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Interviews

Philip Scranton interviewed by Pierre François

Philip Scranton is University Board of Governors Professor Emeritus, *History of Industry and Technology*, at Rutgers University. He directed the Hagley Museum and Library's Center for the History of Business, Technology and Society for two decades and is Editor-in-Chief of the business history quarterly, *Enterprise and Society* (Oxford). His publications include fourteen books and over 85 scholarly articles. The most recent book is *Reimagining Business History* (with Patrick Fridenson), released in 2013 by Johns Hopkins; ahead is another monograph, *Making Jet Engines Work*, publication expected in 2016.

Scranton@camden.rutgers.edu

1 Your work deals with the birth and development of US capitalism in 19th Century, as do several economic sociologists, such as Neil Fligstein or William G. Roy. How would you contrast your approach of the topic with the one they developed?

First, a clarification. My research work started in the early 1980s with asking questions about the character and trajectories of US textile manufacturers, particularly those in Philadelphia, from the late 18th century through the 1880s. That research generated the book, *Proprietary Capitalism* (Cambridge, 1984). For the rest of the 1980s, I extended the Philadelphia project, tracing the regional industry's course through the end of the Great Depression. The bulk of that monograph, *Figured Tapestry* (Cambridge, 1989), dealt with 20th century phenomena, including the suburbanization of production, labor militancy, the introduction of synthetic fibers, and collective responses to crises of war and market collapse. *Endless Novelty* (Princeton, 1997) stretched beyond textiles and Philadelphia to attempt a national-scale reconstruction of what I've termed "specialty production," a durable industrial alternative to America's much over-hyped "mass production," addressing the last third of the 19th century and, roughly, the first third of the 20th. Since 2000, I've moved forward in time, researching aspects of post-World War Two specialty manufacturing, particularly machine tools and jet

engines, ca. 1940s-1980s, focusing on technological innovation and uncertainty.

So, on balance, only a portion of my research has been located in the discourse of 19th century capitalism's 'birth and development,' much like Neil Fligstein, who moved from *Transformation of Corporate Control* in the 1990s to drafting *The Architecture of Markets: An Economic Sociology of 21st Century Capitalist Societies* (2002), then recently policy and theory works – *Euroclash* (2008) and *A Theory of Fields* (2012). It seems to me that Fligstein's research arc bends away from history and toward theoretical synthesis and toward influencing present practice, whereas mine remained historical, pursuing the course of specialty sectors, a critical dimension of production (and by extension, services), both through the Cold War era and beyond the United States. William G. Roy's course has been more eclectic, shifting from *Socializing Capital's* analysis of the great merger wave, its antecedents and implications (1997) to an overview of social constructionism, *Making Societies* (2001), and just last year *Reds, Whites and Blues: Social Movements, Folk Music and Race in the United States*, which explores the ways in which Old Left and Civil Rights activists performed culture from the 1930s through the 1960s. Highlighting these research tracks briefly is the first step to responding to your question, as Fligstein has used his historical efforts to ground economic sociology interventions with direct contemporary impact, whereas Roy moved to other themes indirectly linked to his major work on 19th century US capitalism. I have remained in some respects a traditional historian, an "archives rat" seeking documents and images that can help frame narratives which, in turn, aim to highlight long-term industrial dynamics in modern capitalism.

More directly, there are sharp differences between my three wholly – or partly – 19th century volumes and Fligstein's *Transformation* or Roy's *Socializing*. The latter pair are anchored in the analysis of sophisticated data sets, Roy delivering a "quantitative test of efficiency theory" (21-40) – a core element of Alfred Chandler's heralded arguments for the economic/managerial logics of giant American corporations – and Fligstein utilizing multiple sets, some adopted and others constructed, to address location, survival and related longitudinal characteristics of merged firms and of America's top 100 manufacturers

through the 1980s – also presenting challenges and emendations to Chandler’s findings. Both books take up Naomi Lamoreaux’s reinterpretation of the “great merger movement” at the turn of the 20th century¹ as an axial moment defining American capitalism, though they take different paths in addressing it. Both rely heavily on published primary and secondary sources, with relatively little archival work, as may well be appropriate for studies treating big historical questions by using social science tools and theories. Notably, Fligstein explains that his approach relies on “organizational theory” from sociology, supplemented by economic and historical methods, which bring the state and legislators into play, actors Chandler had sidelined (5-11). Roy notes that his proposal for “an alternative analysis of the institutionalization of the large, publicly traded manufacturing corporation in America” draws upon several decades work in the “new economic sociology”, emphasizing “power theory” and the socialization of property while critiquing customary notions of corporate efficiency (9-16). Roy thinks Chandler is simply wrong, whereas Fligstein suggests he incompletely specified the field of action.

My approach in *Proprietary Capitalism, Figured Tapestry*, and *Endless Novelty*, was to start by noting the silences in the received narrative of American industrialization. Thus, big business was peripheral. Still, this was not an effort, as many commentators have erroneously stated, to emphasize small and middling size firms, but instead, to survey the industrial landscape to determine who had been excluded. The answer turned out to be firms at all scales that took a different path to profit than seeking standardization, cost-reduction, market share, and dominant competitive pricing. As I was employed in the early ‘80s at a small “college of textiles and science” in Philadelphia, the local industrial landscape beckoned. It featured an array of ruins, scattered across a 125 square mile city (ca. 325 sq. km.), that suggested the considerable prosperity of a vast manufacturing system. Familiar industrialization narratives used New England and the South to anchor tales of Massachusetts’ proto-mass production in textiles being superseded by the Carolinas’ cheap, displaced agricultural labor and fast, early 20th century technology – spinning frames, looms, and knitting machines. Chandler had retold the antebellum Lowell story as a preface to introducing genuine big businesses, the railroads, but I wondered what these Philadelphia companies had done to make serious money for generations (judging by the size and number of the city’s mills) and why they weren’t included in the American textile narrative or the wider industrialization

tale. Starting from silences is in a way the inverse of starting with big questions, though filling in those silences can in time generate big questions.

So long before any theoretical frames were installed, my research commenced with an empirical double move. I commenced driving and walking around the city’s decayed textile districts, taking photos and notes, and soon making copies of the 19th century insurance maps that, at intervals from the 1850s, detailed Philadelphia’s manufacturing, commercial and housing sites. This helped me populate imaginatively the spaces in which hundreds of firms, some employing thousands of workers, had practiced their versions of industrial capitalism, versions I couldn’t then specify, but which produced city-scapes utterly distinct from Lowell, Fall River or the South’s mill towns.² Second, about 1978, I was fortunate to secure research access to the Philadelphia Social History Project’s census data files, which in addition to large samples of population information, 1850-1880, included a comprehensive computerization of the parallel manufacturing data, firm by firm in all sectors. However, when examining binders of the manufacturing printouts, I encountered so many anomalous figures that I began checking them against the original manuscript census forms. So many transcription errors surfaced that I set aside the data files, dropping any data analysis that would have used them. Instead, I went back to the manuscripts to extract enterprise information, double-checking every entry to reduce transcription errors. Crosschecking to city directories allowed me to establish the spatial distribution of firms over time, as address did not appear on the census manuscripts. Thereafter, the spiral of source gathering scrolled outward, to industrial directories, litigation case files, trade and industrial journals, personal and company archives, government publications and the like. In a sense, what I attempted was to reconstitute the Philadelphia textile industry’s historical dynamism through layering documents contemporary actors created and through consulting its material residues in multiple neighborhoods.

For several years, my questions were rudimentary: who were these folks and what were they doing? Where did they come from and how/why did they succeed and fail? But the answers were distinctive. In large measure “my” textile capitalists were immigrant entrepreneurs (chiefly British, Scottish and German) running family firms at scales from microscopic to immense. None were incorporated, unlike the Lowell mills; instead they were partnerships and proprietorships. But most important, the work they undertook was not in standard goods, but in up-market special-

ties for fashion (styled and figured fabrics and knits) or for households (carpets, lace, upholsteries), made on order in batches by higher-skilled workers than found in staple goods plants. They also operated in networks of production, some specializing in fine yarns, others in designing, dyeing, finishing, weaving or knitting, rather, in general, than in integrated facilities. Moreover, they established reputations for flexibility, fast deliveries, and opportunistic pricing, the source of sizable, though uncertain, profits that derived from 'hitting the styles.' Their activities replicated patterns found in European textile manufacturing, featuring dis-integrated and interactive specialists who founded their own banks, eating clubs, and trade societies. It gradually dawned on me that these vigorous Proprietary Capitalists had been excluded from the US industrialization narrative because their practices did not fit the teleologies leading to positing big business and mass production as the core American industrial story. At that point larger questions began to bubble up, and theoretical resources from the neo-Marxist (or at least post-neo-classical) economic geography I'd been reading gained value and purchase, especially the urban studies debates triggered by David Harvey's *Social Justice and the City* (1973).

Much the same bottom-up, archive-combing practices informed *Figured Tapestry* and *Endless Novelty*, systematic narrative volumes that enlarged the scope of industrialization's history, emphasizing contingency and complexity, along with a claim that high-value added trades contributed as much to the structure of industry as did the giants of standardization and commodification, making small unit profits on huge volumes. This work perhaps also created back-stories for the later vogue of studying industrial districts/networks as America's Chandler-style corporations crumbled one by one with the secular decline of US manufacturing and the financialization of the international economy. The data I used was presented in descriptive statistics, not regressed in relation to models or hypotheses, and there is very little about any of the three monographs that would resonate with quantitative social science methods – a significant contrast with Fligstein's and Roy's approaches.

2 How would you describe the relationship between economic sociologists and economic historians in general? What is the relevance of sociological tools for your own work? And what has your work been received

and read in the community of economic sociologists?

Again a clarification, as I'm not an economic historian, at least given the field's methodological boundaries since the 1970s. As the preceding text suggested, I do not build/analyze data sets, create proxies, or devise ways to generate correlations/regressions that might argue for, if not nail down, causal relations. Indeed, I am deeply skeptical about the nomothetic viability of the hard-science-seeking aspects of the social sciences, epitomized in the mathematized interiority of contemporary mainstream economics. Such objections hardly matter to practitioners, of course, not even when root-and-branch critiques are delivered by colleagues who can "do the math", like Deirdre McCloskey or Donald Mackenzie. Instead, I work idiosyncratically, in ways that may resonate with Lucien Karpnik's economics of quality/singularities (*economic sociology_the European electronic newsletter* 15.1), as a historian of industrialization, or fieldwise, as a business historian and a historian of technology. Both disciplines have a critical, discursive, and basic-research-centered tradition, and both have gradually moved away from studying iconic objects (firms, artifacts) toward engaging broad processes of institutional, cultural, and practical change. For a more adequate perspective on the relations between economic historians and economic sociologists, I'd suggest you ask my colleague Naomi Lamoreaux at Yale. From scanning several issues of your newsletter, my sense, quite preliminary, is that economic sociologists have begun welcoming historians' perspectives that reflect qualitative research developed outside data-anchored projects. Economic historians, at least in the US, have been little interested in such work over the last two generations.

As for the use of "sociological tools" in my work, this of course turns on the contents of that phrase. One webpage lists the discipline's tools as including "field observation, scientific experiments, questionnaires and surveys, interviews, statistics, and studying primary and secondary resources," which is helpful if not fully satisfactory.³ Plainly, historians cannot use fieldwork (except archeological), experiments, surveys, or interviews (except to document the recent, through oral histories), but do actively work with statistics and both primary and secondary sources, as do I. Crucially, though, historians are impelled by our training to adopt a critical stance toward all sources, given that the information we recover is "always already interpreted" (an insight from Derrida, by way of Heidegger and Gadamer). This is a commonplace in sociology, I expect, but

historians have no techniques to achieve a defensible, “objective” viewpoint that could correct our sources’ twisted “vision” to 20/20-normal. Instead, we “triangulate” in order to secure multiple angles of approach to a phenomenon or process from varied sources, employing a peer-referenced subjectivity to assign relative values to each. As a recent commentary on a US Tax Court decision opined: “If conflicting information is provided in multiple sources, one must consider the hierarchy and reliability of such sources.”⁴ True indeed, and a fundamental challenge when it comes to debating hierarchies and reliability in contexts of power and class stratification. Moreover, with Joan Scott,⁵ when confronting statistics, historians ask who created them and for what purposes? What omissions can we detect and how might they be salient to the original statistical project, much less our appropriations of quantities? These questions and the frequent incommensurability of statistics gathered at different times can undermine database-driven projects in ‘social science history’. My sense that sociological theories are also significant research resources is not reflected in discussions of tools/methods that I’ve run across, but as you’ve asked about using theory separately, I’ll address that shortly.

As for the third query, concerning the reception of my work by economic sociologists, here I am quite clueless. Google Scholar shows some citations of my studies in scattered ec-soc journal articles, but I’ve not encountered a review of or engagement with the main monographs, or with the recent volume, *Reimagining Business History* (Hopkins, 2013), that Patrick Fridenson and I completed last year. Economic sociologists have crowded agendas, certainly, so I’m not surprised at this. Yet it would be lovely were there uses or critiques of my work that I’ve not discovered, or were your questions and my responses to provoke some.

3 One typical way to contrast history and sociology is to underlie the uneven weight of explicit theoretical insights in both disciplines. What is the role of theory in your own work?

OK, it’s good to take up the question of “explicit theoretical insights”. My sense is that the “role of theory” in the social sciences is deeply connected to scholars’ diligent search for rigor, predictability, and scientific standing. Rigor can, in this domain, involve setting aside or abstracting away the situational or non-quantifiable elements of social and historical situations, or, at other sites, it can

invite as comprehensive a depiction as possible of all the knowable actors and elements in motion.⁶ Whose rigor, whose theory, and for implementation toward what ends? These are potentially valuable questions to discuss, and have been current since the 1970s among historians of culture and politics, as well as societies, economies, and technologies (at least in North America).

My own engagements with theory commenced in graduate school at the University of Pennsylvania, where the 1970s history Ph.D program featured a full-year “History and Theory” colloquium with readings stretching across three centuries from Hobbes to Foucault, emphasizing Marx, Durkheim, Weber and Parsons – masters of social and economic analysis. Still, the relationship between theory and “history work” wasn’t obvious; a deep reluctance to “apply theory” was general, as was a deep unfamiliarity with what “application” meant for social scientific colleagues’ research. Our student cohort had encountered grand theory, compelling systems and conceptual arrays, but had little exposure to mid-range theorizing about, say, population trends, urban clustering, or capital flight. Once we finished our degrees and dispersed to teaching jobs, we were on our own, theory-wise. Not long after, Penn History dropped the “Theory” colloquium, signaling disengagement.

For me, the theory “bug” became a long-term infection, not least because, for about a dozen years, I coordinated running a leftist (and used) bookshop here in Philadelphia. Reading *New Left Review* while sitting by the cash register kept me up to date on current controversies along one vector of theory and politics, but college teaching and beginning the textile history research pushed me to gather usable concepts in urban economics and geography, for example. After leaving the shop to others’ care in the early 80s (it’s still in business!), my readings broadened to include most works by Giddens, Beck, Sennett, Latour, Bauman, Bourdieu, and Foucault, and parts of Baudrillard, Douglas, Castells, Goffman, Geertz, Habermas, Weick, Mirowski, and Gadamer, among others. Throughout, as now, I read theory for pleasure, scribbling reactions and cross-references in volume after volume.

And early on, I came across a comment from Anthony Giddens that captured what I’d view as the “role of theory” in my research. I’ve just gone back and recovered it:

“The concepts of structuration theory, as with any competing theoretical perspective, should for many research purposes be

regarded as sensitizing devices, nothing more. That is to say, they may be useful for thinking about research problems and the interpretation of research results.”⁷

Sensitizing devices, nothing more. This unplanned stew of theoretical work has provided me with materials that are good to think, as Mary Douglas put it long ago.⁸ The result is that, with some exceptions, theoretical conjunctions are noted in, but not axial to my publications.⁹ It is not my aim to employ theory to test explanatory hypotheses, nor to expect that theoretical frames will help model cause-and-effect relations – two alternative approaches, both of which lean toward the explicitness pole. Instead I seek to “match” concepts and historical situations/processes, looking among the many options for what strikes me as a plausibly effective means for organizing and interpreting historical evidence. It’s not any kind of scientific practice, but given a richly-populated theoretical terrain, such an approach can invigorate a narrative while broadening the value of a theoretical perspective. In this vein, my articles have sometimes introduced insightful theorists, like Giddens, Beck, or Bauman, to historians who’ve not previously encountered their work. Last, along this line, Patrick Fridenson and I undertook to share set of broadly-theoretical reflections with colleagues and students, through last year’s *Reimagining Business History*, a cluster of 43 short essays highlighting themes and concepts that may spark future research projects. Think of this perhaps as marketing sensitizing devices.

4 How would you describe the typical relationships historians and sociologists carry with economics?

I can’t speak for sociologists at all, of course, but I wouldn’t claim that there are any “typical” relationships among historians and economists, either. Still, it seems that the majority of US economic historians working in the quantitative vein are linked strongly to contemporary economic literatures, as honorific citations in articles’ literature sections would suggest. There are, however, non-conforming economic historians in the US and Europe, for whom the neo-institutionalist literature is salient, many of whom straddle the fuzzy border between blended qualitative/quantitative economic history and narrative-based business history. Some of them publish in the leading business history journals: *Business History Review* (Harvard), *Business History* (UK), *Entreprises et Histoire* (France) and *Enterprise and Society* (US-Oxford UP), the last of which I edit for the Business History Conference. Still, very few articles in any of these

four journals will utilize equations with ‘Sigmals.’ “The firmer the commitment to quantitative social science, the thinner the relationship with qualitative research” might be a plausible rule of thumb... Indeed, a third group of historians, at least in the English-language domain, take a broadly-cultural approach to socio-economic relations and have next to no connection with economics as a discipline, for they regard its abstract formalism, reductionism, and predictability goals as useless when exploring/explaining historical processes in context. Economists’ dismissals of such research as irrelevant to their concerns is wholly appropriate, as it’s not science, and not intended to be. Meanwhile, economists’ aspirations to scientific standing, sometimes cruelly described as “physics envy,” don’t seem to be working out all that well, despite their quasi-Nobel prizes? Last, at work more in business schools than history departments in the UK and North America, a cluster of business historians has found Nelson and Winter’s evolutionary economics provocative and productive as a means to locate institutional practices and problems in the flow of political, economic, and cultural activities.¹⁰ This seems a very promising vector.

5 How and why would you advise a junior researcher in economic sociology to work with historians?

I think this is best phrased “when and why,” because working with historians would surely be a situational prospect. Perhaps there’s a economic sociology project being developed concerning how 21st century information technologies are intersecting with lifeways in Europe’s remaining rural districts, with questions about pathways opened, customary practices challenged, outmigration triggered, or generational (or richer vs. poorer household) tensions enhanced or assuaged. An historian’s involvement might well be judged useful were the project leaders interested in contextualizing this phenomenon in light of earlier information technologies that intensified information flows and altered economic possibilities, such as postal services, rural delivery of newspapers, telegraphy, telephony, wireless radio (for coastal emergencies, weather), commercial and state broadcasting, etc. One could learn how island communities, for example, or rural enclaves in the Midi or Macedonia, experienced these repeated info “invasions” and responded, over the last two centuries, putting a long historical arc, not behind, but amid current activities.

Now a junior economic sociology researcher generating such a 21st century IT topic could not be expected to go

“looking for history” unless in his or her training and dissertation efforts long-term patterns of social relations and cultural practices or medium-term cycles of political and economic initiatives had been given some emphasis. History does ‘matter’ to many writers in this newsletter, but it’s not clear to me how this concern involves much more than scenery-setting, whereas many historians would aver that the power of culture, for example, arises from its status as congealed, and frequently-unacknowledged history or that the capacity of institutions to constrain and facilitate stems from the absorption of historical conflicts into unquestioned routines.¹¹ (For the value of history, see also Kenneth Zimmerman’s commentary on Neil Fligstein’s discussion [*economic sociology_the european electronic newsletter 11.1*, 41-44] of the 2008 market smash. Zimmerman’s text, at *economic sociology_the european electronic newsletter 11.2*, 48-52, especially 50, offers a critique of restricted visions among economic sociologists. Fligstein’s rejoinder at 53-54 is valuable, though I’d think it’s the Bruno Latour of *Aramis, or the Love of Technology* (1996) who would be most relevant to historicizing transformations in economic institutions.)

So, presuming that our junior scholar has an emergent project and that s/he recognizes that history matters, yet isn’t trained in historical research methods, what to do? Two things I’d not recommend. First, don’t consult a near-to-hand historian with a request to “pick his/her brain.” This presumes the historian is a data-source, like a reference book, and is unlikely to be effective. Second, don’t invite a historian of, in my imagined scenario, the Faroe Islands, Sicily, or Macedonia, OR of France’s PPT or Greek radio and television, to become a research partner. The chance that their interests would mesh with your research challenges, indeed that they would comprehend the project’s conceptual foundations is fairly remote, as many of us are both specialized and ill-read in “outside” literatures like economic sociology. Instead, first, I’d ask senior economic sociologists for advice about historians they’ve encountered who’ve done compatible research on earlier eras, whether or not directly informed by economic sociology. Second, I’d suggest getting in touch and involved with one of the European/global online academic networks among, say, communications scholars or the history of technology to begin thinking with colleagues about what sort and what scope of historical reading and research would build a sufficient foundation for the contemporary initiative. These online linkages frequently are productive and connect junior scholars with one another, a valuable outcome. (One group I’ve found welcoming and respon-

sive is the Tensions of Europe collective, run out of the Foundation for the History of Technology at the Technical University Eindhoven, but there are others, of course.) Last, and only if history looks to matter seriously to the project, I’d seek out a relevant summer seminar in historical practice and method to learn closer at hand how historians work and think, how they ask questions and zero in on sources that can provide paths toward answers, and how they build contexts within which those answers can have durable meaning. Ultimately, the most useful situation would be to find a historian intrigued by economic sociology who would join a project team both to become familiar with a neighboring discipline’s ways of proceeding and to share the somewhat different practices historians have devised, seeking intersections and interactions.

Endnotes

1Naomi Lamoreaux: *The Great Merger Movement in American Business, 1895-1904*. New York: Cambridge, 1988.

2This visual survey appeared in book form as Philip Scranton and Walter Licht: *Work Sights: Industrial Philadelphia, 1890-1950*. Philadelphia: Temple University Press, 1986. Paperback 1988.

3http://www.ehow.com/facts_5714343_tools-used-sociology.html#ixzz2xw7bLZMd

4<http://www.marketwatch.com/story/ira-rollover-ruling-stuns-advisers-and-savers-2014-04-04?pagenumber=2>

5A Statistical Representation of Work: La Statistique de l’Industrie à Paris, 1847-1848. In: Scott, Joan (ed.), *Gender and the Politics of History*. New York: Columbia, 1999, 113-38.

6See Bruno Latour: *Reassembling the Social: An Introduction to Actor-Network Theory*. New York: Oxford, 2005.

7Anthony Giddens: *The Constitution of Society*. Berkeley: University of California Press, 1984, 326.

8Mary Douglas: *How Institutions Think*. Syracuse: Syracuse University Press, 1986. Consider her provocative claim that “institutions perform the same task as theory. They also confer sameness.” (59) See also M. Douglas and B. Isherwood: *A World of Goods: Towards an Anthropology of Consumption*. New York: Basic Books, 1979.

9One exception is a just-recently-released article that directly explores the value of historical ethnography in technological history research. Scranton: Histories and Historical Ethnographies of Technical Practice: Managing Jet Propulsion in the US and France. In: *Entreprises et Histoire*(73), December 2013, 108-43.

10See Richard Nelson and Sidney Winter: *An Evolutionary Theory of Economic Change*. Cambridge: Harvard University Press, 1982.

11See Michael D. Cohen et al.: Routines and Other Recurring Action Patterns of Organizations: Contemporary Research Issues. In: *Industrial and Corporate Change* 5(1996): 653-698.