Conclusion: Learning from History

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The introduction to this volume argued that the emergence of routines represents a central and awkward lacuna in the literatures of a number of academic management fields. This emergence, in a wide variety of contexts, is the central theme of this volume's historical essays. Because key aspects of the domains considered there and core reasons why routines are important in them have persisted over time and will continue to do so, understanding the problems and challenges of emergence in history can be germane to, and even helpful for, systematic consideration of future courses of action in similar and related organizational settings.

On such abstracted and general terms, we are content to let the chapters speak for themselves. But this volume's expected readership represents a very concrete disciplinary setting, making some specific issues worth exploring further. The chapters demonstrate that vivid, palpably thought-provoking evidence on the importance of emergence as a phenomenon exists. This is the case despite its not being, as is most evident in at least the professorial part of that readership, statistical in character. These two facts raise deep questions about how students of organizational life and the life of organizations learn and develop research questions and about how they might go about their work more fruitfully, even if less straightforwardly "scientifically," both as teachers and as scholars. They also raise some middle-range questions. If the preceding historical studies proved worth reading, intellectually provocative, or even just useful instruments with which to start classroom discussions, three middle-range questions in particular are both obvious and pressing. Particularly given the size of the potential classroom audience, why isn't there more such literature (and what general orientation might be offered to researchers willing to give it a try)? Given that the research materials and the form of the writing and analysis differ so much from what conventional business school academics and other social science-oriented researchers encounter, how are such readers themselves, or any others, to understand what constitutes good history? There is also the broader question: what is good history good for? The deep questions are far too complex and subtle to be addressed in the space available here. But the middle-range ones are important enough in themselves. This concluding chapter takes them up in turn.

We begin with some background and contextual matters. University-affiliated business schools of recognizably modern form began with the establishment of the Wharton School in 1881 and multiplied in the early decades of the twentieth century. But until the reports of the commissions established by the Ford and Carnegie Foundations in the late 1950s and the entry of new programs since then, the teaching was oriented directly towards practical experience and generally carried out by individuals who had directly "practiced" business.[1] The commission reports strongly urged a greater focus on applicable mathematics and the increasing fruits of the then booming social sciences—disciplinary knowledge, as it is sometimes called.[2] This required different staffing as well as significantly different courses; and the new types of staff members were increasingly judged, in recruitment, promotion, and tenure decisions and in matters of salary and research support determination, by discipline-based criteria.[3] It is not clear how much potential employers valued the students' mastering this material. Increasingly, it seems, over the ensuing decades business school attendance and performance became more important as a screening device in the managerial labor market rather than as an educational experience in itself.

The employers in question were for many years predominantly large corporations in the manufacturing and distribution sectors, financial services institutions, and consulting firms. But since the mid-1990s, this second tentative equilibrium has come under marked strain. MBA students now are much less oriented to finding work in large, established operating firms. They have become much more interested in private equity firms, hedge funds, and start-ups (increasingly commonly of their own devising). Opportunities for entrepreneurship in one form or another have displaced certainty and stability as the most desired attribute. This shift mirrors secular changes in the terms of employment in the larger economy; but it is particularly notable in a population whose members are schooled to be ambitious and are

[1] For the reports, see R.A. Gordon and J.E. Howell, *Higher Education for Business* (New York: Columbia University Press, 1959) and F.C. Pierson, *The Education of American Businessmen: A Study of University-College Programs in Business Administration* (New York: McGraw Hill, 1959), respectively.

[2] For the arc of development and an interpretation, see Rakesh Khurana, *From Higher Aims to Hired Hands: The Social Transformation of American Business Schools and the Unfulfilled Promise of Management as a Profession* (Princeton, NJ: Princeton University Press, 2007).

[3] See also, and perhaps best overall, Mie Augier and James G. March, *The Roots, Rituals, and Rhetorics of Change: North American Business Schools after the Second World War* (Stanford, CA: Stanford University Press, 2011).

concerned to make affirmative choices about careers.[4] Some may presume they will eventually run large enterprises. But, increasingly, they want to start these enterprises or remake them, not take them over, and, in due course, pass them on to some successor.

These students clearly want different courses than those sought in earlier generations. They seem reasonably happy to trust what business school faculty members have come to believe is the basic core of a curriculum; but having arrived with dreams (and sometimes even concrete plans) for future businesses, they want, in addition to instruction in the details of raising funds for new ventures (all the way down to classes on how to project budgets and what are in effect critique workshops for draft business plans), some notion of what sort of problems typically arise in early stage ventures and how to think about addressing them. They have about them very much the air of people who seek food for thought relevant to a career of taking initiatives and making decisions in a very dynamic context. The word "administration" (as in "business administration" or "the administrative point of view") would sound quaint to them.

Three parts of the conventional business school curriculum seem most cogent to these desires. These are entrepreneurship (strictly so called so as to include entrepreneurial finance, a subject not generally treated in finance departments for reasons related to the discussion above), strategy, and organizations. Entrepreneurship courses teach students about the rudiments of getting companies started, as operating entities as well as in terms of finance. Strategy courses are about how both to develop intra-firm resources and capabilities and to position companies' offerings so as to create the possibility of profitable operations in the short run and in the longer term. Organizations courses are about organizations as a field for action.

Although there is extensive and buoyant demand for teaching to address the newer concerns, these areas for the most part lack a well-established body of useful techniques, standard calculations, and the like.[5] Nor are the parts of the academic literature touching on them deeply theorized. Courses heavy on talks from alumni and other successful entrepreneurs and on the development and critiquing of student business plans are deeply prone to the Whig history fallacy, tracing backwards, with implications of inevitability, the lineages of

[4] On the secular pattern, see e.g. Henry S. Farber, "Short(er) Shrift: The Decline in Worker–Firm Attachment in the United States," in Katharine S. Newman, ed., *Laid Off, Laid Low: Political and Economic Consequences of Employment Insecurity* (New York: Columbia University Press, 2008): pp. 10–37, and Matissa N. Hollister, "Employment Stability in the US Labor Market: Rhetoric vs Reality," *Annual Review of Sociology* 37(1) (2011): pp. 305–24. (There are not yet published studies incorporating the period of the Great Recession.)

[5] The obvious points of comparison are accounting, finance, and marketing, all now intensively, increasingly, or quantitatively oriented.

success.[6] More literature, and literature of a different kind, would be helpful. But the incentives are not there for business school faculty to produce such studies. Rapid production of countable and externally validated research outputs is the basic element of all the institutional career and resource-allocation decisions itemized above. The incentives this creates for junior academics, at a stage in their careers at which the particulars of their research programs, social networks, and general perspectives are still in a relatively formative state, to aggressively orient themselves towards the relevant external academic communities are clear, as are the subsequent tendencies to inertia at the level of individual activities and lines of inquiry. Thus the great bulk of academic management researchers undertake statistical analyses of databases assembled by others, the construction of interview—or, more commonly, survey—databases for analysis, and lab experiments (typically with undergraduates and MBA students or self-selected otherwise unoccupied individuals scanning Mechanical Turk rather than with experienced entrepreneurs or organization workers as experimental subjects). Work of this sort certainly aims for relevance and reliable insight; but achieving that is a more uncertain matter.

There may well be a trade-off between the war stories of the old regime and the abstracted social science of the current one; but other alternatives, with more attractive combinations of features, are possible. Against the high confidence but low granularity of detail characteristic of most academic management studies, one might hope for the high granularity of case studies with some answers to the characteristic trailing (and sometimes nagging) questions concerning the representativeness of their examples. And beyond the inevitable elements of idiosyncrasy in individual cases, there is a more systematic reason to address this. Most seriously longitudinal case studies depend upon research materials that have survived in business history either because the firms themselves have survived or because the firms were successful enough that they or their owners were in a position to preserve archival materials independently. One might reasonably have a general concern that firms that survived are not representative of the whole population of firms that started out and that inferences drawn from the particulars of their histories may not extend to the larger population. Like representativeness, this is a concern that can be addressed, not least through careful framing of research questions and contextualization—a certain modesty of claims—that is a counterpart to statistical controls; but without question it is a concern researchers must recognize and engage. In the absence of sufficiently richly detailed longitudinal datasets following a population of start-ups, one possible approach would be

[6] The phrase derives from Herbert Butterfield, *The Whig Interpretation of History* (London: G. Bell and Sons, 1931). See also Naomi R. Lamoreaux, Daniel M.G. Raff, and Peter Temin, "Against Whig History," *Enterprise and Society* 5(3) (September, 2004): pp. 376–87.

to inquire after problems that are difficult for firms that succeed in mastering them, as well as for those that do not, working, open-mindedly, from the problems towards the outcomes rather than the other way round, and using the struggles of the firms to begin to illuminate the contours of the problems. Whatever else they have in common, this is the basic course the chapters of this volume have pursued.[7]

We turn now from conditions of production to the qualities of the work itself. To understand what it is to write good history, it may be helpful to begin with the problem of inference in the social sciences. "Society" appears to have begun to be a subject for systematic investigation only in the early nineteenth century.[8] The earliest researchers sought universal laws as counterparts to those being successfully developed in the physical sciences. The empirical methods of the early social scientists seem very far away from the controlled experiments of physical science, however. Some of the work of the founders (e.g. of sociology) is entirely innocent of measurement and quantitative testing.[9] Some involves the relatively naïve taking of quantitative (or quantifiable) evidence from nature and treating it as an unambiguous observation of some basic facts. "Nature" might be the ordinary flux of events—in the daily life of a person, fully as much as in the course of the daily life of a firm or an economy—or it might have a more interventional quality, as when a pollster telephones numbers at random and asks a set of questions to whoever picks up the phone and is willing to talk. The classics of this long phase began with the sort of correlational interpretation one sees in Durkheim's *Suicide* and the sort of studies on which it drew and proceeds through literature increasingly—as mainframe and eventually desktop computers became common resources to working academics—involving much more elaborate multivariate regressions and related techniques.[10]

Today's scholars increasingly view this old status quo as unsound. The problems with the approach are felt to be three. As we noted above, the sample might not be representative of the population that is ultimately of interest.

[7] We vigorously endorse the idea that there is something to be learned from failure. But there is not nothing to be learned from success. A case study of success is *ipso facto* a study of, if nothing else, something working. Probing what was working and how it worked can be a valuable first step in understanding the full set of contingencies, possible good outcomes, and possible failure modes, in all of this clarifying how general the problems are and how particular are the solutions of the case at hand. (That said, opportunities to observe what happens when the gears fail to mesh can be pure gravy.)

[8] We have in mind the works of Saint-Simon and Comte. There are of course works we would now classify as economics, political theory, or social critique which might also be seen as predecessors.

[9] Consider e.g. Ferdinand Tönnies (translated by Margaret Hollis), *Community and Civil Society* (Cambridge: Cambridge University Press, 2001) and Georg Simmel, e.g. "The Metropolis and Modern Life," in Donald N. Levine, *Georg Simmel on Individuality and Social Forms* (Chicago: University of Chicago Press, 1971), pp. 324–39.

[10] Emile Durkheim, *Suicide: A Study in Sociology* (Glencoe, IL: Free Press, 1951).

Second, it might be very difficult to match up measurable attributes of sample respondents with possible causal factors in the relationships of interest. But the third potential problem is the most severe: it may be very difficult to tease out causal relationships in any unambiguous way in the first place. Even if the various possible influences are measureable, there may be quite a lot changing all at once. Teasing particular relationships out of the hubbub of general interaction may be very difficult.[11]

A series of developments in social science practice, some originating in economics but recently diffusing rapidly across disciplinary boundaries, have beat these problems back a bit. Those diffusing from economics began against the background of the classic instrumental variables techniques such as two-stage least squares estimation.[12] Progress began in the 1970s with an attempt to understand and measure causality in time series terms.[13] This eventually heightened interest in natural quasi-experiments, of which it turned out there were some, and ultimately in real-time experiments designed to isolate actual causal relationships and test them.[14] This progress appears, at least to present-day economists and observers, as a gradual freeing of empiricism from the soup of general equilibrium, in which everything might in principle affect everything else, to carefully constructed observation situations in which distinct causal possibilities can in fact be distinguished.[15] There may be costs in terms of scope and questions to the shift, but what the costs buy is clarity and confidence in inference.

To someone who takes these developments as an unambiguous and all-purpose good thing that solves all problems and who thinks that what historians do is order previously existing and available facts, the work of history writing seems inevitably and primarily a rhetorical exercise, an

[11] This is what economists and others refer to as the identification problem. On its earliest exposition, see James H. Stock and Francesco Trebbi, "Retrospectives: Who Invented Instrumental Variable Regression?" *Journal of Economic Perspectives,* 17(3) (Summer, 2003): pp. 177–94.

[12] Instrumental variables techniques were known to statisticians at least from the 1920s—see the discussion in Stock and Trebbi, "Retrospectives," of the famous Appendix B to Philip G. Wright, *The Tariff on Animal and Vegetable Oils* (New York: Macmillan, 1928). For two-stage least squares, see Henri Theil's two unpublished but widely cited memoranda of 1953, R.L. Basmann, "A Generalized Classical Method of Linear Estimation of Coefficients in a Structural Equation," *Econometrica* 25(1) (January, 1957): pp. 77–83, and J.D. Sargan, "Estimation of Economic Relationships Using Instrumental Variables," *Econometrica* 26(3) (July, 1958): pp. 393–415.

[13] C.W.J. Granger, "Investigating Causal Relationships by Econometric Models and Cross-Spectral Methods," *Econometrica* 37(3) (August, 1969): pp. 424–38.

[14] David Card and Alan B. Krueger, "Minimum Wage and Employment: A Case-Study of the Fast-Food Industry in New Jersey and Pennsylvania," *American Economic Review* 84(4) (September, 1994): pp. 772–93, is probably the most famous natural quasi-experiment. For a retrospective on the move towards natural experiments, see Joshua D. Angrist and Jörn-Steffan Pischke, "The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics," *Journal of Economic Perspectives* 24(2) (Spring, 2010): pp. 3–30.

[15] See, e.g., Joshua D. Angrist and Alan B. Krueger, "Instrumental Variables and the Search for Identification: From Supply and Demand to Natural Experiments," *Journal of Economic Perspectives* 15(4) (Winter, 2001): pp. 69–85.

attempt to limn patterns sufficiently vividly that they offer compelling summaries, but are never really able to step free of the limitations of evidence. In this view, events only ever happened once and that happening is done. There are no tests and no experiments, hence no knowledge. History isn't even just chronicles. There is analytical history, making arguments with more general truth claims. But it is all on the basis of selective treatment of evidence.

Such a view of the inherent and global superiority of the classical statistical hypothesis-testing style carries quite a lot of freight, most of it neither visible in the bill of lading nor acknowledged by practitioners. Its limited notions of ontology (in both historical and statistical social science work) and knowledge are unarticulated and, as one thinks about them, not obviously tenable. But these are well-established and complex issues exploring which calls for a less length-constrained setting. Here it would be most helpful simply to begin with an account of what is involved in writing what historians would regard as "good" history.

Ask a professional historian what constitutes good history and the answer will have several elements. One is fundamental: the facts as recounted and relied upon have to be correct and verifiably so. But even medieval chronicles were selective: having the alleged facts be true is necessary but constitutes only the beginning of the historian's real work. Another element is a matter of framing: it is essential to ask of the available evidence an interesting and significant question.[16] A third is still preparatory and strictly methodological: researchers need to treat sources with a certain degree of skepticism, checking one against the others to detect and counter-balance bias, not to speak of outright fraud. A fourth is interpretive but still straightforward, at least in principle: this is to engage, to be true to all of the evidence—contemporary print and manuscript documents, statistics, personal archival, or interview materials (that is, more intimate traces of what individuals observed, experienced, and thought)—and not just to a supportive subset of it. All once extant evidence has not survived, but historians must query the universe of the evidence which has, a process which routinely forces revision of initial assumptions and questions.

The final part is both interpretive and potentially not so easily reduced to a set of simple concrete instructions: it is to treat the surviving evidence in a deeply imagined way. Interpretation is inevitably an *ex post* construct, but historians recognize further that the material they analyze itself already and always presents interpretations by those who created it. The idea here is to attempt to encounter, as much on its own terms as is humanly possible, the experience of past circumstances as actors encountered them at the time. This is important because historians see their task as understanding why events

[16] Almost all professionally written history addresses questions at least implicitly, usually in the context of prior literature or debates.

developed along one particular course rather than another. It is generally of the greatest importance in doing this to have a sense of what meanings were and how opportunities appeared to contemporary figures who could actually exercise agency. Martin Luther is thought to have said at his trial at the Diet of Worms that he could do no other. These are figures who actually could do something other.[17] To understand agency, one wants to explore what was possible; and to explore what was possible is in part to explore what actors found imaginable. In this in particular we can begin to return to the subject of this volume: if business history is the history of organizations, the history of inter and intra-organizational routines is a part of business history. For organizations are not just observable institutions. They are lived collective experiences.

We can now begin to situate business history among the social sciences, at least as these appear in management academia. The first step is to consider what happens to inference from evidence, once one abandons the assumptions of general laws to be discovered and of random samples of evidence revealing them and the casually assumed insignificance of the other statistical concerns cited above. Precision of estimates, and the concomitant ability to distinguish statistically alternative characterizations of the evidence to hand, are good things all else equal. But a realistic assessment of these inferential problems suggests that the equation of reported estimate precision with confidence in interpretation is often rather strained. The situation is actually a little worse when the problem isn't so much the dubious deployment of standard tools as a single-minded devotion to characterizing a set of quantifiable data without much attention to how coarse the data is relative to the explanatory concepts being invoked or to the cases or other evidence sources that have in one way or another been pre-filtered. In situations like these, which are not at all uncommon, the usual cost of the precision is a loss of grip on individual circumstances; and if the value of the precision is reduced, the trade-off against the reliability of interpretation may be worth critical reconsideration. This is, of course, particularly so if the existence of general patterns stands as a question rather than an assumption.

At the radical extreme of the opposite approach, one would proceed with specific cases and, with the business historical materials sometimes at hand, investigate them in an intensive and an inward- as well as an outward-looking fashion. Then in the best case, with sufficient detail available to genuinely distinguish situational possibilities, generalizability might be limited (though analyzable) but the work would result in a firm grip on actual influences and

[17] Luther's response to Catholic leaders' demands that he recant positions they deemed heretical is widely thought to have been "Here I stand. I can do no other. God help me." See e.g. Roland Bainton, *Here I Stand: A Life of Martin Luther*, New York: New American Library, 1950. (There is some debate among scholars as to whether he actually said it.)

perhaps even on causal relationships. The key challenge is isolating causal relationships or at least fields of influence. Social scientists may be inclined to wave off this possibility, particularly in the face of historians' general modesty about their claims. But these professions are often excessively cautious. Identification, if in informal terms, is exactly what good historians with suitable materials do. Part of what makes good history good is that its authors take inference from their evidence as far as it goes and no further.

Part of the reason a project such as this can succeed is that much of life, and certainly much of business life, can best be characterized as a path-dependent process. Explicitly imposing an assumption of equilibrium as a central element of interpretation often misses out influential, and sometimes even causally important, factors (for example, business actors don't generally seek equilibrium with competitors; usually they seek triumph over them and hence a durable asymmetry). A process perspective can be valuable. When action is contemplated before it happens, a process perspective that does not ignore cognition (in the broad sense, incorporating all three of Dewey's elements of habit, impulse, and deliberation rather than just the last of these) is more important still.[18] This, rather than equilibrium conditions (and still less the assumption that observations represent a state of equilibrium) are the most effective way to populate the landscape of business history's interpretive storytelling.

This suggests that the proper process for explicating firm decisions runs through firm decision making (taken in the evolutionary economics sense in which the "decision" is often to continue doing what everyone had been doing before, instead of something new) and involves exposing process and the mediation of acts and institutions, clarifying what was possible and what was not. Since reasons can be causes, this suggests that the thinking, understanding, and imagining by potential actors represent foundational elements of the picture, and that restricting the information to be analyzed to overt events is quite misguided.[19]

More profoundly, it suggests that exposing process and mediating relationships is also important in clarifying actual causal links and sequences. It is very rare that an experiment can unambiguously reveal causal connections, even in the sciences; and this is even more true in fundamentally non-experimental domains of inquiry.[20] All the varieties of researchers we have been discussing are engaged in the activity of hunting for causes; and it is generally true that

[18] See the discussion of Dewey's ideas in the introduction to this volume and the works cited in its note 12.

[19] Donald Davidson, "Actions, Reasons, and Causes," *Journal of Philosophy* 60(23) (November 7, 1963): pp. 685–700.

[20] On the first point, see Willard van Orman Quine, "Two Dogmas of Empiricism," *Philosophical Review* 60(1) (January, 1951): pp. 20–43.

their work gets not much further than ruling out some possibilities, in narrowing the set of options, rather than somehow revealing the truth. There are many paths to identification, or at least in its direction. Sometimes, to get from here to there, what you want is not a highly abstracted map but a really well-informed local guide.[21] This approach to knowledge may lack the aura of unambiguous knowledge and universality, but complicating a simple picture often clarifies what is actually happening.

The question "What good is history?" can be approached both in the large and in very specific settings. To begin with the former, historical narratives enable contemporary people to find inspiration for action based on heightened understandings of how organizations, processes, and practices have worked and failed in the past. This is not a matter of mechanically deducing specific action rules from past events but rather of sharpening actors' alertness to environmental features that otherwise might be omitted from decision-making consideration.

Individuals learn from history constantly in this sense; indeed failure to do so is a fine flag for persons who cannot function in responsible roles in organizations. Similarly, groups—sports teams, for example—systematically learn from history (from their own prior game performances or about the weaknesses of particular rival players or the obnoxiousness of fans in some places) in order to improve outcomes. Moreover, both for individuals and groups, preserving such history, of triumphs and failures alike, configures the long-term meanings that constitute identity. By extension, we recognize that organizations learn from history in ways both implicit and explicit, and that those seeking to operate as if in a perpetual present deny themselves the value of history as a means to understanding prospect as well as practice.[22]

Much, perhaps most, organizational learning from history is implicit, bound up with routines created long before current employees arrived, with durable rituals and even occasional tall tales of insight and obtuseness or heroism and folly circulated and handed down, or with quietly shared work-arounds that get things done while avoiding particularly difficult managers, offices, and official procedures. Part of what we argue in these pages is that embracing explicit undertakings to learn from history can also bring organizational rewards, not least by recognizing historical situations, challenges, dynamics, hazards, or contexts that are instructively analogous to those we encounter today, and that can condition our planning for decisions by regarding historical phenomena as informal models for current consideration.

Just like us, actors in the past did not know how the efforts they were undertaking would work out, for good or ill. But we have the opportunity

[21] Keith Thomas, "Working Methods," *London Review of Books* 32(11) (June 10, 2010): pp. 36–7.
[22] Indeed, the accumulation of such learning constitutes organizational culture.

not just to know what the outcomes were, but also to research the developments through which these outcomes materialized, including alternatives foregone, actors' or rivals' omission of (what turned out to be) key elements in planning, critical innovations in process or practice, perhaps initially unpromising, that generated unanticipated benefits, and the like.

In this spirit, we would characterize learning from history as open-system learning, in which feedback loops, restructuring based on incoming information, repeated questioning of strategies and structures, and inductive generalizations are central to maintaining fitness within continuously shifting circumstances. By contrast, social scientific management and organizational theory, we would suggest, when seeking universals and patterns and rules relatively indifferent to time and place, trace a deductive pathway within closed systems, where simplification and quantification are necessary tools for achieving high-level generalizations. The proliferation of theories and critiques in organizational and management science suggests that an arc of disappointed expectations has been inscribed in this domain, a series of analytical failures that are one result of imagining that intellectual order maps reliably onto and can shape social practice—something we regard as a basic category error. Historical cases and analyses, by contrast, help situated actors anticipate the disorder that so commonly arises in organizations, allowing them, for example, to create buffer spaces and times to deal with decision-making process surprises, rather than relying on the programmed, advance scheduling that such surprises derange.

In the historical literature, rich examples of category errors and underdetermined rational expectations are readily available. Studies by Peter Hall, Charles Perrow, Dietrich Dormer, and James Scott underscore the strong incentives organizations provide those who streamline problem-solving practices, create rational models, reduce time to decisions, and attack immediate issues, then resist questioning their assumptions when errors propagate and unanticipated consequences mount.[23] Learning from history provides organizational resources to avert hazardous oversimplifications and displace assumptions of continuity between present and future situations.

For however much planners try to routinize operations, the life of organizations appears to be a path-dependent process. Of what is possible and what is not in strategy, this seems to be even more true. Of what happens in the earliest days of enterprises, successful and unsuccessful alike, this seems the most true of all. Path dependency is written into the objects of study as deeply as the sun, the moon, and the stars are a part of life on earth. Details do have consequences.

[23] For four classic examples, see Peter Hall, *Great Planning Disasters* (Berkeley: University of California Press, 1982), Charles Perrow, *Normal Accidents* (Princeton, NJ: Princeton University Press, 1999), Dietrich Dorner, *The Logic of Failure* (New York: Basic Books, 1997), and James C. Scott, *Seeing Like A State* (New Haven, CT: Yale University Press, 1999).

We can put such celestial sentiments in terms that will be very down to earth for this volume's intended readers. Increasingly, as noted above, students in business schools and elsewhere want to learn how to start enterprises. This is a subject matter for which, certain essentially institutional details aside, general laws and principles tend to be few and for which process—actual management—is very important. Such students will get valuable stimulus from thinking about well-crafted case studies of well-chosen subjects. The key feature a case should present to be successful in this role is not that it delivers a general answer but rather that it vividly raises general questions. Being forewarned is not necessarily being forearmed, but it is—in these matters, perhaps for the researcher but certainly for the teacher and students—a valuable first step.

Generally in academic life, the producers of research are, in their teaching, also consumers of it. Among the population of active researchers, the reverse is also generally true. This chapter has argued, however, that the incentives facing most management academics militate against the production of literature about the emergence of order in general and of organizational routines in particular, by people who teach the very subjects for which having such literature would be most useful. Historians are by their training well suited to producing it. Historical research does not have the same form as social science research, but this chapter has argued that while high-quality historical research has its limitations (a feature it has in common with social scientific research), it is epistemologically sound—absolutely not the naïve empiricism that some imagine—and may well be, in important respects, better suited to this subject matter (or any in which individual cognition and agency is a potentially influential, never mind decisive, feature). All that said, there is an aspect of the rhetoric of historical writing that may strike the social science sensibility discordantly. We address that aspect in concluding this chapter and the volume.

We have described the spirit of writing, and reading, critical history as one of complicating rather than simplifying the picture of what is going on in some particular event or domain. "Complicating the picture" strikes some researchers as introducing clutter to potentially simple, clear, and lean-limbed relationships. It is, so to speak, "sound and fury, signifying nothing." It is nothing but noise; and modern computational capacity and statistical methods enable noise reduction on a monumental scale. There may be problems with modeling assumptions, however; and there may be problems with the Gauss-Markov conditions and their equivalents of extreme proportions. Larger circumstances, abetted by conventions of convenience in an ongoing academic community, conspire to leave these shortcomings generally unaddressed. But doing so also leaves aspects of the life of organizations unaddressed as well. Sometimes these matter in merely intellectual ways. Sometimes they matter for deeply understanding the "data."

Our alternative view favors "complicating the picture" because such complications often illuminate the how and why of things proceeding and turning out one way rather than another. Because those who exercise agency—either in self-conscious decision making or in the thousand and one minor steps and interactions that constitute routines, problem solving, and general operations all the way down the organization to the day-to-day activities of operations—act in contexts and with histories and understandings, an account that simply seeks patterns in outcomes and correlations to coarse descriptors leaves out too much. It will in the end always be inadequate to a number of cogent purposes germane to managing well.

Some detail is clutter, of course. There is an enormous amount of simple repetition in the daily life of large organizations. And many explicit decisions have a mechanical quality to them. Yet so much concerning agency eludes the evidentiary net of social scientists. What is required to turn the routine of organizational life into useful, or even actionable, information is embedding those details into an image of sense making, alternatives, and action. This amounts to returning agency to the image of the people in organizations, people high and low. It is not incompatible with an image of organizations running mainly on routine in the ordinary language sense of that word and even with one in which the occasions on which overt departures are required are rare. It isn't about what happens so much as it is about what sort of figures are involved in its happening and how they make sense of it all.[24]

These chapters try to keep this thought in mind while building a picture of the coming to life of organizations and institutions within them and of groups of organizations acting in one way or another in concert. There are lessons to be drawn from this picture, providing less simple but still valuable food for thought. A sense of what things must happen is helpful when going into a situation in which nothing is yet fixed. A sense that things can evolve prepares the mind for confronting situations in which the urgent question is how things might do so and what one ought to think about the various possibilities. History can be valuable even to people long after they exit educational institutions, just as it can be to students who want some sense of the worlds and roles into which they want to enter.

[24] See Karl Weick, *Sensemaking in Organizations* (Thousand Oaks, CA: Sage, 1995).