uniquely American, grew to prominence, making standard and semi-custom iron road and railway bridges. Circulating illustrated catalogues, specialized firms like Keystone Bridge, Phoenix Bridge, and American Bridge Company created national markets for their pin-connected bridges. With nested routines and procedures ordering the processes of design and production, they transformed bridge building from a local and empirical art into a rationalized industry. After 1870, an innovative entrepreneur, James Eads, upset established procedures at these firms. Promoting a new arched

design and a new material—steel—Eads in effect insisted on new routines in the industry. Concurrently, civil engineers and editors of technical journals advocated new approaches in design and construction to counter the contemporary problem of bridge collapses. These novel routines became instruments to force institutional and technological change among the railroads, the iron and steel mills, consulting engineers, and bridge makers that together built these essential structures.

Dams are in some respects even more idiosyncratic than bridges. They may not be less important. In semi-arid environments such as California, they play a vital role in the regional economy. By capturing flood flow and facilitating its measured release, large reservoirs can support the most considered and maximally productive use of limited water resources. But dams generally store massive amounts of water; and the failure of a large dam can wreak tremendous havoc on property and kill people by the hundreds. Chapter 7 by Donald Jackson (Lafayette College) shows that as engineers proposed innovative dam designs in the early twentieth century there also came state regulatory regimes intended to protect citizens from unsafe structures. Jackson considers how two types of dams—concrete gravity and multiple arch—were treated by state regulatory authorities and how rules and routines involved in creating and evaluating designs came about. The role of mass psychology is given special attention in this analysis, providing a revealing counterpoint to the widespread belief that progressive era engineering was founded solely on the application of mathematical formulas.

If research is necessary to operationalize innovations, in a setting with long-term relationships between consumers of the research and producers and the rhetoric of “partnership,” it is easy to imagine struggles over who is to absorb the expense. In the final chapter of Part II, Chapter 8, Glen Asner (Historical Office, Office of the Secretary of Defense) gives an account of the maneuvering between the US Department of Defense and allied military contractors over procurement regulation and the details of the contracting process in the creation, development, and production of Cold War weapons systems. His subject is ultimately how rules and less formalized routines evolve over time. His actors have some interests in common but others in sharp conflict. The contracting situations consistently differ in small details. A set of boilerplate rules, to be applied with complete uniformity, would be, both sides agreed, wasteful and significantly ineffective. So some room for interpretation in the rules was required. Chapter 8 exposes the history of the creation of organizational routines, their incorporation into formal government regulations, and the manner in which these came to be modified or displaced by competing regulations or routines throughout the early 1970s. Routines—in the Nelson and Winter sense—really are truce lines here. And not infrequently one or the other of the parties makes a self-interested exploratory foray and the truce

breaks down. The background interplay between the formal and the informal and the whole structure of positioning and negotiation over the terms of truce are at the heart of Chapter 8.

In organizational histories, existing practices and routines are at times challenged by alternative approaches claiming superiority in one respect or another. The collection's final three research essays represent alternative paths to transitions in practice and routine. The final "Transitions" section begins with a study by Josh Lauer (University of New Hampshire Department of Communication), Chapter 9, analyzing a vivid contest between the customary and the contentious in consumer credit evaluation. For much of America's twentieth century, meeting face to face with credit managers was a necessary, often painful experience for those seeking loans or opening store accounts. Experienced "credit men" used a repertory of probing questions to determine the likelihood that an applicant would be reliable and prudent, rather than a "slow payer," much less a "deadbeat." Yet with the advent of both social scientific surveys and computerized databases, ambitious promoters argued that their quantitative techniques based on coarser evidence could wring the subjectivity out of credit routines, replacing them with objective, predictive measures of consumer behavior which could produce results faster, more reliably, and less expensively. The struggle between character and calculation lasted for decades. By eliminating the need for interviews, credit scoring threatened established routines and the legitimacy not just of the position of experienced individuals but of a conception of the business. The superiority of quantitative procedures was far from readily accepted and its progress was fiercely contested.

In Chapter 10, Michele Alacevich (Loyola University Maryland) reaches inside the World Bank's global postwar development projects to relate the unanticipated consequences of the institution's repeated efforts to devise procedures to assess its initiatives' effectiveness. Alacevich chronicles the development of project evaluation routines at the World Bank. Those who know the bank now will find it scarcely possible to imagine the work of the institution without systematic routines for project appraisal; but that was nonetheless the status quo as recently as the mid-1960s, twenty years into the life of the institution and nearly 370 projects into its work. Management had come to feel that the organization needed assessment, knowledge, and feedback to guide further funding and planning. There were debates about objectives, intellectual foundations, and organizational institutions for implementation. Persistent conflict emerged between those oriented towards the idiosyncrasies of experience and those focused on explicit criteria and measurable and comparable outcomes. It turned out that routines, in a far deeper sense of the phrase, were at issue. Progress was very far from linear. There were, in the end, two major initiatives, the first thought at the time and immediately

thereafter to have been a failure and the second, organized in its aftermath, a success. Whatever else can be said, the overall process was in a profound sense a political one; and it was an irony of the procedures that emerged that there was an intrinsically uncertain aspect to the knowledge they generated. In the fullness of time, the institution itself began to feel that the outcome of the second initiative had major problems and itself required a searching review. Central elements of the first emerged, once again, in the reconsideration.

The final historical study, Chapter 11 by Ann Johnson (Cornell University), concerns how the global automobile industry came to redesign the internal combustion engine in the twenty years between 1970 and 1990. The prompt was environmentalism but the challenge was not simple. The basic design was long since well established. A transition from the associated routine practices was forced on producing organizations by external events and authorities—here the US government’s mandating of auto emission controls from the late 1960s onwards. The new problem was to manage the central processes of combustion and exhaust so as to optimize the output of a variety of pollutant by-products whose production had not theretofore been thought to matter. Chapter 11 documents how the complexity of managing emissions forced auto engineers into a non-traditional set of consultations, conferences, and experiments that ignored company boundaries and corporate restrictions. Each firm had its own designs; but—worse—everything seemed to be related to everything else in terms of the underlying chemistry, physics, and engineering. As knowledge of atmospheric chemistry improved over time, worse still, the regulatory targets evolved as well. Chapter 11 follows the development of two technical subsystems. The most important elements of progress came out of interaction between individuals and groups across individual firm (and disciplinary and national) boundaries. It proves helpful in understanding the process to focus not on the technology per se but on projects and the communities in which the actors involved with them were embedded, communities which had both social and epistemological structures. Order in each aspect emerged; and the way it emerged had some influence on the content of what the community knew at the end. In engineering fully as much as in science, the production of things is not entirely distinct from the production of ideas.

These historical chapters may well strike some readers, social science-educated management academics in particular, as colorful, even thought provoking, and potentially useful as a springboard for classroom discussions leading to other evidence and literatures, but unconvincing when considered as evidence or argument, at base an invitation to conduct statistical inference from samples of $n=1$. But this would be to misunderstand both the methods of these chapters’ construction—that is, how historians work—and the ways historical research can give value to its readers. This is not to say that there is

nothing to the view; but it is to say that the view is misguided and inappropriately sweeping. An elaborated response is certainly in order. This volume therefore closes with a concluding chapter entitled “Learning from history” which considers the likely origins of such an impression and addresses the issues of how careful historians do their work and how that work can help improve decision making.