Business History Among the Social Sciences

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For presentation at the European Business History Association meetings in Ancona on September 8, 2018. These thoughts have been brewing for some time. This text represents a sort of preliminary marshalling, sketchy on details and incomplete in coverage and development, of a much longer manuscript in progress. My thinking about these and related matters owes a great deal to longstanding (and in most cases still ongoing) conversations with my present or ex-colleagues Randall Collins, Steven Hahn, Louis Galambos, Michael Gilsenan, Naomi Lamoreaux, Jonathan Steinberg, Philip Tetlock, and Sidney Winter, my old teacher and friend Robert Solow, and my friend and co-editor (of *The Emergence of Routines: Entrepreneurship, Organizations, and Business History* qv infra, now at last out from Oxford) Philip Scranton. A dutiful institutional survey question most of thirty years ago from my then-colleague Richard Hackman, whose wry sense of humor may have provoked the unexpected and, to me, quite stimulating way I took it also seems in retrospect to have played a role. His death half a decade ago, which certainly seemed premature, was a loss to many beyond his field as well as to those in it. Discussions extending over a period of years now of related issues in the Wharton Seminar on the Evolution of Organizations and Industries have been very helpful. I owe thanks to the Wharton School Dean's Office and to School's Mack Institute for research support. The usual disclaimer applies.

Readers of this text will detect some sparsity in the footnoting. I will post a better referenced version later in August (when we have returned from the beach).

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0. Introduction

It has become clear in recent years that there is interest among business historians—graduate students, junior faculty, and even on occasion some occupants of established posts—not just in business school jobs but in figuring out how to present their work in ways that will register with business school colleagues. There has been a good deal of discussion of how to do this at BHC meetings and conferences in Europe of other business history organizations and also in the recent publication of a collective work entitled *Organizations in Time*, a book that is a sort of introduction to how business history matter might be cast in the High Social Science form that dominates the (largely empirical) field known as organization theory and more generally characterizes research and assessment most prominent business schools.[1] The lead article in a 2015 Special Issue of *Business History* is another recent essay on a related theme.[2] At the same time, readers of *Science* ("Estimating the Reproducibility of Psychological Science," 28 August 2015 [349(6251)]) or even the *New York Times* issues of August 27, 28, and September 8, 2015, will be aware without any further exploration of a growing literature that the mansion of High Social Science rests on shaky foundations.[3] I think business history has a place—indeed, a potentially highly productive place—amongst the social sciences but not at all one of the sort either *OIT* or the *Business History* authors seem to have in mind. Even brief examination of what scientists actually do and modest exploration of what this suggests about what science actually is clarifies the work setting of business school faculty members, the basic project of the social sciences, the weaknesses and inadequacies of the academic management version of social science, and how business history could contribute to the social scientific study of organizations in a non-Procrustean and strength-respecting way. I want today to sketch what such an examination and suggestions might look like, to lay out how such a way of presenting business history material might proceed, and to explain how it might be situated amongst the concerns about inference and knowledge social sciences faculties consider when reviewing research in their own more familiar domains. It seems to me that some specific domains of inquiry are ripe for this sort of treatment and I identify them. I conclude with a proposition.

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[1] Marcelo Brucheli and R. Daniel Wadhwani, *Organizations in Time* (Oxford and New York: Oxford University Press, 2014).

[2] Abe de Jong, David Michael Higgins, and Hugo van Driel, "Towards a New Business History?" *Business History* 57(1) (March, 2015): 5-29.

[3] Open Science Collaboration, "Estimating the Reproducibility of Psychological Science," *Science* 349(6251) (28 August 2015); Benedict Carey, "Many Psychology Findings Not As Strong as Claimed," *New York Times* August 27, 2015, and "Psychologists Welcome Analysis Casting Doubt On Their Work," *ibid.*, August 28, 2015, Lisa Feldman Barrett, "Psychology Is Not In Crisis," *ibid*., September 1, 2015, and Letters: How Reliable Are Psychology Studies?" *ibid.* September 8, 2015. (Subsequent references could well be added, here or below when the topic comes up again.)

1. What science is actually like

Talk long enough to management academics in the High Social Science mode and you begin to forget whatever history of science you may once have known and whatever knowledge of the actual practice of science in our times you may ever have accumulated. The picture of the enterprise of science such people will imply, or perhaps even say, to you is rather different. Science, they will suggest, is centrally as well as fundamentally a theoretical activity. Indeed, theorizing is so much the central scientific activity that the whole activity might just as well be identified with theorizing: theorizing is the *summum bonum* of the thing, not so much the objective as the heart of the practice. The history of enacted science may seem to have a good deal of measurement activity in it, but that is because (the relatively knowledgeable ones will say) measurement is difficult. Sampling is not a question, never mind a problem. Science is about giving law-like explanations for empirical regularities. Laws can have what a mathematician would call boundary conditions and what anyone would call *ceteris paribus* constraints. But given boundary conditions and *ceteris paribus*, the laws of science are of general application.

It usually seems, if you probe a little, that these people have a particular science in mind and that that science is physics. I think it is a very open question whether this account is an at all good description of the development of physics before sometime in the nineteenth century. I am not even sure if it is a good description of physics since then. The quantum physics-trained philosopher of science Nancy Cartwright, for example, points out that physicists distinguish between fundamental laws and phenomenological laws, drawing a distinction between laws which do and do not derive from first principles (as opposed to characterizing parsimoniously observable patterns of behavior of physical objects). She goes on to assert that most laws in physics are phenomenological and not fundamental.[4]

People physicists recognize as their predecessors (even back to the Pre-Socratics) surely were seeking explanations of observable physical phenomena; and sometime after an extended period (of centuries) of reflection on empirical evidence and the development of useful (in particular, mathematical) formal tools, physics in the nineteenth century began to develop a recognizably theoretical wing. The question is whether this wing is so core to the enterprise as to be constitutive of it. Recognizably modern physics did not come into being, after all, because the explanations suddenly got better. It came into being as an alternative to a completely different method of explanation. The theologians against whom Galileo and others struggled were interested in explaining and in what were, in their minds, theories just as much as the Pre-Socratics were. What distinguishes both of them from what we know now isn't theorizing. What is recognizably modern about recognizably modern physics is systematic: it is the confrontation between theorizing and potentially decisive empirical evidence. Galileo wrote to Kepler complaining that his critics, even astronomers among them, declined to examine evidence through telescopes. The emblematic moment lives in legend at least with Galileo, after a session with the Inquisition, saying under his breath, descending the steps of the building, "*Eppur si muove*" ("And yet it does move"). Certainly such a story captures something essential about what physicists do.

[4] Nancy Cartwright, *How the Laws of Physics Lie (*Oxford: Oxford University Press, 1983).

The other, perhaps less obvious but equally important, element is an assumption about the subject matter under investigation. The assumption is that the phenomenon, at least when isolated of *ceteris paribus* concerns and examined on a suitably bounded domain, is indeed invariant. The phenomena of science are phenomena of which there can be general laws. It is possible, at least in principle, to give for them a list of initial conditions, and, given those laws, to say what will happen. Simple causal explanation is possible.

I should make one further remark in this section. In the realm of fundamental theories, the transit from theory to hypothesis and decisive test can be a simple and direct one, so it makes sense to think of science as an activity as comprising only theory and experimentation. In the realm of phenomenological theories—apparently a vastly larger one—matters are much more complex. It is a commonplace of the social science seminar room for someone to remark "Let's see what the data say." But data rarely speak by themselves, unassisted and without intermediation. I think that in the domain of phenomenological theories, theory generation is in itself a problem. The world science studies is a world without intentions and agency. These are, so to speak, underdetermined.[5] In the wider world modeling which includes modeling action with these attributes, we are definitely in phenomenological theory land. I suppose a simpler way of putting this point is to say that in those realms, theory doesn't do itself. It requires an external infusion of ideas.

2. The social sciences as a situated discourse (with drift, as it were)

This next objective of this paper is to characterize the methodological practice of academic management research. But it is important to begin with some institutional context. The context I have in mind is the teaching that pays the bills.

Business school students are not like arts and sciences students and this is even more the case at the post-graduate level. This is not merely a matter of the opportunity cost of the time of their degree course. Business school students, particularly at the postgraduate level, are in my experience very practical people, not simply repurposed intellectuals. They are very often interested in careers that will make them a good deal of money, interested in retiring at a relatively young age rather than finding work they will want to do until they are no longer capable, and so forth; but the more striking trait is that they seem in general to be people who want for their work being paid to make decisions. They are, as a general matter, a good fit for jobs in which everyone cannot get on with their work until the person at the top makes up their mind. Is all the information one would like to have in hand before making a decision available? No? These are people who would be comfortable making a decision anyway. They are satisficers, people of "good enough" decisions with more taste for getting on to the next challenge than for getting this one absolutely correct *sub specie aeternitatis*. What they want out of a business school education, screening that is valuable in their own job-hunting in their student years and going forward aside, is help—ideas, training—in functioning in such settings and, ultimately, in making decisions in such a way.

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[5] Thomas Nagel, "What Is It Like To Be A Bat?" *Philosophical Review* 83(4) (October 1974): 435-450.

When I started work at the Harvard Business School in the late mid-1980s (as a case-writer in 1986 and as an assistant professor in 1987), the School was still known in some quarters (and not unflatteringly) as the West Point (the Sandhurst or St. Cyr) of Capitalism. As at West Point, there were many traditions, change in which seemed to happen rarely if ever—the rooms, the style of the teaching materials of the teaching itself, and so forth. There did, however, seem to be a transition under way in what the French would call the *formation professionelle* (but which I mean here specifically as the training of professionals, vocational training only in the most literal sense of the phrase) of the faculty. Since the student days of the then Dean, the University had required doctoral degrees of assistant professor appointees. But faculty members a decade and more older than me seemed mostly to have Doctor of Business Administration degrees, the School's own doctorates. Indeed, these DBA's seemed to be something that happened to them after their MBA, typically an MBA's from the School itself. These people were a subset of the School's best students, a subset of individuals who felt an impulse towards the academic life (at least HBS-style, a version that involved a lot of executive education at the tenured levels and, along with that, a lot of consulting, the occasional directorship, and in any case a generally very comfortable. life.) DBA dissertation research might involve faintly ethnographic seriously embedded observation. But writing case studies deriving from consulting experiences or conversations with old students passed internally for research from members of the faculty. Articles in the *Harvard Business Review* advertised new ideas that might serve as consulting vehicles. Books wrote up development of those ideas with supportively themed collections of case studies. or were collections of case studies deriving from course development activities. The Harvard Business School Press published the cases (its revenues provided about one-third of the School's operating budget) and the idea books. Irwin generally published the case textbooks—perhaps they had already sunk the required costs in marketing infrastructure. For present purposes, the point of this was that peer review and some notion of scientific progress was no part of what I am describing. It was more like a professional services firm than the Faculty of Arts and Sciences. The culture of the place was a very thick one; the entire tenured faculty voted as a Committee of the Whole in personnel matters; and the junior faculty certainly had the impression that the Old Guard ran the show. You could do the work you wanted and enjoy, as much as possible, the ride; but if you wanted to fit in, you had to do as they did.

The educational demographics of the faculty intake were changing, however. Consulting firms had long since hired very large (30 percent plus) percentages of the graduating MBA classes but the 1980s were the so-called "Deal Decade" and Harvard MBA's were streaming towards Wall Street in record numbers. The salaries and career prospects on offer were greatly in excess of what the Business School could offer and the School increasingly turned for junior faculty recruits to what it cheerfully if informally referred to as "the disciplines". These were typically social sciences of one sort or another—economics, political science, sociology of an organizational bent, social psychology, etc. The School got individuals who were interested in some aspect or another of organizational life. But the thick culture also created a problem. These individuals knew that even if they "went native", they were far from assured of getting tenure. Unless they were fairly confident that if their bid failed they could move to a client company or consulting firm (one of which was what the HBS MBA-trained disappointed assistant professors did), they felt they had to keep their hand in the professional discourse to which they had been trained. So whatever else they did, they pursued projects and wrote papers aimed at social science journal referees. If they were to be promoted and they weren't easily able to walk the walk and talk the talk, so to speak, they had to be colorable as experts of some sort. (Discipline-based letter-writers, pleased at the thought of placing one of their own in such a position, apparently tended to write enthusiastic letters.) If they were to go back to their home discipline and be on the market as anything other than a long-in-the-tooth rookie, they needed publications. Either way, if they were to hedge their bets, they had to publish in disciplinary journals.

Business school enrollments and faculty lines seemed to grow vigorously in the 1980s and the decades afterwards in both the US and abroad. They did so predominantly at university-based institutions without the large graduating classes of the Harvard Business School to provide them with intellectually first-rate graduates to recruit into teaching and without the thick HBS culture (and donor base) to anchor them anywhere other than in the world of the social science Ph.D. assistant professors they found it easiest to recruit. That the university-level tenure review committees were generally heavy with other social scientists did not interfere with this shift. Average practice business school academic research came, increasingly, to resemble social science research. (Indeed, the demand for publications, coupled with the consolidation of journal publishing—predominantly from the hands of learned societies into those of commercial or commercially-minded enterprises eyeing an increasingly institutional subscriber base and thinking cheerfully about all-or-nothing offers or their pricing counterparts—led to a proliferation of outlets.) The literature grew accordingly.

What was the evidence behind the literature? In the old-style HBS research, doctoral-level research and beyond tended towards deeply embedded observation of firms and (to a much more modest extent) industries. These researchers had some incentives to produce milestones of major projects and not just a steady stream of one-off cases. But the new-style assistant professors faced somewhat different and altogether more powerful incentives, evaluation criteria that determined whether they could keep their jobs. They needed evidence that any examples they worked up could be situated statistically; and they needed above all a body of work impersonal discipline-based peer reviewers would say established an identity and real authority within the discipline's own discourse. This more or less directly created incentives against investing time and lapidary energy in creating novel and robust data assets. The new-style assistant professors instead drifted naturally towards either pre-existing datasets assembled by someone else (the Patent Office, the Department of Commerce's quinquennial Economic Censuses and higher frequency surveys, Security and Exchange Commission filings, the University of Chicago's Center for Research on Securities Prices (CRSP) price series, the Securities and Exchange Commission, Standard & Poor's, etc.), small response rate surveys of corporate officials, or "laboratory" experiments typically conducted not on working executives but on undergraduates or frequenters of the Mechanical Turk website. The first of these types of sources predominated in studies whose subjects were firms or industries. It will be worth remembering below that the measures derivable from these sources are all measures, in one way or another, of outcomes. Answers to questions that fundamentally engage matters of organizational process or individual cognition may be obscure in such evidence. The old Harvard-style research, for all its limitations, was very strong on this. Altogether, then, as the population of academics grew larger and more professionalized in the evaluation standards which were applied to them and so in the incentives they faced, research drifted towards what was easy to study, or at least to complete studies on. The interpretive stretches, so to speak, grew longer and longer.

3. Social science in academic management studies and some problems

We step back from these practical matters for a moment to return to a more elevated plane. What is the "social science" which management academics bring to the table (that is, to their evidence)? It is helpful to begin several steps back. Intellectual historians trace the origins of sociology, as social theory, back at least to Comte in the 1820s and sometimes to Montesquieu and other philosophically-minded figures of the preceding century. But sociology as the discipline of the study of social relations and structure rooted in one sense or another in social facts really seems to have its beginnings in the writings of a group of mainly German writers in the second half of the nineteenth century—Simmel, Tönnies, Durkheim, and Weber surely belong on the list, though there are others one might add—who were deeply engaged with less with the Enlightenment than with the felt meaning of life in the age of industrialization and the growth of large-scale bureaucratic organization. The characteristic works of these writers are interpretive essays considering qualitative evidence. Durkheim publishes his *Suicide* only in 1897.[6]

Twentieth-century sociology has hardly been without theorists.[7] But the general drift of practice in the subject seems to have drifted firmly not just towards the empirical but towards the quantitative. This seems to be true of the social sciences on a much broader scale. This has partly been a matter of the capture and rendering into forms usable by researchers of quantitative evidence; but it is in no small measure also due to the radically declining costs of computational capacity. The laptop on which I am typing this text, which I can hold not uncomfortably in one hand, has more computational capacity than the mainframe in the computer center of my first university in the mid-1970s. This has facilitated much more than such exercises (in not much more than counting) as textual analysis. The basic idea of regression analysis in particular had been known to mathematicians since the beginning of the nineteenth century, Legendre and Gauss having published papers about it in 1805 and 1809 respectively and Gauss having published a version of the key result now known as the Gauss-Markov Theorem in 1821; and in a series of developments by British statisticians (Yule, Udry, Pearson, and above all Fisher) in the decades surrounding 1900 it took more or less its modern form.[8] Regression is not, in itself, a form of causal analysis. But it has come to pass as such.[9]

This suggests two (extended) remarks, one concerning interpretation as a general matter and the other concerning technicalities of statistical inference. These amount to criticizing this body of research on its own terms. The criticisms are essentially methodological. After

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[6] Emile Durkheim, *Le Suicide: Etude de Sociologie* (Paris: Ancienne Librarie Germer Bailliere, 1897).
[7] Consider e.g. Talcott Parsons in the United States, Anthony Giddens in the UK, Pierre Bourdieu in France, and Jürgen Habermas in Germany.
[8] <This version of the paper is in general under-referenced. [Specific requests very welcome!] But this paragraph in particular may call for detailed citations and/or a reference to a history.>
[9] There is at least a paper to be written glossing this sentence. I have no space for it here.

exploring the remarks, I will go on to make some criticisms in terms of what this body of research generally misses. These latter criticisms are of a more substantive nature.

In the typical academic management paper the central inferential weight is placed upon tests on estimates of regression coefficients. The first remark concerns regressors' calculation. A regression coefficient is more or less by construction a measure of association. The usual interpretation is that it measures the effect on the dependent variable of an incremental change in the value of its associated variable holding the levels of all the other given variables constant. But note the way causal language slips in. The coefficients are, computationally, the output of an optimization calculation in which a set of weights on the values of independent variables are sought that will minimize the sum of the square of the distance between their weighted average value and the value of the dependent variable.[10] This is in effect an essentially geometric characterization of the relationship between a group of vectors—a characterization of data.[11] The characterization may be more or less precise and this precision can be given a statistical interpretation (this basically being what the coefficient tests measure); but it remains a characterization of the fall of data, a quantitative measure of ex post association, and not a causal explanation. Correlation, as we have known since Hume, is not causation; and in the context of interpreting regression results causal explanation has to come from somewhere outside the calculations.[12] It has the form of an explanation, with implicit counterfactuals; but it has no content. Tight estimates that explain an agreeably large part of the variation in the dependent variable enhance our confidence in causal stories. But they are no ultimate foundation for such confidence.

The second remark is in effect about the regression's data i.e. the evidence that goes into the calculation. There are two common problems with data prevalent in the academic management literature. Much of the literature derives from actual observation i.e. *in situ* of the phenomenon of interest. But a substantial portion of the literature, particularly—though increasingly not exclusively—in the sub-fields closest to psychology, consists of experiments. Over and above the question of whether the experimental setting tracks closely the setting of practical interest, the experimental subjects are often not individuals comparable, in their education, work experience, general maturity, or in other respects their perspective to the people the theorizing is about. Most experiments are, as noted above, conducted on populations of undergraduate or MBA students or from the pool of people who go to Mechanical Turk looking for gig work.

The second is subtler. A calculation that comes early on in the classroom exposition of regression analysis demonstrates that omitting genuinely causal variables from the calculation (in particular, ones correlated with other included variables) will bias the estimates of the coefficients on the included variables and can do so in a particularly pernicious way (in that expanding the sample size—the amount of evidence—will not necessarily fix any estimate

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[10] David Luenberger's *Optimization by Vector Space Methods* (New York: Wiley,1968) embeds the derivation in a highly abstract account of optimization.
[11] See the exposition in Edmond Malinvaud, *Statistical Methods of Econometrics* (Amsterdam: North-Holland, 1966).
[12] <Again, a reference to Hume [*Treatise,* Book 1] in order here?>

precision problems).[13] The estimated standard errors will also be biased. Having the right data is very important even to estimating relationships in which all the important causal relationships are easily captured by third-person quantitative evidence.

This is trouble enough, but the trouble does not stop here. Researchers often face a difficult inferential problem in putting hypotheses to the test: it is often the case in the evidence they have sampled that more than one causal force is plausibly at work. This can lead to crippling difficulties.

The classic example concerns the supply and demand relationships expounded in an introductory economics course. The supply curve plots out quantities that potential producers will offer at different offer prices. This will be a rising curve on the traditional diagram giving quantities on the horizontal axis and price on the vertical axis, since as the offer price rises, higher-cost firms will find it profitable to enter and produce. The demand curve, which shows the level of output that will be taken up by consumers at various possible prices, will generally slope downwards in the same space. Supply and demand will be equal, and generally show no proclivity to change, all else equal, at the price-quantity pair at which the two curves cross.

There are many policy questions the answer to which will depend upon estimates of the shape (and, in particular, the slope) of the supply or demand curve for a given market. But a series of observations of putatively equilibrium price-quantity pairs will only trace out the one if the locus of the other has been stationary (so that any variation involved shifting the equilibrium up and down the curve in question). If both have been moving at the same time, the observed p-q points represent a hodge-podge and not estimates of either individual relationship.

Statisticians refer to inference problems of this sort as identification problems. They completely bedevil most natural data sources. Economists used to be fond of saying that they cannot conduct experiments, their subject is a world in which history is created only once; and over the half century to the 1990s, few economics journal articles dependent upon statistical evidence and inference from regressions had data innocent of these problems. But this state of affairs began to change in the course of the 1990s. The most celebrated paper exploited what was then called a natural experiment: to study the effect of minimum wages on employment, the researchers exploited the fact that two adjacent states had different minimum wage rates but in areas straddling the border fast food chain establishments that in effect standardized employment conditions and other aspects of the economic problem firms were portrayed as solving.[14] Comparing the behavior of firms in the establishments on either side of the border was presented as a natural controlled experiment, in which all variables except the one of interest were held constant and that one only varied. This approach caught on so effectively that it spurred not only the investigation of many further natural experiments but the conscious design and execution of many actual experiments, some in companies in the industrial economies and many in the Third World. The diffusion rapidly proceeded outside of economics. It has increasingly become a standard element of Ph.D. training in empirical methods the social sciences that feed business school recruitment and in business schools' own Ph.D. programs. A large fraction of the rookie

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[13] Any intermediate econometrics text will include a proof.

[14] David Card and Alan B. Krueger, "Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania," *American Economic Review* 84(4) (September, 1994): 772-793.

job talks in my own department in recent years have involved studious discussion of identification strategies, the hallmark of this approach. Indeed, considerations of identification seem to arise earlier and earlier on in the paper development process. Needless to say, as all this is happening, time is passing and the pool of journal article referees is changing. It is having some effect on editorial decision-making outcomes. Perhaps that is one cause of the change in project selection and paper-writing.

It would be reasonable to say that this development has been very good for the soundness of inference in the statistical studies in question. It has been less good for those studies' subject matter. In effect, the diffusion of this approach has increased the amount of clear inference but has moved to confine inference, clear or otherwise, to questions which can be investigated with suitably controlled experiments. This was not a problem in positive terms, in that there were quite a number of interesting questions which could be approached in this way. It is, on the other hand, a problem in that many questions of interest are for one reason or another bound up in settings quite resistant to experimental manipulation. The old lament of "knowing more and more about less and less" has more bite when the scale of the "less and less" gets small enough that whether it can be aggregated back up to a subject of interest is open to question.

I should raise one further point. It concerns the use of the word "theory" above (and in empirical social science practice). It has become a convention in academic management articles in the social science style in particular that each should contribute both to "theory" and to evidence. Since talents are heterogeneous, one might well have predicted that this would lead to a decline, on average, in the quality of published theory discussion. But the reality is far worse than that. There isn't any question that Durkheim and Weber had theories. One would say the same of their more recent avatars. But what passes for theory in the academic management literature is for the most part a very intermediate range matter, far closer to a description of the data (or to what are in effect summaries of the datasets of earlier researchers) than to a genuinely causal theory, never mind one with real microfoundations.[15]

This would be merely unsatisfying on its own. Its combination with another development has led, I think, to a kind of widespread discouragement that might represent an opportunity. The second development, flagged in the Introduction to this paper, burst into relatively widespread public recognition at the end of last summer with front-page *New York Times* articles reporting and then following up on a recent *Science* article.[16] The subject matter of all of these was a massive and sustained attempt by a consortium of researchers to replicate the central results of a hundred articles in high quality psychology journals. (Psychology is, for these purposes, methodologically one of the social sciences I have been discussing.) The consortium researchers made very extensive efforts to obtain the original data, to replicate exactly the way

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[15] On the subject of theory in this domain, there is food for thought in the symposium pieces Robert I. Sutton and Barry M. Staw, "What Theory Is Not," Karl E. Weick, What Theory Is Not, Theorizing Is," and Paul J. DiMaggio, "Comments on 'What Theory Is Not," *Administrative Science Quarterly 40(3)* (September, 1995): 371-397 (Weick's essay being particularly to the point of the sentence).

It is a curious commentary on the enormous weight put in the refereeing process on the (generally statistical) analysis of evidence that the graduate students seem to place very little weight on it in their own literature consumption. See Stephen R. Barley, "60th Anniversary Essay: Ruminations on How We Became a Mystery House and How We Might Get Out," *Administrative Science Quarterly* 61(1) (March, 2016): 1-8 at 4.

[16] See Footnote 3 above.

the data was processed before analysis, and to replicate the reported analysis using the same software, etc. They were in touch with the authors and asked questions when questions arose. The bottom line of the effort was startling: in only thirty-nine percent of the cases were the researchers able to replicate unambiguously the published results.

The general sense was that the cause of the non-replication was unlikely to be straightforward scientific fraud (making up data points, say) but rather abuse of the background assumptions of statistical methods (for example, not reporting negative or inconclusive results, winnowing databases, and the like): in effect, presenting definitely non-classical statistical hypothesis testing methods as if they were otherwise, conducting the classical tests, and drawing the inferences appropriate to the tests properly conducted. But many faculty members, particularly untenured ones and ones whose income is dependent upon ongoing grant support, feel they need a steady stream of publications. Referees are unlikely to catch this sort of behavior, since they read the manuscripts but don't, extraordinary circumstances aside, actually try to do the tests themselves all over again.[17] Journal editors have settled into a culture of valorizing newness of results. The *Science* article suggested a very vicious circle.

I now turn to some matters of substance. The first concerns what is left out. The problem of studying outcomes is the same problem economists encounter in confining their studies to equilibrium states: if what econometricians call the data-generating process is fraught with contingency rather than driven by laws of uniform application, the process that leads to the outcome may well have a powerful effect on which outcome is available for study. (Frank Hahn once put this point epigrammatically: "Long runs are only interesting if there is a plausible series of short-runs leading up to them.") The second is that the source of this contingency is often individual agency or path dependent processes (which you could think of very broadly as the emergence of routines) in individuals or groups. Social science measures almost always leave such considerations out entirely.

I return to an argument hinted at but not developed above. This "incompleteness" characterization, while true enough, understates the significance of the problem. Causality may have been a relatively simple thing in the inanimate world of physics and possibly even in the ambiguously animate world of molecular biology. But what about a world full of agency, never mind one full of agency conditioned on expectation of the agency of others (we are, after all, talking about *social* science)? The problems with an essentially behavioristic approach must be greater still. Reasons can be causes.[18] In the unambiguously animate world of conscious actors who can make interpretations, form intentions, etc., the unearthing of deterministic causal relationships in behavioral data may be a chimerical objective since investigation of causation that does not take understanding and expectations seriously is bound to leave out important potential elements. Philosophers of science can write all they want about a covering law theory of history.[19] There never were any examples that seemed persuasive, still less exciting, to

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[17] And they are not usually in a position to do so even if they are so minded, some small progress on this front in recent years notwithstanding.
[18] Donald Davidson, "Actions, Reasons, and Causes," *Journal of Philosophy* 60(23) (November 7, 1963): 685-700.
[19] Carl G. Hempel, "The Function of General Laws in History" *Journal of Philosophy* 39(2) ((January 15, 1942): 35-48.

working historians and there was certainly never any groundswell of affirmative response.[20] It may well be that the most to be hoped for, with any fidelity to the perspective of the actors, is to narrow down the range of possible explanations, to explore what it was like to have to make up one's mind under the circumstances, and to see what generalizations might flow from that while resolutely maintaining a sense of what is known and what is not.

Where does this leave social science-style academic management research? It strives to be scientific (one might even say that it suffers from hard science envy); but it is generally weak on the theorizing side of the scientific and even weaker on the testing side that is the heart of what is really is to be scientific. It is weak on the social aspects of its subject, not in its choice of research topics so much as in the sort of evidence it brings to bear. Yes, as sources cited in the Introduction suggest, it is a business in itself. Arguably it is, as Paul Samuelson used to say, a business of taking in one another's washing; but it is a business nonetheless. It is probably a business too entrenched to be vulnerable to attack; but I do want to argue that it is a business vulnerable to competitive (actually, will argue, to cooperative) intrusion.

4. A contingency approach: Analytical case studies as a modest identification strategy (or, What can a good case study, presented in the right way, do?)

I have been arguing so far that trying to retool business history to look like academic management's social science normal practice is a mug's game. The problem is less that our material isn't generally suitable for this, though it isn't, but rather that it is a peculiar aspiration—a counsel of desperate longing, it seems to me, rather than a strategic initiative. Business historians may refer to a variety of examples, but from the perspective of social scientists they are basically in the case study business. I think retooling is a very much less promising approach than accepting that our work will look always like case studies to social scientists in general and to social science-trained management academics in particular. Instead, we should ask what would make for a case study such an audience would find compelling—not just arresting on the facts and in the exposition but compelling in its cogency to matters they were already thinking about or were poised to find intriguing. The short version of this would be "Eccentric in format, incomplete in persuasiveness, but full of food for thought." The basic strategy is to figure out what their methods cannot deliver but would nonetheless be helpful for progress in their larger objectives and then to deliver it. We shouldn't, in other words, file and stretch and generally bang away at our work to make it look like a substitute for theirs. We should try to be a complement instead: not "more of one means you want less of the other" but rather "more of one means you want more of the other".

In an institutional setting like the one I described above, I think there is a real opening for research that raises questions with vividly memorable material. This isn't testing; but as I argued in Sections I and II, testing isn't everything. It isn't even everything essential. Some sorts of business history are, it seems to me, in a position to deliver on a question-raising agenda. But doing so effectively will take some fairly specific orientation. In fact, I will argue, doing so effectively starts from the question of what is missing in the conventional academic management literature. I think there are missing elements at two distinct levels.

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[20] Hempel's article has been the subject of so much criticism over time that it now seems almost a straw man.

Business school faculty teach, whatever else they do; and both faculty and administrators care about enrollments as well as individual teaching ratings. Most of the research the faculty publish is in an idiom with which the students are unfamiliar and is oriented to debates of which they are ignorant. Business school students gravitate to, and appreciate, courses they feel they learn something from. It is often said that the fundamental difference between history and the social sciences is that historical analysis generally takes a narrative form and social sciences analysis does not. I think this takes a form of exposition for a matter of substance and is in that misleading. I think that the fundamental difference is that historical analysis is generally about agency, about why events and outcomes emerged as they did rather than some other way as a consequence of individuals acting in some way when they might have acted otherwise. Social science research seems, in contrast, often to imagine itself in a world of deterministic laws and causal relations and indeed in one of these in which all of the relevant evidence is easily accessible to third parties. The social sciences look for relatively invariant patterns where historians may be interested in patterns but in a deep sense are most interested in where the variation, actual and potential, around the patterns comes from. One could minimize this distinction by saying that what social scientists are trying to do is to determine a set of categories and contingencies which have the structure of a causal theory if not the full detail of one. But even if these categories and contingencies appear and become relevant only over time, the underlying causal process still lacks any element of agency. It is still a physics of the social world and thus fundamentally different from the historian's view of that world.

Putting the distinction that way is, on the other hand, actually quite helpful in thinking through how business history could be helpful in a business school setting. Business school students are there (usually at a considerable opportunity cost) because they want to proceed on careers and into positions which will involve considerable decision-making responsibilities. In some fields, they hope to learn specific techniques and calculations; but the most helpful larger picture to have of their objectives is that they want, beyond the certification and the branding, to develop their judgment. The appeal of case method teaching is precisely that it obliges the student to diagnose problems and to develop and weigh possible solutions. (The appeal of doing this in groups of students is the opportunity to vigorously contrast alternative approaches, that is, to have active debate.)

Firm and industry history can be thought-provoking to such people. When such history is framed in an anti-Whig fashion, as an evolving environment and, from the perspective of firms, a series of challenges and responses (or non-responses) rather than as a series of outcomes, with a careful focus on organizational routines and what potential decision makers understood and knew, what sense they had of alternative courses of action, and what they thought would happen if they pursued any one rather than any of the others, it is in effect a very richly detailed multi-part case.[21] Most Harvard Business School cases are in one part or, occasionally, two. But Benson Shapiro's famous twenty part "Inland Steel Product Policy" case (with supplementary videos, no less), which the faculty in my day seemed to consider a faintly Dickensian self-

[21] See Raff, "How to Do Things with Time," *Enterprise & Society* 14(2) (September, 2013): 435-456.

indulgence (he had others with eight, nine, eleven, and fifteen parts, respectively), appears to me now in a rather different light.[22]

So much for the students; but what of the faculty, these days even at Harvard for the most part trained as social scientists or socialized to think about research in the social sciences way? I think that they are socialized to think of case studies in one of two ways, neither either necessarily relevant or particularly helpful. One is as research studies that promised highly generalizable lessons but turned out to be of at best idiosyncratic interest. The other is, like the HBS case studies, as a staple of their teaching materials but studiously designed to raise questions and prompt analysis rather than actually performing analysis or volunteering any conclusions.

But the sort of view of firm- and industry history I am suggesting suggests a very different possibility. I have argued that academic management research seems to suffer badly from hard science envy but is in fact generally weak on all aspects of the scientific, questions and how to answer them alike. Business history probably doesn't have much to add on the answering side, at least in the spirit social scientists are reaching for. But it has a great deal to contribute, I would argue, on the question side.

Case studies that reach towards generality in part by exposing the mechanisms by which familiar outcomes arise (inevitably, in the process exploring the opportunities—understandings and expectations—and agency as well as the observable circumstances of the actual or potential decision-makers) and in part by exploring what is idiosyncratic about those circumstances and what is not represent a very different sort of project. Such studies are generally not, strictly speaking, statistically identified; but the spirit of their project is in part identification and their very matter is an aid to practical men and women carrying out identification at work on their own. They do not provide tests or anything like controlled experiments i.e. with test- and control groups. But they can be extraordinarily helpful, in expositing process, in generating categories and questions. Given the right sort of subject and good luck with sources, the possibilities for getting into the thick of academic discussions and debates as well as of classroom discussions are considerable.

5. Publication and communities

This is to say that the right sort of business history can indeed be food for thought for colleagues (in journals, in seminars, even in job talks) as well as for students. The harder task is to get a job in the first place. Historians who position themselves as historians have a problem from the start, since in the context of a search turning up lots of candidates with conventional backgrounds and prospective writing career trajectories it is hard to successfully pitch someone who is hard to understand and compare in either way. But materials and then a job talk that speak to the search committee and others at the talk will go far. The journals appear to be fonts of convention and methodological fetishization; but I think there is an increasing atmosphere of

[22] Benson Shapiro, "Inland Steel Co. [A] (HBSP 9-587-134) – [T], Airframe Industry [A] (579-057) – [O], Chase Manhattan Bank [A] (9-590-084) – [K], Sedek Industries, Inc. [A] (584-132) – [I], and Bella Beauty Products [A] (9-587-092) – [G] plus Supplement.

nervousness and frustration among the decision-makers there and, again, an exotic who speaks compellingly can get considerable attention. A search committee will eventually ask itself what will come next, following on whatever the current project is; but a showing that current projects have pieces and facets and that there will be projects to come will go far if the current project seems imaginative and compelling enough/ (This I have observed.)

That suggests that one crucial question is what to talk about. It doesn't matter for these purposes why the researcher got interested in the research subject: the objective has to be to present material in terms of the audience's own interests. Two general domains immediately suggest themselves as areas of active research and hiring and, equally, areas on which firm- and industry-oriented business historians might be able to shed thought-provoking light. I itemize them, suggesting some specific topics, below.

The first is called in the business school curriculum Strategy. It means something like "aspects of the conduct of a business, statically and over time, to facilitate the production of supra-normal profits". (This is the economist's view of the competitive market on its head: it is about how to frustrate the forces your undergraduate microeconomics teacher was so insistent upon so as to earn profits above the competitive level.)

The dominant school of strategy theorizing argues that distinctive and difficult to replicate capabilities and their persistence at the firm level are what lies at the heart of the favorable profit outcomes.[23] But published empirical work on capabilities is overwhelmingly cross-sectional and relatively coarse-grained in character. Detailed and probing studies of the development, maintenance, and persistence of capabilities in individual firms and industries would be of great interest.[24] There have for some time been arguments that the ability of previously successful firms to adapt their capabilities over time has been key to many such firms' survival and probing studies of such dynamic capabilities would be of very great interest indeed.

A sense of the prospective value of some particular set of capabilities may be at the heart of any firm's foundation. But firms that have become important tend to be large and established. The question of how a firm gets from the one stage to the other—that is, the challenges of scaling or even just the initial ramp-up—are grossly underexplored. Microeconomics encourages students to think that there isn't a problem. (Students who think that all firms are like Internet firms that can rent cloud capacity and then contract out fulfillment to Amazon will think the same. They'll be just as wrong.) Research on cross-sectional datasets blurs away the problem. But talk to any business executive and you will hear all about the difficulties and dangerous choices. Academics have begun to notice this in a highly abstracted setting.[25] The

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[23] <Literature highlights here helpful?>

[24] There certainly are examples of a different approach. Two well-known articles are Steven W. Usselman, "IBM and its Imitators: Organizational Capabilities and the Emergence of the International Computer Industry," *Business and Economic History* 22 (Winter, 1993): 1-35 and Daniel M.G. Raff, "Superstores and the Evolution of Firm Capabilities in American Bookselling," *Strategic Management Journal* 21(10-11) (October/November 2000): 1043-1059. For monograph-length treatments, see Takahiro Fujimoto, *The Evolution of a Manufacturing System at Toyota* (New York: Oxford University Press, 1999) and Raff, *What Became of Borders?* (manuscript in progress).

[25] See e.g. Sidney G. Winter, "Scaling Heuristics Shape Technology! Should Economic Theory Take Notice?" *Corporate Change* 17(3) (June, 2008): 513-531 and Thorbjørn Knudsen, Daniel A. Levinthal, and Sidney G. Winter,

feel of the cloth would be helpful both in helping such academics figure out what details might be worth emphasizing in their modeling and in suggesting issues not captured in their original conceptualization.

Organizational adaptation is a similar sort of subject. There is now a very extensive literature studying the significance of organizations' ability to adapt to changing competitive or technological circumstances as a key factor in explaining industry dynamics. But most business school professors' understanding of how organizational adaptation might actually work derives from either coarse-grained external studies or a handful of teaching cases that do not in themselves represent a random, or even a systematically stratified, sample of anything. Careful empirical studies of successful and (perhaps equally important) failed change efforts which are sensitive to potentially idiosyncratic or constraining aspects of circumstances and opportunities would represent an advance in an area of considerable practical interest.

There is a sort of open ground between organizational adaptation and capability development that would also be worth occupying. Our intuitions about how competition works are often those suggested by an introduction to neoclassical microeconomics, with its analysis organized around competitive markets for commodity products. In fact, firms in most industries work very hard to differentiate their products and other aspects of their offerings precisely to have a distinctive positioning vis a vis potential customers and thereby avoid direct head-to-head competition with other firms. Detailed studies of firms managing this and adapting as competitive circumstances change would be welcome and might well shed light on the mechanisms of an obviously important but not well-studied phenomenon.

Another aspect of competition is only now coming into clear theoretical view and the empirical literature on how it works in life is thin almost to the vanishing point. This is the systematic management of value creation and value capture theorized along the lines of cooperative game theory.[26] This is in its outlines familiar to us from examples, one obvious one concerning Amazon's competition with the big box bookstore chains (though almost everything Amazon touches has this way of thinking running like through it like a red thread); and any Apple user will be familiar with the presentation of a well thought-through strategy of this sort (as will anyone paying that faculty member's equipment bills). I think that this perspective will start coming to wider attention now that the MIT Press has published the first popular exposition.[27] Settings in which this kind of strategic thinking is powerful are becoming increasingly salient (and economically important). I think that the first study to really make the empirics vivid will be remembered.

Resource allocation is, in a way, the most primitive activity of a corporate office and of an individual executive: allocating resources and then overseeing, to one degree or another,

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"Hidden in Plain Sight: The Role of Scale Adjustment in Industry Dynamics," *Strategic Management Journal* 35(11) (November, 2014): 1569-1584.

[26] For a business historical perspective, see Daniel M.G. Raff, "Competition and the Origins of Inequality" (typescript 032X15) (a BHC talk). See also my "Value Creation, Value Capture, and the Book-of-the-Month Club: In Its Own Time and in the Time of Amazon," (typescript in progress, most recent version 052318).

[27] Harborne W. Stuart, Jr., *The Profitability Test: Does Your Strategy Make Sense?* (Cambridge: MIT Press, forthcoming, 2016). (The exposition may be popular in style but the author trained as a mathematician and the text is very careful. [I think it is actually an excellent example of how both ends can be served.])

execution is what they do. The wisdom, and perhaps even the efficiency, of these activities may well have everything to do with whether the organization can carry on over any extended period of time. The most well-known study is based on four close case studies conducted over the period 1964-1966 i.e. half a century ago.[28] The infrastructure of information capture and -processing and the speed and potential radicalness of change in technology and product markets have changed enormously over that period. A recent conference volume edited by the author of that study and a junior colleague does not seem to have been widely noticed, still less thought provocative. This is an opportunity that would require sustained close research and more suitable for a Ph.D. student or someone already established than someone working on a tenure dossier; but I regard it, as a subject, as extremely ripe fruit.

The second general area is Entrepreneurship. The academic literature on entrepreneurship is shot through with peculiar silences. The proximate cause of the silences is that it is far easier to do with research with previously existing data sets or epistemologically unsound surveys than it is with genuinely longitudinal studies. This is even more worrying when the target population of firms will in large numbers fail and the kudo's among distant students of the field go to those giving analytical narratives explaining success. So there is a great deal of literature on the financing of start-ups and of financial aspects of founders' exit. There are many studies of network effects in early-stage finance. There are similarly many studies of aspects of team composition in early stage firms. There is a very economics-oriented literature about the effects of start-up activity on patenting (and vice versa) and on overall employment and economic growth. But there is very, very little about growing firms and still less on growing them profitably.[29]

The emergence of organizational order and specifically of organizational routines is an obvious subject. The theoretical literature on routines dates back at least to Nelson and Winter's 1982 monograph (with antecedents back even farther in the Carnegie School branch of the organization theory literature) and it has in the past decade, depending on your point of view, either been having a second flowering or suffering an attempted hijacking.[30] Some of the empirical studies in the second flowering stream are brilliantly memorable (no one who has read Birnholtz, Cohen, and Hoch on Camp Poplar Grove will ever think about summer camp in quite the same way again) but there remain great open spaces of business life waiting to be explored.[31] Phil Scranton and I have edited a recent OUP volume the heart of which is eleven business history studies; but these only scratch the surface empirically and the volume does not really develop categories, still less an analysis.[32] This is terrifically fertile ground (as the reviews published to date all note); and I think it will remain so for some time.

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[28] Joseph L. Bower, *Managing the Resource Allocation Process: A Study of Corporate Planning and Investment* (Boston: Harvard Business School Press, 1970).
[29] Raff, "What Entrepreneurial History Could Be and Why It Matters," typescript.
[30] The *locus classicus* is Richard R. Nelson and Sidney G. Winter, Jr., An Evolutionary Theory of Economic Growth (Cambridge: Harvard University Press, 1982). For what some think of as the highjacking, the seminar paper is Martha Feldman and Brian W. Pentland, "Reconceptualizing Organizational Routines as a Source of Flexibility and Change," *Administrative Science Quarterly* 48(1) (March, 2003): 94-118.
[31] Jeremy P. Birnholtz, Michael D. Cohen, and Susanna V. Hoch, "Organizational Character: On the Regeneration of Camp Poplar Grove," *Organization Science* 18(2) (March-April, 2007): 315-332.
[32] Daniel M.G. Raff and Philip Scranton, eds., *The Emergence of Routines: Entrepreneurship, Organization, and Business History* (Oxford: Oxford University Press, 2017).

Capability development and dynamics and scaling problems in this setting necessarily bear structural resemblances to their manifestations in established companies; but they are if anything of greater salience in firms just starting out. Development is important because without the credible promise of this working capital will not be forthcoming. Capability dynamics are important because unless the entrepreneurs have a very large lead on anyone else who has noticed the opportunity, they may well find that they have to "pivot" in midcourse. The problems of scaling are of course particularly acute when cash resources are thin. All of these together amount to a sort of phenomenology of start-ups coming to real life as practice. It is startling, given the demand for entrepreneurship courses and teaching (and the apparent availability of endowments to schools that take this sort of thing seriously) how little literature there is for academics to read and for students to imagine themselves into and to learn from concerning such subjects.

6. Conclusion

All of the topics I have itemized in the previous section could be thought of merely as longitudinal studies. But they are at their core history, not least because the interest in them lies most immediately in the agency of the actors. Historical case studies of the phenomena will seem cogent; and Very Very Modern History studies along similar lines will too. Both, with favorable case circumstances, archival, and, if possible, interview access, offer the possibility of representing an intervention into the journal article discourse without the business historian's trying to squeeze themselves into the corset of methodological conventions that don't really fit. This strikes me as a far more promising way to make business history seem useful in a business school setting. It is certainly a feasible program—it is what I have devoted a large part of my research efforts to over most of the past two decades. I hope some listeners take it seriously.[33]

In the spirit of the longstanding expressed concerns, I want to offer an incentive. I have raised some money to run a series of working group-sized conferences. (You could think of them as Paper Development Workshops if that language is familiar.) The idea is to have a small group of researchers and a somewhat larger but still intimate group of senior faculty members in related academic management positions. These will all be individuals in the thick, in one way or another (or several) of current literature debates. At least several will be on the editorial boards of journals that might be open to publishing cogent historical research. I want to invite some business historians with material that might be shaped or focused along the lines I have been sketching and the point of the meetings will be to give the historians a concerted and extensive

[33] Readers (at least in their capacity as authors preparing citations for their articles) clearly take the possibility seriously. The statistics for the most widely cited *Enterprise & Society* articles given on the website of the Cambridge University Press (the current publisher) seem oddly disconnected with those yielded by a search on the journal's title on Google Scholar. But as of the day the first draft of this paper was completed, my "Superstores and the Emergence of Firm Capabilities," which appeared in the *Strategic Management Journal*, had more citations than the most widely cited article that *Enterprise & Society* had to that point ever published, at least according to my research assistant (though to be fair, only slightly more); and about one-and-a-third times the citations of the articles in second through fourth place. I think this is more a fact about the ripeness of subjects and the discourse conventions in different communities rather than the intrinsic merits of particular articles; but the numbers are thought-provoking nonetheless.

sense of what might seem interesting about the material and how it might need to be developed to be make an impression on the relevant audience or discourse. In short, I want to create an institution that will last for at least a little while to help people who want to shape a presentation of their material to the sort of audience that might matter for business school appointments and personnel purposes. If this might conceivably be of interest, please get in touch with me.