Dissertation Session Comments

Steven Tolliday

Department of Economic and Social History
University of Leeds

The dissertations I selected are highly diverse. They encompass a wide variety of subjects, and above all methodologies, freely crossing boundaries and combining the traditional focuses of business history with social history, the history of technology, economics, and individual and collective biography. In the limited time available I cannot give a detailed review and assessment of these theses but will concentrate instead on trying to respond to some of the most original aspects of each thesis under consideration.

David Kirsch

David Kirsch has written a methodologically and conceptually adventurous thesis. Drawing on "social constructionist" and "path dependency" approaches, he seeks to re-examine the origins of a major technical system and explore "the openness of possible alternatives." The thesis has a solid business history core: a measured and serious business history of important firms and enterprises related to the early electric car. But its broader significance rests on its wider approach to issues in business/technology history.

Could the outcome of the early technical competition between electricity and internal combustion have worked out any differently? Kirsch seems to want to assert the openness of alternatives, but I have the feeling he never really believes in his own script of openness.

The position that Kirsch has to dislodge is one of simple technical determinism. Internal combustion triumphed because it was a superior technology. The limits of batteries, in particular, could not be overcome while those of gasoline engines were satisfactorily resolved: *ergo* it's the battery, stupid, and at best the electric vehicle might have found a bigger niche in urban transportation or delivery systems.

This is a pretty solid case to crack. In the 1990s, with a strong commercial spur provided by U.S. emissions legislation, General Motors has in the last couple of years begun to market a serious electric passenger car (through its Saturn dealerships, mainly in Southern California). The EV1 runs on lead-acid batteries, has a 70 mile range and an 80 mph *maximum* speed. It takes three hours to recharge the battery (though the electricity costs only $1.90). It sells for $35,000 plus $2,000 for a home re-charger. Almost 100 years of battery

BUSINESS AND ECONOMIC HISTORY, Volume Twenty-six, no. 2, Winter 1997.
Copyright ©1997 by the Business History Conference. ISSN 0894-6825.

development has not produced a vehicle which can compete on key performance characteristics with the gasoline car. The reasons for favoring electric cars appear to be primarily ecological rather than technical or commercial.

And if 1990s battery technology is unimpressive, how much more so was that of the 1890s. 1890s batteries were horrid, mucky things, and, according to Charles Duryea "worse to take care of than a hospital of sick dogs!"

Does Kirsch convincingly reopen the case for EVs? He seeks to identify a number of possible crucial "decision points" where the early challenge of EVs might have been sustained by better business strategies of EV companies; "chance events" like accidents, race results, droughts, etc.; the strong law of large numbers; and, the timing of the development of battery exchange schemes.

His aim is to underscore the possible role of historical contingency, but his conclusions are remorselessly timid. Successive episodes end with phrases such as: "It is reasonable to imagine..."; "it is possible to imagine..."; "it is intriguing to imagine"; and "it is not inconceivable...". And chances of alternative outcomes are: "limited"; "small"; and "vanishingly small."

One accepts that alternatives were "conceivable", but were they *really possible*? Has Kirsch actually opened any real counterfactual windows here? Much of his account of the business failures of the early EV companies, in this sense, adds to and compounds the technical failure story rather than providing an alternative to it. In the end, his strongest case seems to be that there was a possible opportunity for a bigger niche for EVs: what he calls "a meaningful separate sphere" (particularly if EV entrepreneurs had played their cards better and – probably in conjunction with utilities – developed battery exchange programs at an earlier date).

The door to alternatives which has been opened slips closed again. As a result Kirsch seems a bit stuck when it comes to policy prescriptions. There is a plea for policies to "encourage technological diversity" or to keep open parallel technologies. But at what cost? How many alternatives? Just electricity? What about steam too? But maybe there are other doors into examining technological alternatives? Kirsch, in keeping with many path-dependency explorations. focuses mostly on small events and historical contingencies. Perhaps in doing so, he takes his eyes off larger (though also contingent) structures.

Lead-acid batteries, even in their poor state of the 1900s had a range of about 50 miles at 25-40 miles per hour. This sounds poor compared to gasoline engines – but, at that time (and with a little scaling up, even today), it is actually all that most people want to do with their cars most days. Yet, from an early date, people wanted cars that could do what they rarely *actually* do. Car buying decisions focused on maximum performance rather than typical use. GM's Saturn marketers are thinking of addressing this via bundling in with the sale of an electric car coupons for 10-30 days per year of rental of gasoline cars for these "non-typical" uses. Is it not at least worth looking further at why and how buying decisions came to be structured by "high-performance" product characteristics? The creation of an image or a dream of motoring (in which gender and masculinity; community, power, and space may all figure very

concretely) might in this perspective be more central to technology choice than we have thought. We would need, for instance, to know more about how people used and ran their cars in those early days.

In "social construction" theories, the pathbreaking "heterogeneous engineer" is the one who can simultaneously solve both the technical and social problems of a technology. In this case a "social" solution might have made the technical shortcomings less of a barrier for electric vehicles.

Kirsch's thesis is bold and highly stimulating, and a challenging foray into the overlaps of business and technical history.

Eric Guthey

Eric Guthey's fine and exuberant thesis is a reworking of a traditional theme in business history – the critical biography of the "great man" à la Tarbell, Josephson, etc. – for a postmodern world. The core of the work is a debunking of the myth of Ted Turner, skewering the legend of the self-made man or of the individual who single-handedly rewote history. Guthey is wonderful in destroying Turner "lore." His inheritance of a "bankrupt" company after his father's suicide, is shown to be generous cash flow to bankroll other ventures. While he was "single-handedly" turning round companies, he was actually away for months at a time on his yacht (Guthey can probably give you the dates and times). The energy of the thesis is remarkable, its dislike of Turner palpable, and its rejection of great man accounts undeniable.

But Guthey aims to do more than just legend-busting. He wants to use biography as a fruitful entry point for an understanding of the dramatic transition in American broadcasting in the 1970s. Does he succeed?

I felt there were some major problems. In a sense Guthey is trapped by the biography format. He wants to write an "anti-biography," to play off character against context, but somehow the exegesis of context is recurrently subordinated to the biographical narrative. The questions that force themselves to the fore are: Who thought it up? Who invented it? What was Turner's role? etc. Yet in successive episodes it often seemed to me that "going after Turner" precluded a more contextualized and broader analysis.

There are several contexts which I believe merit fuller investigation. We get tantalizing glimpses of the "social world" of Southern business in Georgia and elsewhere. We see elements of the clientilism, competition, wheeling and dealing, and risk taking. But the characters and networks remain background for Turner rather than being examined in their own right. The dilemmas of regulation as the system comes apart in the regulators hands is remarkable. Every protective move the regulators tried to make to keep a tottering edifice together seemed simply to open up new opportunities for liberalizers or to trigger impressive perverse effects (as in the defense of "local" rights and anti-leapfrogging rules). Guthey is great on how Turner interacted with this pattern and capitalized on it. But the regulators are left "out there," the pattern and logic of their actions relatively unexplored. I missed comparisons with Turner's rivals and competitors. Guthey notes that he was not the only one pursuing

various options, but he never follows through the strategies of rivals like Metromedia, Gannett, and Time Inc./HBO to show what others could make of the same opportunities or to identify and highlight the distinctiveness of Turner himself. Again the biographical focus perhaps makes it hard to handle such issues.

Finally, I wondered whether the rejection of character-centered causality perhaps went too far. It is fine to knock down exaggerated views of the influence of an individual. But what then *is* the role of character and individual? Turner was not just a cypher for the hidden hand of industrial change either. Sometimes this issue pops its head above the surface. In analyzing the rise of the Superstation, Guthey demolishes myths of single-handed action, but also concedes that Turner "deserves much credit" for the company's success. There were individual impulses that paid off handsomely (backing Professional Wrestling, locking-up movie libraries, etc.) and Turner often went against the grain. While his Channel was succeeding, others around him were failing (and being eaten up by Turner). I'd like to see a closer analysis of what Turner did right and others did wrong and a close definition of the role of personality.

I also look forward to Guthey's next installment. This is an unfinished project. We have only really reached the launch of the Superstation in 1976 and the real big deals like CNN and the media mergers are yet to come. Many questions that are opened here can only be fully answered after those issues are folded in too. I hope it will not be too long before the next installment.

Steven Toms

The dissertation by Steven Toms provides a further sharp contrast. This is a wonderful contrast and comparison to Leunig's also excellent thesis on very much the same subject. The difference in approach is striking. While Leunig uses Occam's razor to identify the necessary data to verify or disprove hypotheses and then goes out to pin it down, Toms is catholic and pluralist. He believes that the plentiful financial and accounting record of the Lancashire cotton industry (generally neglected or dismissed in the past) can provide new sources and angles on questions about the industry, and he dives into this wealth of difficult data to find it. And he comes up with treasure trove.

He convincingly demonstrates that these financial records can yield good and comparable material. He assembles new data series from fairly intractable stuff, and then he goes on to use them to produce some quite original conclusions about the industry (many of which, from a quite different approach, confirm what Leunig is independently coming up with). Perhaps his most important contribution is to shift the focus of analysis from the industry to the level of the firm: or to see an industry as a congeries of differentiated, segmented, and rivalrous firms.

On the classic rings versus mules debate, Toms makes two important contributions. Firstly, he shows that rings v. mules was a continuing and open technical race with advantages on both sides until the advances in ancillary technologies gave rings a more pronounced advantage in the 1920s. Till then

rings did not save on *total* labor costs and had significant cost advantages only in certain specialized niches of the industry.

Secondly, he shows that, nevertheless, ring spinners (notably in Rochdale) were more profitable than most mule spinners. However, he argues that these higher profits resulted from using rings for specialized niche production. (Though here a problem still remains: Given the profitable performance of rings *within these niches* why did more capital not move towards those market segments?)

But his biggest innovations come in his analysis of the finance of the Lancashire textile industry, where he puts his painstaking accumulation of data to excellent use.In particular, he highlights the hitherto unremarked pattern of flows of capital in the industry. He notes that (with a few exceptions like Rylands and Ashton) there was little tendency to reinvest profits to build up existing companies and their managerial capacities. Instead Lancashire shareholders typically *divested* capital from companies in the form of large share dividends and the repayment of loans (Oldham was known as "Diviborough") and then reinvested the capital in the industry in the form of new flotations of mills. The high dividends created the buoyancy to support the new flotations. The new flotations depended heavily on fixed interest loans, and once they made profits the surplus cash was used to repay the loans rather than deepening the investment in the company (a phenomenon with many echoes in modern leveraged buyout practices). Firms – but not the industry – were divesting heavily during the 1900s. Powerful promoters and shareholders dominated and corporate organization and specialist management remained weak.

In some respects this was a perversion of an earlier period of "shareholder democracy" or even "co-operative capitalism" in the Lancashire textile industry. In the 1860s and 1870s broadly based shareholder bodies exercised close accountability over managers and developed the firms. But by the late 19th century, powerful individuals were taking over this shareholder role for rather different purposes. Powerful individual venture capitalists pursued short-term returns and were less likely to plough profits back into the firm than specifically "family" businesses.

These investors were very sensitive to the signals of the market. In the first decade of the 20th century cotton profits were high and the industry was awash with capital. Unlike the steel industry, for example, owners and shareholders were not locked into the industry by fixed investments and low rates of return. Capital was available for redeployment and it followed the highest rates of return. These, however, were not in the new industries of the Second Industrial Revolution, but, above all, in cotton. Far from being in decline, Lancashire in the first decade of the 20th century was at its zenith of profits, outperforming the new industries. The old technical and specialized structure was yielding peak returns just at the time historians have been most critical of the failure to move into new industries. The reasons for this Indian Summer perhaps lay, Tom argues, in booming overseas demand and favorable international monetary conditions. But this was complemented by restrained real wage growth and industrial peace, which meant that productivity and

profits were also growing together. The attractiveness of cotton investment was irresistible: only in retrospect can we "blame the markets for giving the wrong signals" when we know that these favorable conditions could not be sustained.

Thus, Toms shows that the fortunes of the industry were not determined primarily by industry organization and technology alone. Strategy, finance, and profitability made contributions that have hitherto been underestimated.

Some questions remain unanswered: Why were new flotations seen as better ways to profit than further investment in existing companies? The implication is that new flotations could address themselves to particular market segments most flexibly as simple specialized ventures. But this leaves certain puzzles about how markets were identified and segmented, how "specializations" were defined in technical and marketing terms – and indeed shifts questions back to how Lancashire worked as a large industrial district of specialized firms. Interestingly, combining the work of Toms and Leunig seems to leave the question of "mules versus rings" almost overdetermined. Toms provides an interpretation that seems to adequately explain these issues in terms of market specialization and demand patterns. Leunig can determine them quite adequately through relative factor cost measurements alone. How do these two things connect up? Putting some of this work from the two these together in collaboration could be of the greatest interest.

Tom's work is often formal and technical. Perhaps the main weakness is some failure to capture the spirit of the sector and the nature of the formal and informal relations in the sector (perhaps like Geoffrey Tweedale so successfully did for Sheffield in *Steel City*). If the massive scholarship of the dissertation can be more thoroughly disciplined behind narrative and exposition, this work can be developed into an even finer book. In a densely developed historical field, the work of Toms and Leunig seems set to create a new historical orthodoxy.