

Symposium on Harrison's "Lean and Mean": What are the Questions?

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1. Introduction

I am grateful for the opportunity afforded by Prof. Zoltan Acs to re-examine the several themes contained in my recent book, *Lean and Mean*, and I had great respect for the scholars who were asked to contribute the critiques. But with a few exceptions, I'm afraid I learned less from these critics than I had hoped. Many of what I consider to be the most serious hypotheses and concerns about conventional wisdom that are expressed in the book (and in the five refereed journal articles whose publication proceeded it¹) are all but ignored, here.

In what follows, I will of course address what substantive criticisms there were, and try to ignore most of the *ad hominem* attacks, although, again, there are fewer of the former (and more of the latter) than I, or readers of this journal, might have wished for. If I did not do so sufficiently already, in previous writing, I will again acknowledge my debt to the work in Europe by Storey. In a few cases, notably with respect to the paper by Lerner, I appreciatively stand corrected on certain matters, and will say so.

But what turns out to be my main chore in this reply is to restate the main themes of the book, and to restate certain questions about the received wisdom on small business economics. I think these questions are important. I had neither the wit nor

the time to answer them thoroughly in the book. So, in the interest of promoting the inquiry, I will set them out, yet again.

2. The growth and diffusion of network forms of business organization

For reasons which are becoming more well understood to the research community, the largest corporations appear to be turning increasingly to inter-organizational, inter-sectoral, often cross-border boundary-spanning networks of suppliers and partners as a (partial) substitute for simply expanding their own wholly-controlled operations, e.g. by building a new plant, or adding a department to an existing facility. Both production and overhead activities within the individual firm itself are reduced in scale ("downsized"), and activity that was previously performed in-house is systematically contracted out to ostensibly external suppliers ("outsourced"), even during such periods as the 1990-91 recession, when (in the past) the make-buy ratio tended to be increased in order to keep core employees (quasi-fixed factors of production) occupied.

The big firms turn to such networking in order to access distant resources and institutionally-impacted information, to penetrate otherwise sheltered markets, and to become acceptable contractors to the governments of other states and other countries without having to make the fixed commitments associated with internal expansions and branch-planting. In an era of chronic uncertainty, heightened global competition, shortened product cycles, and rising short term performance requirements (manifested in higher de facto hurdle rates of return) imposed on managers by boards of directors and by the big institutional stock holders,

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this behavior is understandable. In terms of both firm- and national-level innovation, it is even arguably efficient.

The critics might have observed, but in any case, readers of his journal need to know, that there is a burgeoning literature on this subject, especially in the business schools and in organizational sociology. The questions are good ones: how new is this sort of networking? Are strategic alliances and other network forms *stable*, or do they tend to be formed, to collapse, and to re-form as opportunities and unexpected pressures require? Do networks tend to appear more frequently in some socio-economic contexts than others, e.g. how well do the Japanese *keiretsu* perform in the U.S., Great Britain, and southeast Asia? Then there are the paradigmatic questions, such as how well transactions cost economics explains this development. Finally, there are a host of policy questions, having mostly to do with whether using public expenditures and regulations to encourage such networking meets some public purpose and could in any case differentially affect firm behavior, and whether such interventions threaten the competitive principles that underlie received anti-trust policy. I cannot have hoped to provide answers to all of these questions – I would not know where to begin – but I certainly raised them in the book.

That is why I am thoroughly perplexed by the attitude of John de la Mothe, Gilles Paquet, and Jeffrey Roy. I simply cannot understand how they could write that I focus on the big firms “almost to the exclusion” of small and medium sized firms, governments, and other businesses. Whether the technical jargon is about “transversal coordination”, “quasi-vertically integrated structures” (a favorite among French and Italian friends), or my own “concentration without centralization”, the “alternative” model is that of interfirm *networks* consisting of both small and large companies.²

When Japanese scholars write about the horizontal (inter-market) and vertical (customer-supplier) *keiretsu*, or about bank-centered business networks in this country, the analytical question of how *coordination* is managed is quite central to the inquiry. Perhaps I overstate (or, more likely, have insufficient data for strongly supporting) the case for the hypothesis that the tendency is toward networks governed by big firms. But to say, as do

de la Mothe *et al.*, that “coordination may equally well be catalyzed by larger firms, by smaller firms, by governments’ industrial programs, or by spontaneous, tacit or emerging alliances wherever firms (of any size) interact” is no hypothesis, at all; it is a list. I recognize all the examples on their list. But to assert that *coordination* – which means possession of special power and influence, based on resources, position, license, or whatever – is associated “equally well” with any of these is analytically useless. They and I would all be better off specifying the conditions under which the coordinating role is more and less likely to fall under one or another of the organizational categories on their list, and then try to go out and measure how coordination is actually distributed. The evidence I have seen suggests a dominant role for the big firms, but I am certainly willing to be shown otherwise.

3. Small firms do not create the most jobs

A second theme of the book – and the one that presumably motivated Prof. Acs to organize this symposium – has to do with the widely perceived growing relative importance of small firms in this era: as “job generators” and as both designers and users of best-practice technologies. Others have taken positions on this question without any reference to the growth of networks (or, for that matter, to any structural changes in the socio-economy, in this country or abroad). But that is not what I have done, in *Lean and Mean*. There, the global changes come first, and provide a context within which to reinterpret the “small firms” debate.

Within that context, here is what I conclude, almost entirely from a reading of the research of other scholars, trade association data, and government statistics that carefully distinguish between the firm and the individual constituent establishment. Between roughly 1960 and at least the late 1980s, using the OECD-ILO operational definition of “small” as an establishment or enterprise with fewer than 100 employees,³ there was virtually no change at all in the size distribution of establishments or whole companies in the U.S., in either manufacturing or the private sector as a whole (the trends were also substantially flat in Japan and Germany; only in the U.K. did the small firm share rise). Storey observes that the

levels vary across countries; not only most of Europe but Japan, too, has higher small firm shares than the U.S. True, but my story is about trends, since the popular and political discussions are entirely about intra-national trends.

Perhaps those shares finally did begin to rise in the U.S. after 1987, the date of the most recent Censuses of Manufactures, Services, etc., available to me at the time of writing. But if so, I argue that this is likely to be importantly a consequence of the downsizing, outsourcing, networking activities of the big firms. Moreover, to the extent that the limited direct evidence (mostly from the U.K. and Germany) of substantial large firm *de facto* control over smaller suppliers is eventually found to be generalizeable, a growth of the small firm share that is driven by corporate strategic behavior does not have the same interpretation, or the same implications, as the popular "growth of entrepreneurship" story that is in such wide circulation.

Research on small firm dynamics, pioneered by Storey and his colleagues in the U.K. and pursued most recently in this country by economists at Dun & Bradstreet Co. and by Davis *et al.* at the National Bureau of Economic Research, strongly indicates that a tiny fraction of startups in any cohort accounts for the great majority of job growth over a subsequent period. At least in the U.S. (if not in Britain), the biggest firms have been the most likely to survive over long periods, while the "hazard rate" for the smallest is extremely high.

Normatively, the smallest companies on average pay the lowest wages and are least likely to offer benefits to employees, as well as the least job security. We have known this through the research of Gordon, Medoff, Loveman and many others cited in *Lean and Mean*. Around this average is presumably substantial variation. Thus, it would not be surprising if some professionals, e.g. computer programmers, editors, and financial analysts, "chose" the insecurity and lower expected income associated with self-employment or working in a small consulting firm, either for the psychic income afforded by freedom from supervision or for the (small) chance of making a killing (I qualify the word "chose" because our data seldom tell us anything about choice, only outcomes). But as the Bureau of Labor Statistics'

displaced worker surveys tell us, there is also a very considerable mass of workers who are involuntarily dismissed from large companies, where they earned higher incomes, and are re-employed in smaller businesses at lower pay.

One reason this is important is that, to the extent that downsizing and outsourcing are secular developments, and if I am correct that, on balance, they entail a shifting of work from generally large "core" firms to networks of generally smaller companies, then such a transformation could well be contributing to the measured, now almost universally acknowledged secularly increasing inequality in earnings, especially (but not only) in the U.S. I will return to this point in a moment.

In any case, Storey points out that "large firms do not have to be the prime provider of jobs in order to be the key players in a global economy". That is precisely my point about the centrality of generally large firms within spatially extensive, often cross-border, typically inter-sectoral production networks.

4. Flexible technology does not privilege smaller units of production

The question of the relationship between firm size and innovativeness – or, more properly, about the relationship between innovation and the distributions of size and economic power within an industry – is hardly a new one. As I say quite explicitly in the book, referring to Scherer, Cohen and Klepper, there is no mileage in making claims that one or another type of industry structure (more competitive, more oligopolistic), let alone one or another firm size, *per se*, is "more innovative". What I question in *Lean and Mean* is the claim – implicit in the original work of Piore and Sabel, quite explicit in Acs, Audretsch, and Carlsson – that flexible technology systematically privileges smaller units of production.

I think the answer is "no". Theoretically, large organizations are capable of incorporating flexible production cells as elements/departments/divisions/branches within their managerial orbits. And as the empirical evidence shows, they do. Whatever else may be true about the size/scale/innovation nexus, it is asymmetric. Small firms, with relatively fewer (or more uncertain access to) resources, find it difficult to use technology to

produce on a large scale. Big firms produce at all scales, large and small, using many different technologies and production processes. Large companies such as Lotus, Microsoft, and Japan's "software factories" seem to have found a way to incorporate craft work (in this case, programming) into their very complex and large-scale organizational structures, just as the big automakers and others, here and abroad, articulate work teams and shared control over technology on the factory floor with essentially bureaucratic forms of company-wide management. That the big firms are finding ways to reconcile large size with small scale is why I believe that Piore's important idea of "flexible mass production", Adler's concept of "learning bureaucracies", and Kelley's theory of "participative bureaucracy" provide better descriptions of how industry is evolving than does "flexible specialization".

Trade association and official government data, together with independent scholarly research, unambiguously show that both the takeup and penetration of (for example) computer-controlled factory automation during the 1980s were systematically greater for large than for small firms. If this technology, and others like it, did privilege smaller companies, then one would expect to see it being used more commonly by them, especially given the declining cost of acquiring at least stand alone computer numerical control machines. I understand Klepper's reasoning that the largest, most resourceful firms may acquire at least one such piece of equipment even if they have little use for it. But precisely because of size effects, the share of such tools in the firm's total stock would then decline with size. In fact, at least in the U.S. data, that share rises. Indeed, as I reproduce in Figure 3.1 in the book, the big firm-small firm penetration rates actually *widened* during the 1980s, according to Carlsson.

There is an important lesson here, I think, and that is to be wary of attempting to read off organizational structure from the so-called "imperatives" of technology. Organization and technology are independent (albeit intersecting) constructs. Neither determines the other.

Finally, in *Lean and Mean* I question the validity of the Small Business Administration's data base on innovations, which forms the empirical ground for the multivariate statistical work by

Acs and Audretsch. None of the contributors to this symposium either tried to defend those data (which would have amounted to implicitly calling for more data sets like it to be constructed) or challenged my criticism of them. That leaves us nowhere, on a very important subject: what kinds of information are appropriate to studying these problems?

My criticism was rather generic, and of course in no way cast aspersions on the theoretical or technical skills of Acs, Audretsch, or anyone else. I asked whether self-reported "innovations" are likely to be reliable or representative, especially when a small firm or entrepreneur's motive for explicitly reporting an "innovation" is to attract finance. I noted that the great majority of such self-reported innovations in the SBA data set fall into the residual category of "miscellaneous incremental innovations", and questioned whether models with a dependent variable having such a skewed distribution could possibly yield robust inferences, given the methodology employed by the authors. I thought those were responsible, sober questions. I am disappointed that my critique has not aroused a serious response. Is the appropriateness of the data we use to answer our questions about small business economics that marginal a matter?

5. On the role of finance in forming and reproducing the industrial districts

This is surely the weakest aspect of the book. I had only a few things to say about the role of financial institutions in relation to those geographically concentrated clusters of firms called "industrial districts". In the case of Emilia Romagna, in Italy, I drew on my own interviews, and those of the Bolognese research institute, NOMISMA, to show how the penetration into the region of such outside financial holdings as those controlled by De Benedetti have transformed the ways in which the local area networks operate, e.g. by imposing standardized supplier selection criteria on the local firms.

As for Silicon Valley, Lerner's essay in this symposium is a welcome corrective on some of the facts which I took from Florida and Kenney's *Breakthrough Illusion*, and a reasonable request that I tone down the rhetoric. I am, of course, fully

responsible for reproducing factual errors; mea culpa. But I don't think that is all there is to this interchange. Lerner introduces into the discourse a number of historical institutional facts which I, at least, always welcome in doing political economy (note in the chapter on Italy how I relate the rise and stall of the woven textile district of Prato to the rise and fall of the international agreements that, for a time, imposed severe protectionist tariffs and quotas on textile imports from the Third World into Europe and the U.S.).⁴

I take Lerner's twin reminders that (a) the venture capital industry has, since the 1940s, *always* been about maximizing capital gains; and that (b) the federal deregulation of pension funds in the late 1970s freed up an enormous volume of savings to be mobilized by the venture industry, to constitute a story about the *timing* of the burst of firm formations and sell-offs after 1978, in Silicon Valley and elsewhere. This story seems broadly consistent with my own. In other words, I don't see my argument implying opportunistic behavior, so much as the relaxation of a regulatory constraint with strong effects on the subsequent development of sectors (and places) highly dependent on attracting capital investment. That such financial deregulation reinforces what Michael Porter and others call the short-term decision-making bias of American investors would also be relevant, to the extent that impatient capital is antithetical to the stability of local social relations (but then, perhaps it is not, as Saxenian argues).

I think that these are fascinating, interesting questions, varying answers to which underlie the apparent disagreements between (for example) myself and Saxenian, whose excellent book, *Regional Advantage*, makes the strongest, most well-developed case yet for why the embedding of business relationships within common local institutions and non-economic shared values is important to understanding why some high tech regions do better than others.⁵ Readers of this journal need to understand what the questions are. My particular answers are less important to furthering the shared research enterprise than is a sharper framing of the questions. That is why I found Lerner's essay so constructive and helpful.

6. Growing polarization of the work force: The "dark side" of flexible production

To my great surprise, none of my critics address this theme at all. As work is increasingly shifted from the internal labor markets of the big firms to temporary help agencies and generally (although not invariably smaller) external suppliers, and as even core labor processes inside the big firms make greater use of part time and "temp labor", horizontal equity is threatened, especially where people with the same skills work side by side (for a time) at very different rates of pay and benefits. It is also quite plausible that this sea-change contributes to the now widely acknowledged secular rise in earnings inequality. We cannot know, since the data base for testing such a hypothesis does not exist, but I am willing to bet that at least part of what is now being attributed by economists to "skill-biased technological change" or, more generally, to rising returns to higher education, is actually a result of this shifting in the locus of where production takes place. Concern about growing income polarization hardly needs justification; last November's congressional elections, alone, indicate the importance of this problem to (among other things) conditioning the political climate within which business firms must operate.

7. On policy

Several of the contributors to this symposium take me to task for having too little to say about economic development policy, or of being uncharacteristically tentative. I agree with this assessment. It is difficult to prescribe a coherent, consistent approach, given the contradictory, complex findings that are reported in *Lean and Mean*. I had a devil of a time with chapter ten.

For example, I share Teece's concern that a knee-jerk anti-trust stance could endanger the innovativeness within networks that comes from the kind of sustained learning that collaboration makes possible (as evidenced, for example, by Sematech). And yet I also recognize, and to some extent share, Siegel's fears of encouraging anti-competitive combinations.

If any of the critics read into my writing a value judgment in favor of "vast intercorporate networks

of interlocking companies", then I haven't written nearly so clearly as I had hoped (or those critics do not know my earlier work). On the other hand, I cannot have been more clear in saying that I take the evidence on relative small firm backwardness to mean that economic development policy needs to be especially targeted toward upgrading the technical capabilities of smaller companies and building closer links between them and the big (or meso-level) firms that might act as their customers. I say this so often in the book, in my academic papers, in my consulting work, and in seminars at Carnegie Mellon University which Klepper has attended that I am literally at a loss to explain how he could suggest that I would "no doubt argue" that "there is no need for policies to promote small firms". I am no fan of class legislation, generally, and thus do not endorse (say) tax incentives to small firms simply because they are small. But in a dozen other ways, I do strongly advocate special support for smaller companies, as part of a strategy for encouraging the building of innovative networks. In this regard, I find the ongoing U.S. Commerce and Defense Department programs that promote technology transfer, manufacturing modernization and the use of "dual use" technologies, to upgrade the capabilities of especially the small and medium sized companies, to be quite positive developments.

On the subject of the role of unions in strengthening the innovativeness of small firms, Siegel is correct in reading me as "pro-labor" (although that value judgment does not really play much of a role in this particular book). There are many examples of unions that actively support what are clearly labor-saving innovations, in order to increase long-run growth and the demand for skilled labor. On the other hand, I cannot convince myself that worker buyouts or labor-management problem-solving teams – both of which are high on the labor movement's list of policy priorities, these days – are, by themselves, contributing importantly to the modernization of smaller firms, in particular, and to technical progress, generally.

8. A closing statement on style

Some of the critics seem to be put off by my popular writing style, and choose to strike out in rather intemperate language which does nothing

to promote our own mutual learning about the problems raised in *Lean and Mean*. I am philosophical about this (I am also amused, given my bulk, at being likened to Don Quixote!). After all, what is an author to make of it when what one critic (Siegel) praises as a "careful marshalling of the facts" and another (Storey) essentially calls "old news" is judged by yet a third (the Canadian trio) to consist of nothing more than "rash attacks"? As for Klepper's characterization of my factual description of the well-known direct relation between firm size and wage rates as depicting the small firm sector to be "the repository of society's economic dregs" (a term I would and could never think of using), I'm afraid this says more about the critic's attitude toward low wage workers than it does about me or my book.

But there's no doubt that popular writing about serious subjects in economics is both dangerous and messy. Still, someone has to do it, if the broader public is to be included in the discussion (and if it is not, then of course our intellectual explorations cannot be considered democratic, with a small "d"). Surely the professoriat is not licensed to communicate solely with itself.

I tell my students that their most important task in their writing, whether mathematical or literary in style, is to frame good questions. That Prof. Acs decided to mount this symposium, that such a diverse group of scholars agreed to participate, that INC Magazine is itself preparing a special issue on the subject, and that the book will appear (at last count) in at least four foreign language editions, suggests that the subject matter of *Lean and Mean* is being taken seriously. I am most grateful to the editors of *Small Business Economics* for acknowledging the importance of my questions, and thank my critics for challenging and causing me to re-think my particular answers.

Notes

¹ See Harrison (1992, 1994a,b,c).

² I find myself rather put off by the inclination toward reification that permeates their paper, and many others like it in this field. There is little useful social science in De la Mothe *et al.*'s proposition that "technical, functional, social, historical, ideological and cultural forces . . . 'find' each other and create new forms of combining in the perennial search for synergy". "Forces" don't 'find' or do anything; actors and their organizations do, within the context of forces, relations, and

the rest. It is an insult to the careful theorizing of Cohen, Levinthal, Saxenian and others mentioned by De la Mothe *et al.* to associate those names with such imprecision.

³ The U.S. Small Business Administration's definition of "small" as consisting of businesses employing fewer than 500 people is manifestly unhelpful to analysis, and should be changed, both for research and policy (e.g. resource allocation and regulatory) purposes. According to the Census Bureau, in 1987 fully 98.4 percent of all companies in America were, by this definition, "small". Considering just manufacturing, that share was 93.9 percent. A working definition that includes nearly every member of a population is at best not very interesting. We could wish that this analysis of the changing (or unchanging) firm size distribution could be conducted in terms of sales (which is how business itself measures power and influence) or value added (which would, in an era of rapid technological change, help us to triangulate measures based purely on employment). But as every researcher in the field knows, the only possible way to construct reasonably comparable data series, across time and countries, is to use employment, with all its ambiguities.

⁴ I also argue that such powerful firms as Hewlett-Packard, and non-business institutions such as the U.S. Department of Defense, have always played a major role in the economic life of the Valley. I perhaps overstated the case to the point where it appeared to some readers, including Lerner, that I was dismissing the role of the clusters of smaller companies –

whatever the role of venture capital in integrating the latter. Here, I will only repeat that my main argument is that these smaller, agile computer and semiconductor designers and producers are linked together into networks in which very large and powerful corporations and other organizations are arguably the hubs.

⁵ I have been well aware of Saxenian's work for many years. She was my teaching assistant at MIT, I was a member of her doctoral dissertation committee on the project that became *Regional Advantage*, and we are close friends.

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