How to Do Things with Time

DANIEL M. G. RAFF

Sample selection bias is a common problem in the business history literature. This paper proposes methods for researching and writing the history of firms and industries designed to address the problem. The key elements are a forward-looking perspective and close attention to the development over time of selection environments, the resources individual firms can mobilize, and understanding an agency within the firm or firms.

"It is hard to think away out of our heads a history which has long lain in the past but which once lay in the future[.]"

F. W. Maitland
"Introduction [to 'Memoranda de Parliamento']"
Records of the Parliament Holden at Westminster on the Twenty-Eighth Day of February, in the Thirty-Third Year of the Reign of King Edward the First (AD 1305)
(London: HMSO, 1893): lxxxiii

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> "The theme is loss, we take it." "What," she said, "else?" "Are you specific as to what is lost?" "Brutally." "Snow White," we said, "why do you remain with us? Here? In this house?" There was a silence. Then she said: "It must be laid, I suppose, to a failure of the imagination. I have not been able to imagine anything better."
>
> Donald Barthelme
> "Snow White"
> *The New Yorker*, February 18, 1967

The text that follows lays out some views on the methods appropriate for studying firm and industry history over time. Although trained as a neoclassical economist, I was struck, as many others have been, by the account of firms and firms' decision making in Nelson and Winter's 1982 monograph.[1] Indeed, after a quarter of a century studying actually existing firms and teaching students of business administration, I have come to think of this essay's subject as a question of evolutionary economic history. I am still enough of an economist, and enough the son of a mathematical statistics professional, to believe that some careful thought about method can be helpful before confronting evidence. So my question here can be stated simply. What sort of historical methods would be appropriate to writing the history of a world in which evolutionary economics was (in a broad sense) true?

This question bears closely on a long-established strain in (if not indeed a central part of the historic core of) business history. The answer I propose is not utterly remote from the literature on the social, cultural, and political context of business of the past several decades so valorized by Richard John in a recent review essay.[2] But it is very much focused on what happens inside firms, what factors influence how well firms do at any moment in time and over their life courses, and how populations of firms evolve over time. This internally oriented style of business history may be less popular now than it once was. I hope to show in the following text, if it needs showing, that there is life in it yet.

One group of individuals of interest to business historians that does not regularly read *Enterprise and Society* but that might find my question cogent is researchers and teachers concerned with the business school subject called strategy or, sometimes, competitive strategy. This field has historic origins but now seems to many rooted in

1. Nelson and Winter, *An Evolutionary Theory of Economic Change*, 1982. For present purposes, the central ideas are that information is limited and rationality bounded, organizations function for the most part in a satisficing mode, carrying out routines that tend to persist over time, however much they may also search for new routines from time to time, and that the behavior of populations of organizations changes predominantly through selection processes, such as entry and exit, rather than through the sort of intraorganizational optimizing adjustments envisaged by neoclassical economists ("feedback, not foresight").

2. John, "Business Historians and the Challenge of Innovation."

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the empirical observation that even within fairly well-defined industries, some firms persistently have very much better profit outcomes than others.[3] Strategy is concerned with the question of why this is so but proceeds from the perspective of firms and senior decision makers in them rather than from the perspective of market equilibrium (as an economist might). The characteristic strategy question is "What objectives might a firm pursue, and by what means, to produce sustainably superior operating results?"[4] Business history is on this account intimately related to strategy.[5] One might even argue that it is, in principle at any rate, the evidence behind strategy.

There is a short answer to the historical methods question; and it is a different answer from the currently dominant modes of business history writing (and, equally, their counterparts in contemporary management research). The short answer is that moments of decision making, suitably embedded in operating environments and cash constraints, are a more-revealing and less-distorting focus than outcomes, that decisions cannot be analyzed properly without serious consideration of the alternatives, and that any emphasis on the consideration of alternatives lays bare the dependency of decision making on actors' understanding of what courses are in fact open to them and what the consequences of pursuing these would be, that is to say on the state and development of the actors' knowledge and understanding.[6] Time plays a central role in any such analysis, not in the role of an ad hoc assumed period of adjustment from one equilibrium to another—either as a parameter to

3. The empirical economics (i.e., statistical) literature on this point begins with the article by Schmalansee, "Do Markets Differ Much?" Important subsequent contributions in this vein include the studies by Rumelt, "How Much Does Industry Matter?"; G. F. Waring, "Industry Differences in the Persistence of Firm-Specific Returns"; McGahan and Porter, "How Much Does Industry Matter, Really?"; McGahan and Porter, "What Do We Know about Variance in Accounting Profitability?"; T. W. Ruefli and R. R. Wiggins, "Industry, Corporate, and Business-Segment Effects and Business Performance: A Non-Parametric Approach"; and McGahan and Porter, "Comment on 'Industry, Corporate and Business-Segment Effects, and Business Performance: A Non-Parametric Approach.'" (Porter's celebrated books approach the same subject matter in a rather different style.)

4. There is an emerging subliterature, to which I am highly sympathetic, which takes the view that this characteristic question is a step away from traditional microeconomics in the right direction but a smaller step than would be desirable. For a generally accessible exposition, see, e.g., Harborne Stuart, "Value Creation, Competition, and Business Strategy." Adopting this new view is entirely compatible with the remainder of the paragraph.

5. This would be particularly so if the superior outcomes derived from capabilities, themselves deriving from commitments that are costly and not easily reversed. For a classic statement of that view, see Ghemawat, *Commitment: The Dynamic of Strategy*.

6. I write here as if it is obvious who the relevant actors are. This is clearly not necessarily so, though it often seems to be in the sort of situations I wish to consider. (Indeed, seemingly innocuous "actor," "decision making," and "choice" language involves more restrictive assumptions and potentially misleading connotations than may immediately be obvious. I go into this in more detail later.)

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be estimated in an assumed functional form or as a sort of expository device to keep everything from happening more or less at once—but rather as the medium through which understanding and the imagination of possible future courses of action develop. And agency plays a central role in what is being described: a history of actors exploiting resources and time to develop ideas about what might work and to learn what actually does—at least well enough—is not a history of watching time pass but rather one of actors doing something with it. Similarly, writing such a history is a matter of exploring what happens in time. This is the meaning—these are the two meanings, really—of my title.

This is a very summary account. Short answers are no more than provocative without questions. (This is, indeed, an implication of my analysis.) These short answers are in fact a response to difficult problems. I should begin with the problems.

Before launching into that, however, a brief further introduction may be in order—some people encountering the text that follows may be coming from a rather different background than that presumed previously and experience suggests that a little, rather different, orientation may be helpful for them. In this essay, I want to explore how (and what) we can learn reliably about a particular type of social science subject. The subjects at issue here are intrinsically historical. The thought of exploring them systematically brings to mind common problems with much social science research as one encounters it in the seminar room.[7] But this class of subjects raises questions of its own; and these questions may seem particularly salient to conventional social scientists. Historical explanation, after all, may not seem to fit so well into the basic model of high social science explanation. In the latter model, law-like hypotheses are confronted with empirical evidence (i.e., data). The data are construed as a sample and the question addressed is essentially how likely it would be for the sample to be observed if some hypothesis was in fact true.[8] It may be difficult to answer this

7. For example, presentations that offer highly elaborated discussions of particular test statistics but seem oblivious to the distinction between statistical and substantive significance (i.e., the distinction between being able to estimate the value of a parameter in an equation relatively precisely and that parameter having a large effect on the dependent variable) and are very casual about counterfactuals, assume rather than investigate the satisfaction of the Gauss–Markov conditions or their counterpart (i.e., assumptions required for estimation procedures to have their canonical properties), and seem completely naïve on the subject of the distinction between a sample and a population.

8. The classical question, more precisely put, is how likely it would be for the observed pattern to be observed if some null hypothesis—e.g., that there is no systematic relationship—were in fact true. The locus classicus for this approach is R. A. Fisher's *The Design of Experiments*. (The contrasting approach is that of the Tukeyean exploratory data analysis [see, e.g., Tukey, *Exploratory Data Analysis*]. The approach I will outline later has, at a certain level of abstraction, something in common with Tukey's methods.)

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entirely persuasively; but it certainly is an intelligible and well-formed question. Compared to high social science data sets, the evidence of history generally seems quite unruly. The whole rhetoric of sampling may seem inapt since it is not clear that historical settings, fine veined and particular as they may be, ever repeat themselves.[9] It may also not seem entirely clear what it is to be an explanation, still less a cause, in history.[10] It may seem even more obscure how one would argue, still less enhance, the credibility of a causal analysis. Part of what I want to do in this analysis is to probe a little more deeply into what it really is to explain, and to explain persuasively, phenomena in this domain.[11]

I begin with the domain context—with a professional discourse and some literature. Economic history as a subject might well encompass the entire variety of economic life. As a practical matter, the term has come in Anglo-American circles predominantly to refer to aggregates—global trade, the economic development of nations, markets for particular goods and services, and so forth. (The history of consumption sits somewhat uncomfortably between economic history in this sense and social history.) Business history as a subject is more disaggregated. It concerns firms in an essential way: what happens between them and consumers, what happens among firms, and what happens within firms.[12] These are the professional discourses on which the following text touches most closely.

There has been *methodenstreit* in both these arenas. The one in economic history had its peak in the 1960s and 1970s. The traditionalists worked from archival and mainly prose sources, while

9. Mark Twain is thought to have written that while history does not repeat itself, it does often rhyme. It is an amusing fact given the content of the remark that while it sounds very much like something Twain would have written, no one seems to be able find a source. The remark bears consideration nonetheless; and I will return to it later.

10. One might well, to choose a trite example, ask whether the man on a white horse should count as an explanation. During the advent of the statistical approach to economic history (for more on which, see the following text), historians sometimes complained that the explicit construction and analysis of counterfactual propositions, to which the high social science approach would generally seem committed, was fundamentally alien to their craft. (See, e.g., Redlich, "'New' and Traditional Approaches to Economic History and their Interdependence." *Journal of Economic History* 25, no. 4 [December 1965]: 480–95.) The general, and I think persuasive, response was that this only involved making explicit what the more conventional historians were doing implicitly all along. (See, e.g., the closing section of R. W. Fogel, "The New Economic History: Its Findings and Methods." *Economic History Review* 19, no. 3 [December 1966]: 642–56.)

11. This is related to the question of what cases and case studies might be useful for the business school curriculum. I will not have anything explicit to say about that here, but on case studies and business schools (and how business historians might fit in), see my "The Teaching of Business History in the United States."

12. Business history can concern much more, as perusal of the pages of any recent issue of this journal will demonstrate. My point is only that there are individual businesses in it in an essential way.

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the Young Turks were quantitatively oriented and framed their questions in terms of (typically Marshallian) economic theory.[13] If markets worked, how big was the effect? And did it matter? The tone of the discourse was strongly positivist, not offering hermeneutic-style interpretations but rather framing crisp hypotheses and seeking to put them to an unambiguous statistical test.[14]

The conflict between the two groups was quite sharp in the beginning, though the fact that most of the Young Turks ended up working in economics rather than history departments took off some of the edge. Eventually, what was once radical and imaginative became, for the most part, conventional and routine. The Cliometric tradition survives and still sometimes illuminates. But it is, for the most part, a much more modest undertaking now.

The tension in business history is both subtler and less advanced. It is subtler in that it is rare that issues of interest to business historians admit of statistically testable questions and rarer still that data on which such tests might be conducted are available to researchers. On the other hand, the impulse behind the struggles in economic history was only in part about measurement and tests.[15] It was, as I have suggested, in major part an idea about the role of theory, about the interplay between abstract ideas concerning how and why systems under study proceed from one observable state to the next and the observable evidence itself.[16] Much business history actively shies away from theorizing. And in the wider subject to which one might look, many theorizers, even theorizers about firms and their interactions, seem to know little about actually existing firms. (Some even seem to try to make a

13. For an early brief overview of results, see the study of Fogel cited in Footnote 10 and reprinted as the opening chapter of R. W. Fogel and S. L. Engerman, *The Reinterpretation of American Economic History*. For a confrontation between old and new, see G. R. Elton and R. W. Fogel, *Which Road to the Past? Two Views of History*.

14. Taking all this together, one leading figure wrote of "economic history as a form of applied economic analysis" See Temin's "Introduction" in *The New Economic History: Selected Readings* (The Fogel and Engerman- and Temin-edited volumes cited in the preceding footnote and in this one are the two standard collections of the period.) Making the counterfactual points of comparison explicit was a major theme in this discourse and the detail of the appropriate counterfactuals was sometimes a topic of fierce debate. On the first of these, see (most famously) R. W. Fogel, *Railroads and American Economic Growth: Essays in Econometric History*, on the second, David, "Transport Innovation and Economic Growth: Professor Fogel On and Off the Rails."

15. Though this did play a significant role in the struggles' genesis. Fogel, the great entrepreneur and provocateur of the old New Economic History, says he spent most of his twenties on Communist Party picket lines. After he became disillusioned, he told me once, he decided he wanted to go into a line of work in which he could tell with confidence whether an argument was right or wrong.

16. Note that this usage of 'theory" is not quite the one postmodernists make. I have a little bit to say about postmodernism—see the remarks about Quine and empiricism later.

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virtue of this.)[17] Yet, one might think that theory, or at least generalization, might be useful in making sense of practice and its history.

The grand synthesizers of course do generalize. But the focus of their effort, and of the pride of the grandest of them, lies in documenting their story in great detail.[18] I do not wish to quarrel with the individual details they adduce. The issues I want to raise enter in during the framing stage of construction—in the formation, to mix my metaphor a little—of the big picture of what is to be explained.

Let me give some more specific context. It was easy for middle-class Americans in the years just after the recent millennium, or indeed in the period before, to take for granted the smooth and relatively efficient coordination of a large and complex economy. Even in eighteenth-century Scotland, Adam Smith had been impressed with the organization of activity within firms as well as by the coordination mechanisms working among them.[19] Observing America in the years around 2000, Smith would surely have been not so much impressed as astonished.[20]

The dominant account today of coordination mechanisms in American economic history is the work of a business historian, Professor Alfred D. Chandler, Jr. [21] Chandler's major writings transformed the study of American business history as they emerged, and his perspective has unambiguously dominated the field for the past quarter century or so. Precursor works of Chandler's have been influential for a longer period still.[22] For reasons developed later, however, I think that the time is ripe for a new synthesis.

Chandler, in particular in *The Visible Hand*, emphasized the notion of the superior efficiency of large-scale managerial enterprises over market mechanisms in the coordination of economic activity. In particular, he attributed the success of the US economy in the twentieth century to the rise of a coordination mechanism, which,

17. Friedman, "The Methodology of Positive Economics." Winter, "Developing Evolutionary Theory for Economics and Management," is very helpful on this point.

18. Alfred D. Chandler, Jr., on whose synthesis see the following text, was fond of saying that the first responsibility of the historian was to say what happened. (The core chapters of his first important monograph, *Strategy and Structure: Chapters in the History of the Industrial Enterprise*, do not seem to me so vulnerable to the critique I advance in the following text of his later and more synthetic works.)

19. See, e.g., Smith, *The Wealth of Nations*.

20. See the rendition in Paul B. Seabright, *The Company of Strangers: A Natural History of Economic Life*.

21. His masterpiece is *The Visible Hand*. His *Scale and Scope* takes the basic analysis somewhat further and puts it in comparative perspective. His later *Inventing the Electronic Century* and *Shaping the Industrial Century* have had less impact.

22. The most important of Chandler's early essays can conveniently be consulted in McCraw, ed., *The Essential Alfred Chandler*.

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he argued, was vastly superior to the market. This was the large-scale enterprise, directed by bureaucratic hierarchies of salaried professional managers, vertically integrated to capture economies of scale, and (in time) horizontally diversified to capture economies of scope. The American economy did better than others in significant measure, in Chandler's view, because it embraced this mechanism most extensively.

There are problems with Chandler's history, however. I will argue that the problems are to some extent matters of substance but are more deeply understood as deriving from a fundamental problem of methodology (and one quite distinct from the business history *methodenstreit* referred to previously). The approach I will lay out can be mobilized to address both. The past appears, on the view I will develop, to have a very different character and the future appears in a novel perspective.[23]

It seems obvious that a new synthesis is in order when one considers the ways the business environment and outcomes have changed in late twentieth century. Classic Chandlerian firms have run into problems. More specialized, vertically disintegrated firms have sometimes done better even in those original firms' own industries. Intriguingly, these firms have substituted for managerial direction forms of coordination—e.g., long-term relationships—that historians tend to associate with earlier eras, i.e., before the so-called "market revolution" or the rise of big business. And horizontal diversification has for the most part developed a bad name in the capital markets.

The melancholy story is most vividly told in terms of the great exemplar of Chandler's innovations, the General Motors Corporation (GM). GM was one of the earliest implementers of Chandler's key systems and investments.[24] It reaped a brilliant bounty. In its heyday, it commanded more than half the American market—the world's largest and most attractive—for its product. The company was very profitable and seemed the essence of stability in this: its shares were a mainstay of conservative investment portfolios for many decades. It was also one of the largest private employers in the economy and paid a generous wage, so in that way too, its prosperity seemed America's. The widely held view was neatly captured by a remark a GM chief executive made during hearings to confirm him as Secretary of Defense in the early 1950s. "[F]or years

23. See Lamoreaux, Raff, and Temin, "Beyond Markets and Hierarchies. See also Footnote 30.

24. See Chandler, Jr., and Saisbury, *Pierre S. Du Pont and the Making of the Modern Corporation*; and Freeland, *The Struggle for Control of the Modern Corporation*.

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I thought," said "Engine Charlie" Wilson, "that what was good for our country was good for General Motors, and vice versa. The difference did not exist."[25]

The American economy has waxed and waned since those words were spoken. But it has for the most part done quite well; and it remains the largest, and the richest, market in the world. Yet, in the several decades from the early 1970s, despite all efforts, GM's decline was been steady and apparently inexorable.[26] Its share of the domestic market sank to less than one-third.[27] Its profitability lagged terribly. The problem was not that industry profitability had collapsed. Toyota—the leading Japanese-based firm and a major force in the US and global markets more broadly—was a vivid counterexample. (It was for many years so profitable that, in Japan, it was sometimes referred to sardonically as Toyota Bank.) GM's extensive campaign to improve its production quality with robot technology in the 1970s was widely regarded as a failure and a waste of money on the scale of billions of dollars. The company's attempts to develop new models for work organization and human resource management ended up largely isolated at the test site. Its performance in developing new products was better than it had been but remained far off industry best practice. The company's relations with its suppliers continued to bear the burden of an unhappy past: the captive factories were spun off to an uncertain fate and the remainder of its sources continued with the familiar adversarial and suspicion-laden routines. Information technology seemed, on balance, to exacerbate these problems rather than ease them. GM's channels of distribution remained expensive, inefficient, and basically entrenched. They were not transformed by new information technology either. And the basic product was mature. GM lost business and margin to firms that were organized differently (internally and in their value chains). It shed noncore businesses; but students of the industry did not expect any dramatic turnaround in its core even before the company's dramatic problems in the recent and nearly disastrous macroeconomic crisis.

How idiosyncratic is this story? The most careful work to date on the fate of the giant enterprises of the early twentieth century as a group indicates that relatively few in fact sustained their

25. "Excerpts from Two Wilson Hearings on Defense Appointment," *New York Times* (January 24, 1963).

26. The best nonscholarly book on this is by the leading Wall Street analyst of the industry of the day. See Keller, *Rude Awakening*. For a somewhat later view, see Maynard, *Collision Course*.

27. After bankruptcy, the figure has been approximately or slightly less than one-fifth.

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successful positions over an extended period.[28] More striking still, other research suggests that the vibrant performances of the past third of a century have come from sectors and firms organized with structures and emphases different from those in Chandler's firms.[29] The self-promotion of present-day management consultants to the contrary notwithstanding, prototypes for these arrangements seem to exist much farther back in American history. In that light, Chandler's corporate economy appears more as yet another phase than as an end point. The recent crisis and the company's resort to bankruptcy protection to remake fundamentally long-term contracts and financial commitments in hopes of reconfiguring itself more survivably relative to changed circumstances only emphasize the aptness of this assessment.

The appeal of an explicitly evolutionary approach, with its bounded rationality, routines, satisficing, and selection pressures, in understanding the sort of history I have just sketched seems obvious. Companies and their counterparties do not know precisely what to do. Satisficing rather than optimization is the way they pursue profits. They have routines; and outside of these, they fumble about. And so forth. But the implications of a Nelson-and-Winter-style perspective on making sense of historical evidence are more profound than this. The Nelson-and-Winter perspective signals a deeper problem than just short-run inefficiency and the lack of an end point.

Two colleagues and I have taken a preliminary pass at fixing the history of the Chandlerian firm.[30] We attempted, through a reconceptualization and reconsideration of the evidence, to situate the changes of the past quarter century or so in the broad sweep of a history we viewed as both complex and still unfolding. The way we tell the basic story, it is not a simple tale of the triumph of a specific type of coordination mechanism—a tale inevitably told

28. See, e.g., Hannah, "Marshall's 'Trees' and the Global Forest." Hannah's evidence suggests that this is a more complex and pointed phenomenon than the simple regression to the mean often observed in statistical studies of industrial organization.

29. This fact has, of course, been widely noted: even General Motors has sought to revive its fortunes by alliances and experiments. For something relatively close to primary source evidence (though of course not to be confused with that), see, e.g., Badaracco, Jr., "General Motors' Asian Alliances," and McMahan, "Saturn Corporation in 1998."

30. The version that eventually appeared in print is the article cited in Footnote 23. The *American Historical Review* space constraints were very severe, however, and the paper as published is in most respects a miniature, and underdeveloped at that. Our "Against Whig History" is a little better but is itself constrained by the format of a debate. This essay represents the first sustained attempt to work out and articulate the methodological foundations.

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looking backward. Rather, it is an account of the dynamics—the ongoing history—of a population of coordination mechanisms. The particular mechanism showing such a stark and simple profile in 1977 casts a much more nuanced shadow a quarter century and more later.

In filling in the history's details from this rather different perspective, we naturally sought to do more than observe that classic Chandlerian firms no longer fit in so simply and straightforwardly. We tried to analyze why this is so in our first study. In doing so, we found it helpful to go beyond the simple contrast between market relations and hierarchical ones, showing both how these two basic forms are more complicated and socially constructed than is sometimes appreciated and, more important, that long-term relationships, interfirm networks, and industrial clusters have long played a more important and more dynamic role—in particular, a more important role in the history of innovation and of response to opportunities and shocks—than the conventional view acknowledges.[31] This in itself led to a revision of Chandler's account of the historical record of American business.

More deeply, recognizing the basic task to be explaining why any particular coordination mechanism does or does not do well in some particular setting suggested a very different approach to the analysis and writing of the history of coordination mechanisms, and of business history more broadly, than Chandler's. We put forward an approach in which—because environments do change over time—robustness of the analysis to change in the moment of observation is a central virtue. Proceeding in the fashion we suggested was and remains very different from standard practice and represents a new synthesis in a rather different sense from the one usually invoked by historians.[32]

The approach we began to develop involved considering business history as a history of choices rather than outcomes, placing great emphasis on understanding what the essential and unavoidable

31. On the complexity of markets and hierarchies as they actually exist, one might point on the one hand to the standard setting required for the development of a national-scale wheat market (see Cronon, *Nature's Metropolis*, 104–19) and on the other, to the remark of Wellington—Wellington!—that "[n]obody in the British Army ever reads a regulation or an order as if it were to be a guide for his conduct, or in any manner other than as an amusing novel." (quoted in Halévy, *History of the English People in the Nineteenth Century, Volume I*, 85.)

32. Historians use the phrase to indicate a new interpretation of a well-known body of evidence. Generally, this involves placing more weight on some elements previously thought not significant and less weight on elements previously thought central. The common usage among chemists, in contrast, indicates a sequence of actions, a new path to a familiar endpoint. To a chemist, a synthesis is nothing but a process. This is the sense that I have in mind.

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tasks of business administration were and what problems in carrying these out and establishing an attractive competitive position presented themselves to the responsible figures in specific historical contexts.[33] Doing so leads naturally to a program of, on the one hand, reconstructing alternatives and the logic and processes by which choices came about and, on the other, systematic consideration of how selection environments—rather than just the decisions—evolved over time and dynamically influenced the available alternatives. That is the natural precursor to the program described here.[34]

Viewed abstractly, such a bottom–up approach to historical writing is not without precedent, though the precedent that mattered came from a surprising quarter for historians of management, viz., the labor history from below of the 1960s. The trumpet blast of this was Edward Thompson "seeking to rescue the poor stockinger, the Luddite cropper, the 'obsolete' handloom weaver, the utopian artisan, and even the deluded follower of Joanna Southcott from the enormous condescension of posterity. Their crafts and traditions may have been dying," Thompson wrote. "Their hostility to the new industrialism may have been backward-looking. Their communitarian ideals may have been fantasies. Their insurrectionary conspiracies may have been foolhardy. But they lived through these times of acute social disturbance, and we did not. Their aspirations were valid in terms of their own experience . . ." and so on.[35] One of our objectives was to restore agency, and an open-textured sense of the future, to the historical understanding of managers and entrepreneurs.[36]

But we had other concerns as well. Some abstraction may be helpful at this point. A given course of events can be described in a forward-looking or a backward-looking way. In the backward-looking approach, the finish line is set and then the race discovered. The important figures in the race are then those who win or place. The forward-looking approach, in contrast, attends also to those who do not finish the course or end up far back in the pack. It focuses

33. I continue in this paragraph with the convenient conceits of Footnote 6. But again, see later discussion.

34. We had been feeling our way toward such an approach for some time. See the work we wrote or commissioned for a series of conferences we organized, sponsored by the National Bureau of Economic Research, revisions of which were published as Temin, ed., *Inside the Business Enterprise*; Lamoreaux and Raff, eds, *Coordination and Information*; and Lamoreaux, Raff, and Temin, eds, *Learning by Doing in Organizations, Markets, and Nations*.

35. Thompson, *The Making of the English Working Class*.

36. Thompson was, of course, concerned with the agency of a very different set of individuals. For expositions of the agency of their counterparts in a more business historical context, see Montgomery, *Workers' Control in America* and *The Fall of the House of Labor*.

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on the reasons these starters' races came out the way they did. This way of proceeding undermines the natural temptation of the backward looker to prize universal explanations over differentiated ones and ones that attribute long-term survival to persistent conditions—indeed, to conditions hypothesized to be permanent—rather than to transitory ones.[37] The reasons survivors look strong now are not necessarily the reasons they survived the crisis then.[38]

One attraction of the backward-looking approach concerns what one might call theory assessment procedures, i.e., the sort of tests one might apply to an explanation to decide whether it is persuasive. Business history, as noted previously, rarely offers opportunities for crisp hypotheses and direct falsification tests.[39] What else is available? In the writing of business history, generalization and the narrative style are preferred. But narrative is inevitably selective. Chandler had his view. His narrative includes a tremendous volume of context and detail supporting that view. That it does so certainly enhances the credibility of the view. But his narrative also leaves out a good deal. How do we decide whether or not what is left out is important enough to belong in and whether, ultimately, Chandler's emphases are misplaced? The article by Lamoreaux, Raff, and Temin in the *American Historical Review* offers up a different narrative, one that includes Chandler's developments but also includes others, both during Chandler's period and after. The emphases are definitely different. We also implicitly invoke a coherence theory of truth in advancing our position, showing how various salient bits of evidence fit together, and the fact of such coherence is good as far as it goes. But it does not really address the question being raised here. Short of a quantitative analysis drawing on data that do not generally appear to exist (and, at that, resolving presumptively difficult identification problems), how can one make arguments about relative importance?[40]

It is tempting to answer that what is important is what turned out to matter (by one criterion or another). But reflection on this response just reveals that the question of relative importance is indeed bound up deeply with the question of whether the perspective of the present is a biased one.[41] If it is biased (in the statistical sense of sample

37. For a common procedure of economists (the "survivor principle") highly susceptible to this temptation, see Stigler, "The Economies of Scale."

38. This program thus has a natural affinity with that of Fridenson, "Business Failure and the Agenda of Business History."

39. But there are exceptions. See, e.g., Raff, "Wage Determination Theory and the Five-Dollar Day at Ford." (On the falsification approach in general, see Popper, *The Logic of Scientific Discovery*.)

40. That is, the principal competitor to a coherence theory of truth is a correspondence theory. But where is the evidence to which a correspondence theory might be applied?

41. This is, again, the problem with the Stigler approach.

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selection bias), then a coherence criterion applied in the future might be a test of some (statistical) power, since future events might well arise that would cast doubt on the alleged overall pattern. But this must be cold comfort, for the problem facing the historian struggling to make sense of the record now is that the future has not yet happened. One might argue, however, that help is closer at hand. There is a statistician's solution to the sample selection bias problem; and it holds a clue for what the present-day historian can do now.[42]

The statistical problem arises when the analyst tries to estimate relationships on a sample in which the observations do not represent a randomly selected subset of the underlying population but in fact systematically underrepresent—or miss out entirely—data points with certain specific traits. The classic economic example concerns the determinants of wages and working hours. Wages and working hours can only be observed for individuals who have looked for and found jobs. For the simple statistical procedures to work properly, the sample would need to report what wages and working hours the labor force nonparticipants or dropouts would have obtained had they participated.[43] The essence of the solution turns out to be to model (and estimate) the selection process and to incorporate this modeling and estimation into the estimation of the relationships of primary interest.[44]

42. On the statistical approach, see Heckman, "The Common Structure of Statistical Models of Truncation, Sample Selection, and Limited Dependent Variables and a Simple Estimator for Such Models," and "Sample Selection Bias as Specification Error."

43. In one business history example, the question concerns how firms respond to cyclical downturns. There is a set of observations before the downturn and another after. The selection problem is that some firms go out of business in the interim, and the survivors are not, in general, a random sample from the initial population. See Bresnahan and Raff, "Plant Shutdown Behavior during the Great Depression and the Structure of the American Motor Vehicle Industry."

44. Implementing this technique econometrically is not an entirely straightforward matter. The method only works if the selection equation is identified, i.e., that the selection into or out of the population depends on factors distinct enough from the main behavioral relationship that the two can be estimated, simultaneously but reliably distinctly, from the extant data. (For an overview of the Heckman procedure with a discussion of this problem, see, e.g., Little and Rubin, *Statistical Inference with Missing Data*, 225ff., especially 230.) In the classic statistical settings, this question comes down to the question of whether there are variables that belong in the one regression equation but not in the other. This is of course a highly abstract characterization, and, at that, a characterization of an inference procedure most historians would think informationally impoverished. In the settings with which the present analysis is concerned, the issue comes down to whether the causes of exit are distinct from large-scale changes in the overall competitive environment. These are not completely unrelated, of course (this is part of my point); but in my observation, they are generally not at all the same. The demise of individual firms is generally not inevitable. (Business schools even teach courses about how to manage through such potentially disastrous situations, i.e., avoid them when they are threatening but have not yet overwhelmed a firm.)

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This inference problem has the same basic structure as the historian's and its solution is helpful in thinking about how historians might proceed. Suppose—to make matters as simple as they could possibly be—that it is clear from general principles what the list would be of all elements (here coordination mechanisms) that might ever conceivably be judged important. Suppose, further, that it is clear from general principles what changes in the environment might favor one over another (or at least create niches within which one or another might survive). Then a forward-looking approach could be deployed. By construction, such an approach follows all the serious contenders from the start. Its focus is in effect on their survival conditions. And—getting back to the history-writing context—it makes no commitments whatever as to overall closure. Short of the discovery of new facts about old events, it cannot be wrong-footed by subsequent developments: the mere passage of time, with its attendant new events and history, is not a potential problem.

It is easiest to carry out this forward-looking approach to history writing when there is information about a genuine cross-section of the population at least at the beginning if not over time. But this is rarely obtained in business history. What can one do *faute-de-mieux*? If the objective is, as it was in the Lamoreaux, Raff, and Temin study, to study the history of systems, one idea might be to cull the accumulated history for a provisional universe of types and to study their careers through an analysis of the impact of changing conditions on specific examples. The idea would be to illuminate the conditions under which archetypal institutions are attractive enough to come into use, conditions under which they prosper or at a minimum do sufficiently well to be spared abandonment, and conditions under which they do die out. Rather than offering a master narrative of the economy, one would study the conditions of growth and decline of mechanisms. This would show the excessive simplicity of the markets-versus-hierarchies distinction and show off the persistent variety of institutional arrangements without making anything too simple.

There is a natural counterpart to such a history of organizational institutions for following the history of an industry or even a specific company. Here, the variety would typically come in terms of strategic choices of one sort or another—product categories, production technologies, employee or organizational skill sets, supply chain structure and attributes, and so forth—rather than (or rather than just) forms of organization. The present bias in company histories in particular seems quite striking, and it may in fact actually be easier to get a confident grasp on the opportunities and challenges of environmental conditions and on the variety of competitors and their action in

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such a narrower domain.[45] In either type of case, institutions on the one hand and industries or individual firms on the other, nothing in what I am suggesting implies or otherwise builds in the view that history has stopped or will come to an end. An analysis of this forward-looking sort is, as framed, retrospective without being too sure that it is right. This is the proper procedure for an evolutionary world.[46]

This approach may seem on its face quite unfamiliar. Indeed, the immediate reaction of many to it does not get past the name. This reaction is that the entire approach is a species of oxymoron, that history is inevitably backward-looking. I hope I have shown that this is simply confused, that at least in economic matters the historian can devote him- or herself to knowing some particular time and set of circumstances, and what it might have been to live and to act in them, without necessarily having his or her perspective entirely distorted by the time and circumstances in which the historian him- or herself works and lives. The detail has the attractive additional feature, viewed from an economist's perspective, that it puts choice, and above all actors and their actions, at the center of the analysis.

The other common resistant reaction is that as a history of institutions, this is history all right but the history of categories rather than of actually existing historical actors. This objection too is easily dismissed. At an intermediate level, this is no more than an objection to the idea of mobilizing abstract argument, whether well founded or otherwise, to understand the logic of what happens to concrete individuals. It is, in effect, an objection to the idea of theory being even potentially useful. One does not have to go as far as the Strategy Research Initiative does to feel this is clearly not plausible and, implausible or otherwise, no way to conduct research.[47]

A third (and much more sympathetic) type of resistant reaction says that this makes sense on its own terms but does not consider sufficiently seriously the fact that firms (and large organizations in general) often act without consciously making decisions. This seems to me a sound characterization of organizational behavior but not a fundamental objection to the approach I am proposing.[48] At any juncture at which strategic

45. There is no space within the constraints of this essay to illustrate. I explored some examples in the second and third lectures of a series (of which a version of this essay was the first) given at the London School of Economics in May 2012 and will publish versions of them in due course.

46. There is an interesting, if very sad, counterpart to this approach in the literature on Soviet history, and in particular, on the history of Soviet economic policy, in the interwar period. See, e.g., Tucker, "Stalinism as a Revolution from Above."

47. See, e.g., http://strategyresearchinitiative.wikispaces.com/High+Quality+Theory.

48. Neuropsychological evidence suggests that it is a better characterization of individual decision making than economists and decision theorists might like to acknowledge. For a general audience article giving references to technical literature, see Smith, "Neuroscience vs. Philosophy."

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choices are possible, inaction (the continuing of established routines and investment programs already under way, the shunning of possible initiatives previously passed over and newly arising alike, and so on) is always another possibility.[49] Even self-conscious decision making may be heavily subject to long-term drift in routines, and even conscious deck-stacking processes, on the part of involved individuals or interests.

What I am calling choices here may ring deeply to some of the unitary rational actors and expected utility-maximizing decision theory. The approach I am proposing is in fact much more expansive than that, leaving plenty of scope for exploring the mechanics of inertia, drift, and constraint (and their underlying sources in organizational routines and cognition, institutional structure, the entrenchment of interests, and failures of leadership and mobilization) as well as the historical development of affirmative actions.

It may be helpful to expand a little on the psychology and general orientation I personally see in the background here. Just as it is, as a matter of empirical fact, a mistake to imagine organizations as continuously formulating strategies, I think it is a mistake to imagine individuals, even in moments of organizational crisis, continuously thinking in the ways imagined by neoclassical economics, and, behind it, formal decision theory.[50] That perspective strikes me as essentially normative, and one can see its roots at least as far back as Simon's classic *Administrative Behavior*, in which he writes, "'decision-making is the heart of administration, and that the vocabulary of administrative theory must be grounded in the logic and psychology of choice."[51] Descriptively speaking, I disagree with the first claim in this; and regarding the second, I think the model in the background in important respects too simple. I have come to take my cue from Dewey's *Human Nature and Conduct* instead.[52] Not only is cognition in the wild not like the decision-theoretic account of neoclassical economics, as we have learned from Kahneman and Tversky and other researchers working in that tradition, action in the wild is not just cognition.[53] It has interacting elements of—to use Dewey's

49. Thus, Alan Taylor in another context famously wrote "German history reached its turning point and failed to turn. This was the fateful essence of 1848." Taylor, *The Course of German History*.

50. For a lucid overview of the basics of the latter, see the still-valuable Luce and Raiffa, *Games and Decisions*, §2.2–2.6.

51. Simon, *Administrative Behavior*.

52. Dewey, *Human Nature and Conduct*. For an introduction to this approach, see Cohen, "Reading Dewey: Reflections on the Study of Routine."

53. For the initial and still-valuable overview of the empirical inadequacies of the classical decision theory, see Tversky and Kanheman, "Judgment Under Uncertainty." For a thought-provoking study of cognition outside of the laboratory, which is thought provoking in this article's context, see Hutchins, *Cognition in the Wild*.

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terms—habit, impulse, and intelligence, where by intelligence Dewey meant something like cognition in the now more familiar language, by impulse emotion, and by habit something far more extensive than mechanical repetition (which he thought of as "dead habit"), instead encompassing the entire repertoire of flexible dispositions to action, with flexibility a matter of adapting to context.[54] In Dewey's view, these human faculties rarely operate in isolation. In particular, he thought of habit as shaping the workings of intelligence in quite profound ways.[55] Action is thus deeply historicized, and part of the project here, I would say, is to uncover that history.[56]

So far, then, so good. The more persuaded may then want to know what would actually happen next if a historian were to implement this program to study the history of an industry or an individual firm. The answer to this may give more comfort still. There are several more steps, and the next one is in fact much more familiar, at least to some. (It is the final step, to which I will come in a few moments, that reintroduces the sense that the approach I am proposing is really something radical.)

Having established a perspective, the historian must next examine a specific present. How are the firms in question in a position to make the choices that they do? Given the choices they actually make, what are the selection pressures bearing down on them? Analysis of the opportunities and of the selection mechanisms is, at least on the surface, traditional microeconomic analysis shorn of the equilibrium assumption, mobilized only to ask questions concerning the landscape of competition, what business there is to be had and who gets it, and what the consequences are once the flow of cash coming in confronts the firms' expenses. Yet this does not sound like traditional business history, whatever direction it is facing. Addressing these questions implicates, I will argue, far closer attention to resources (real and financial), routines and capabilities, and actual activities than is commonly offered in either business history or the usual analyses of economists. The evolution-oriented historian then wants to capture the history of these more foundational elements as time moves forward.

Which firms get the business depends on two sets of considerations. The first concerns what different firms can offer to customers who might come to place the firms in their consideration sets and the costs they face in doing this. Both the offerings and the related costs are in a deep way consequences of activities the firms carry out and

54. For "dead habit," see Dewey, *Human Nature and Conduct*, 51, see also 32.
55. Ibid; 121.
56. *A propos* Lamoreaux, "Reframing the Past," with most of which I, perhaps unsurprisingly, agree wholeheartedly, I would add that while framing is surely part of cognition, cognition is not, so to speak, so much as half of action.

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collaborate in, with, standing behind these, capabilities the firms (and perhaps their collaborating parties) have built up over time, including control and governance systems, and behind many of these to an important extent, resources the firms have invested in.

The text in parentheses concerning collaborating parties is sometimes much more than a speculative afterthought. In many industries, the proper unit for comparison is not the individual firm—say, a retail firm or a manufacturer—but rather a value chain of committed long-term collaborators.

The second set of considerations amounts to competition—what are the alternative sources of supply and what can each offer?[57] The range of alternative sources in itself depends on anticipated demand (roughly speaking, the size of the market), the structure of costs of production, and more direct barriers to entry. Both of the last two are also rooted in incumbent firms' capabilities and resources.

A characterization of the cost side (the presence in available technologies of, e.g., economies of scale or scope, the sunkness of fixed costs, etc.) and of the activities that generate whatever revenue does flow in must address the preceding considerations if it is to give an account of where amongst the relevant population of firms revenue flows and net profits will accumulate. Thus a history of industrial organization (at least from the horizontal perspective) and of firm outcomes in the short run is inevitably rooted in a history of the accumulation (and decline) of valuable resources inside firms and the capabilities and activities flowing from those resources.

A facet of history more directly accessible from accounting statements also matters. The net profits referred to earlier are the pool of wealth from which, in any period, investors are paid and cash retained inside the business. What might be done with this unreserved money if it stays in the business? It might be paid out, or reserved, for capital investments. But far more commonly than is often appreciated, it simply goes to working capital, funding the creation of inventories and even sales in the short term. (This is of course more the case in some industries than others. But it is very true of some interesting ones, in our times and in earlier ones.)

Both these forms of investment amount to setting aside financial resources to pay, in one way or another, for future operations. Insufficient flows to investors in public companies rarely bring the firm to a halt. Insufficient working capital can be another matter entirely: a firm that cannot produce and cannot get its output into the channels of distribution ultimately cannot sell and be paid. Firms

57. Stuart (see "Value Creation, Competition, and Business Strategy") develops an intriguing analysis of aspects of this.

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sometimes can, and sometimes do, borrow against the promise of future revenue streams when internally generated working capital is insufficient. Such borrowings provide external funding for whichever purpose, but at the cost of burdening cash flows in the future. (Sometimes the fiscal stress is transitory or the investment in future trade self-sustaining or even more. Sometimes, of course, the expectations are wistful and the borrowing disastrous.)

Firms proceed forward in time in two ways in this conceptualization. The first is basically a matter of routine. Profits in any given period are at least sufficient to finance operations in the next. Depreciation of equipment, turnover in employees and so change in the stock of staff skills and network capabilities, and shifts in demand are not so large as to impede continuing activities and continuing minimal fiscal balance. Sometimes, the future really is like the past: the firm "knows"—through its routines—what to do and that is good enough.

But change is common at the micro level, even in conditions without major environmental change. So this sort of account, even when of a third-person variety, needs to encompass both development and innovation. Both of these manifest themselves essentially in activities (even when the change involves something as concrete as an only superficially new product). Implementing new activities and new activity patterns involves sunk costs and coordination difficulties. In retrospect, one would say that these have a history. The history of these costs and of their sinking can generally be traced out and the foundations in the deeper layers of capabilities and resources explored.

Thus, the short-run analysis focuses on how the various relevant firms or types of firm differ, what the various (types of) firms can do, and what financial resources are generated when they act. In the medium term, the question is how the portion of those financial resources that stays inside the firm plus whatever else comes in are mobilized to change the firm's action possibilities over time.

Is that enough? Can we just start with epochs (of relatively stable operating environments or of moments of some entrepreneur's having gotten started), identify the demand and cost conditions facing each generic type of actor or focal specific actor, reckon in reactions or otherwise determine actions under the circumstances, and then set the machine and its advance equipment into motion? There are two problems with any such approach to closure. One is that it assumes equilibrium (and, more precisely, actions and action contexts that might lead to equilibrium) or at any rate some particular behavioral rule.[58] The other is that it leaves out agency. The two are not unrelated.

58. This is, of course, the point of takeoff for Nelson and Winter.

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I want to approach the question of what might still be missing from a direction that may at first seem a bit oblique. I will end with some practical details of how to do history. But I have to begin with some analysis of what historical explanation requires in order to get there.

History may be put to any of a variety of purposes. But most significant historical writing, interpretive as it may be, involves explanation. A comprehensive chronicle would not involve any explanatory claims; but neither would it seem to shed any light on the significance of individual events. It would be mere description. The significance of individual episodes (however construed) in their time, never mind over time, lies in an account, explicit or implicit, of why they mattered.

It is, broadly speaking, impossible to describe why an event mattered without implicating an account of how it mattered. There are at least superficially highly various ways of approaching this "why" question. But the question is more or less unavoidable in any explanatory activity, and all of the different approaches can ultimately be construed to involve causal claims. (This is why Hume referred to causation, in a memorable phrase, as "the cement of the universe.")[59]

One event might be said to cause another in the sense we casually associate with the physical sciences. Hempel wanted to interpret this as an instance of the working of some covering law.[60] The high social science approach of the Strategy Research Initiative referred to in Footnote 47 is in this spirit. But it has come to seem naïve. Quine, in particular, has argued that it is very difficult to get at the truth of theoretical propositions such as these covering laws by testing implications piecemeal.[61] It is generally possible, he claimed, to protect central theoretical claims against adverse evidence by introducing sufficient subsidiary hypotheses.[62] Quine's claim is spectacularly true

59. Hume, *An Abstract of A Book Lately Published entitled, A Treatise of Human Nature* (published anonymously in 1740, reprinted more or less in facsimile by the Cambridge University Press with an Introduction by J. M. Keynes and P. Saffa, 1938), 32.

60. See in particular Hempel and Oppenheim, "Studies in the Logic of Explanation."

61. Quine, "Two Dogmas of Empiricism." This essay seems, philosophically speaking, to be the jumping-off point of postmodernism. (The central claim is sometimes referred to as the Duhem–Quine thesis, referring also to Pierre Duhem, a turn-of-the-century French physicist, mathematician, and philosopher of science. But Duhem's version, presented in his 1914 *La Théorie Physique, Son Objet et sa Structure* is much more modest than Quine's. On Duhem, see most conveniently, Duhem, "Physical Theory and Experiment." On the overstatement, see inter alia Harding's "Introduction," XII).

62. Quine "Two Dogmas": 40.

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of covering law explanations that might explain many traditional historical subjects.[63]

One potentially different sort of reason historical explanation seemed to fit awkwardly within the covering law model approach is that the intentions of actors often seem central to the outcome that come about. But intentional explanation fails to be causal only to the extent that mental events cannot be causes; and the philosophical consensus now seems to be that it is a sort of category mistake to imagine that mental events cannot be so regarded.[64]

Functional explanation has seemed a third and very different approach. The basic idea in functionalist explanation is that institutions and other observable states of affairs are to be explained by how they relate to their environment, i.e., to one another.[65] Yet functional explanation seems in this setting to be an assertion about the equilibrium states of systems and to neglect entirely the question of how the systems might proceed from one equilibrium to another: outcomes are explained not by their antecedents but rather by their concomitants. It is, in this deeper sense, not a species of historical explanation at all. Teleological explanation, when one thinks about it, only makes this even more explicit.

The whole of the preceding discussion presumes that explanation and empiricism in general are as a general matter possible. This view is in some tension with the postmodern (sometimes postpositivist) literature that flowered in the closing decades of the twentieth century. That literature sought to undermine confidence in the robustness, and so ultimately the very possibility of credibility, of empirical research of any sort.[66] This literature seems in retrospect to derive from some sort of the postmodernist argument like that of Quine. Quine's argument itself now reads as hyperbolic and overstated.[67] He himself drew back.[68] The postmodernists have something of a point

63. Though hardly all: see, e.g., McCloskey, "Does the Past Have Useful Economics."

64. This is the Anomalous Monism of Donald Davidson. The locus classicus is Davidson, "Actions, Reasons, and Causes." (I suppose this to some extent rehabilitates the view on historical method that R. G. Collingwood put forward in his posthumous book *The Idea of History* (Oxford, UK: Clarendon Press, 1946) with which my proposals hereafter have some common ground.)

65. The most famous settings for this sort of explanation in some circles are in sociology and social anthropology, where institutions are sometimes explained in terms of their ability to support social stability and in the political–economic writings of Marx.

66. "[P]ostmodernism rejects all empirical work as illusory and deceptively scientisic." Bruno Latour (trans. Catherine Porter), *We Have Never Been Modern*.

67. For a very detailed critique, see, e.g., Zammitto, *A Nice Derangement of Epistemes*.

68. Quine, "Two Dogmas in Retrospect." See also Quine and Ullian, *The Web of Belief*.

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and we should, as always, be circumspect in our judgment of evidence. But we should not feel paralyzed and unable to act.

So we can stick with analyzing causal explanation. I return to my main theme. For a potential actor to make up his or her mind concerning what to do—to decide in the sense I identified earlier—is for that individual to consider a variety of actions with (arguable) causal significance and to choose from among them one course to follow. In other words, all decisions of the sort I have in mind rest on notions in the mind of someone of the form "If we do X (in the environment I imagine), something as Y will happen." To understand, and to explain, the conscious and intentional component of action is to understand the comparison between, and the choice among, these putative causal chains and so to involve the analysis of each of them.[69]

All causal claims implicitly invoke counterfactual conditional propositions. An event (or, more granularly, an action or a decision) matters because (i.e., when) other, perhaps subsequent, outcomes or patterns would have been different had the event in question not occurred. (If this were not so, we would be discussing distinctions without differences.) This assertion may seem obvious; but note that it immediately draws the analyst onto terrain in principle much more uncertain than that of the naïve chronicler. If explanation involves the more or less explicit contemplation of counterfactuals, then explanation of decisions involves probing, again at least implicitly, what an embedded actor might reasonably have expected to happen if something salient about the world had been other than as it was.

A piece of machinery is helpful at this point, that of a possible world. The basic ideas of possible worlds analysis have been traced back as far as Leibnitz, but the modern presentation owes to works of David Lewis and Saul Kripke (who worked out the formal semantics as a matter of technical logic).[70] The idea of a possible world may sound dreamy (particularly when the burly and heavily bearded Lewis insisted on ontological realism in his exposition) but it seems straightforward as an interpretation of modal logic. The assertion that a proposition is necessarily true is there interpreted as the claim that the assertion holds true in all possible worlds. The assertion that a proposition is contingent is interpreted as the claim that there exist possible worlds in which the assertion holds true and others in which

69. It is of course the case that some decisions are about specific issues and some are about the allocation of decision-making authority to individuals (perhaps individuals with robust proclivities).

70. Kripke's studies are very technical. Readers curious for their flavor would be better advised to begin with Hughes and Creswell, *An Introduction to Modal Logic*. (This is technical in itself but is certainly more accessible.) David K. Lewis, *Counterfactuals* (1973) is more accessible in style, though perhaps also quirkier.

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it does not. The assertion that a proposition is necessarily false or impossible is interpreted as the claim that there are no such possible worlds. Contingent truth, contingent falsehood, and mere possibility are easily defined. The problem of evaluating the plausibility of counterfactual conditional claims is in essence the same as the problem of evaluating the plausibility of possible worlds.

The plausibility of possible worlds in an economic setting involves a species of proximity. What we require for present purposes is a sort of topology of alternatives, a means of distinguishing what is near from what is far. In an economic setting, some features change only slowly, e.g., the financial resources available to a company, the physical capital in place, and the routines and capabilities of the organization as these exist at any instant in time. But many are more plastic. The commercial histories of Amazon.com and Google can be seen as histories of exploring the uses to which certain basic infrastructures can be put; and the early history of Toyota is in a different way a matter of exploring the plasticity of ways of producing a relatively constant product. There are resources that represent constraints, at least in the short run; but there is scope for imagination and reimagination too.

In a setting specifically about economic decision making, then, we can go rather farther than the constraints of the ex ante. Proximity, in this setting, turns on intent, knowledge, and an embedded sense of feasibility. The question is not what has been done but rather what the potential decision makers think might be done. This is a particularly salient matter in times of crisis; and that thought brings in its train the reflection that contrary to the way business school professors generally teach competitive strategy (and the Strategy Research Initiative authors seem to conceive the empirical subject), many firms do not as organizations think deeply, or even often, about strategy—rather, questions about structure, power, and leadership entirely aside, tactical questions preoccupy the decision makers whose opinions might even conceivably matter almost all of the time and essentially strategic thoughts only intrude when some long-established course of action threatens to become no longer viable.[71]

If this last assertion is correct, an account of why a firm moved forward as it did—the natural complement to the forward-looking perspective—must be rooted in what the potentially influential actors know, can imagine, and think possible in the environment they inhabit at moments when conscious decisions must be made. Cognizance of

71. My language earlier may have suggested an account of novelty much more focused on discovery than invention. I hope the text here makes clear that I mean to leave room for what looks, from the perspective of actors, like the latter and for interactions between environmental feedback and such initiatives to create elements of path dependency.

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economic closure conditions must be part of their evaluation process, since firms cannot, e.g., continue after they run out of money. But fine game-theoretic inferences concerning the responses of others to their initiatives may be the very least of what is involved. Establishing a good explanation of events taking place over time turns on following the evolution of intent, the development of knowledge, and the extent of what is possible. Demand is obscure and organizations may sometimes seem almost as obscure. The main tool with which to understand how these progress is neither a static structure of costs nor a characterization of momentary demand. Still less is it a notional reaction function. It is a reconstruction of the lived experience and understandings, and predispositions to action, of an organization and, to some extent, of the individuals whose actions comprise it.

The reaction of high social scientists to such a proposal may be not so much dissent as a sense that this is all very well but it is not knowledge and it certainly is not science. To the second of these, I am inclined to respond that this is indeed what the early stages of proper science look like when viewed up close. The mechanical application of classical statistical methods to situations not really well suited to them and to questions not really the ones a neutral seeker after truth would ask is hardly a good standard of comparison.[72] What I am proposing is not an attempt to reduce outcomes to thought or somehow to drive history away into an untestable mode of analysis unaddressable by critical evidence. It is not even committed to, or even rooted in, a notion of the radical idiosyncrasy of specific cases. To return to the remark attributed to Mark Twain about history not repeating itself but nonetheless often rhyming, I think that many aspects of firm operations, not least among these important aspects of cost structures, represent, at least in the moment, hard constraints. I believe that many of these aspects can fruitfully be grouped into types and subtypes. I therefore do not believe that this approach is somehow hostile to generalization.[73] It does, however—to use the Tukeyean language—have more about it of the exploratory than the confirmatory.[74] I think there is something worth exploring.

The approach I am proposing is an attempt to reconstruct choice opportunities in order to trace out how choices actually emerged. The real question is whether such broader reconstruction is knowledge, or

72. One prominent commentator put the point quite epigrammatically. See Leamer, "Let's Take the Con Out of Econometrics."

73. Indeed, as generalization proceeds, it is easy to envisage expositions being organized by types of episodes rather than the firm or industry subjects of the underlying narrative. I am proposing means of learning from history, not just a program for propagating it.

74. For Tukey, see Footnote 8.

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at least a firm step on the way toward it.[75] I have argued in the preceding section that the answer to this is a vigorous "Yes." The question of whether it goes far enough, I am happy to leave to concrete cases.

Where does all this leave us? "In a report for the publisher," the historian Keith Thomas wrote recently, "an anonymous reader of the manuscript" of a recent book of his commented as follows[76]:

> . . . . There is always a line of argument, but it tends to be both contained and artfully concealed in a great many references to and citations of a generous selection of . . . [contemporary] texts and documents, which account for a high percentage of the text [to hand]. According to strict and even censorious critical criteria, these materials cannot stand as proof of any argument, since the reader is in the hands of the author and of what he has chosen to serve up as, strictly speaking, illustrations of his own contentions, it being, in principle, always possible to build up a different picture with the aid of different examples. The last thing one will find in this kind of social-cultural history is the allegedly knock-down evidence of statistics, but the wholly justified implication is that these matters are best understood with the aid of what German social scientists and theorists call the faculty of *verstehen*.
>
> That, I think [Thomas writes], is a very kindly account of what I try to do: to immerse myself in the past until I know it well enough for my judgment of what is or is not representative to seem acceptable without undue epistemological debate. Historians are like reliable local guides. Ideally, they will know the terrain like the backs of their hands. They recognise all the inhabitants and have a sharp eye for strangers and impostors. They may not have much sense of world geography and probably can't even draw a map. But if you want to know how to get somewhere, they are the ones to take you.

Keith Thomas's history is a sort of historical anthropology. What of time in itself? When J. L. Austin took the stage at Sanders Theater to give the William James Lectures in 1955, subsequently published as *How to Do Things with Words*, he was quarreling with the logical positivists.[77] The purpose of his lectures was to explore the idea that the understanding of words in use could not be reduced to the meanings of the words abstracted. Sometimes, speaking was acting: sometimes, one could do things with words. I have been arguing that one can do things with time as well.

75. Quine does not, for all he has written, disbelieve there are firm steps toward knowledge. See Quine and Ullian, *The Web of Belief* (especially Chapter VIII).

76. Thomas, "Working Methods."

77. Austin, *How to Do Things with Words*.

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I do not mean to assert an exact parallel. My overall title is, indeed, a sort of pun. Managers, confronting a crisis, like Harold Lloyd dangling from the clock hand in the famous silent film image, seize the opportunity of time to fashion a response. Many will remember the image from a poster or a book illustration—it is one of the handful of the most famous stills of the silent film era. Some who have actually seen the film may also remember what the clock looked like afterward. Mr. Lloyd's character doubtless thanked his lucky stars he had gotten into trouble at 2:45 rather than at 6:30; and although he never made it back to the (merely apparent) safety of the window sill, he was still able, with enterprise and persistence, to improvise a course of action that ended with him comfortably out of danger (and, indeed, having gotten the girl). There are often opportunities if one can only find them.

Managers, then, can do something with time. But so can historians. I want to push business historians in the direction of Keith Thomas's local guides. I also want to encourage them to explore what happens as time plays out, as opposed to exploring what happens in moments in which the passage of time does not matter.[78] It is there, out of equilibrium—in the imagination and fashioning of a future—that one finds the beating heart of the sort of evolutionary history I am advocating.

One might focus on what it is that firms actually know. In a sense, I focus here on how firms learn, a more consistently dynamic question. Let me close this essay by being very concrete about the title. How do I recommend that historians do things with time? First, by looking at historical events from a different perspective, as sequences of challenges to be addressed rather than as initiatives that have already happened; second, by attending in a fashion at once microeconomic and dynamic to how what firms—individually and, as seems appropriate, in groups—can do changes; and third, by exploring actors' choices deeply through illuminating the alternatives the historically situated actors faced and show how the actors made sense of them. These represent a coherent program. It is not a very conventional one as history goes, but that is not an obvious virtue in itself, and the sort of approach I am recommending fits in very comfortably with modern thinking about how firms and competition actually work, the actual historical subjects in which I am interested. It takes, of course, concrete historical analysis to show that the virtues of this program are more than abstract. I hope I show in work expounded elsewhere that they can indeed shed some light.

78. To quote Heidegger (of all people), "Offenbart sich die *Zeit* selbst als Horizont des *Seins*?" *Sein und Zeit*.

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