Firm Size, Efficiency and Employment: A Review Article

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1. Introduction

In their recent book, Storey and Johnson (1987) have provided the most comprehensive account to date of the many studies on firm size and employment behavior which have appeared in the last decade or so. Interest in this topic was sparked by Birch (1981)[1], who provided evidence that most new jobs in the U.S. were generated by small firms. This finding has been basically upheld and extended to other countries by subsequent writers (including Acs and Audretsch, 1987a; and Birch, 1987), in spite of controversy over details, and owed its considerable impact on policy making to a number of factors discussed by Storey and Johnson (S-J). However, they do not consider the surprising neglect of this and later studies by mainstream labour and industrial economists.

In the 1960s and 1970s it was widely believed (though without much hard evidence) that modern technology and economies of scale implied efficiency gains from large firms. Particularly in Europe with its tradition of cartellization, government policy encouraging merger and favouring large firms has skewed the size distribution and raised barriers to entry and exit (Geroski and Jacquemin, 1985). In the meantime evidence accumulated that small firms were often more innovative, that large, "mature" firms were rather inefficient, and that non-pecuniary costs of size were substantial.[2] Furthermore, a secular shift of demand away from mass-produced goods, where scale economies were most plausible, to customized and batch production, and services, where efficient scale tends to be relatively small,[3] coincided with an uprising of popular sentiment against bureaucracy[4] as well as post-OPEC unemployment and stagflation.

These developments have stimulated government programs in support of small firms which are listed by S-J. Surprisingly, however, they do not discuss the neglect of firm-size and employment questions in the relevant fields of economics. One likely reason is also a major weakness of S-J's book and most of the research they survey, and that is the atheoretical, largely descriptive nature of this work. A lack of hypotheses based upon standard economic theory has hindered recognition of the area, as well as progress beyond collection of stylized facts.[5] In this review we shall therefore attempt to discuss some of the main results in the light of current research in labour economics and other fields, and suggest some promising avenues for future research.

2. Stylized facts

The modern contractual view of firm organization[6] renders the distinction between independent enterprises and subsidiary establishments less clear-cut than implied by S-J. Many specialized, small subcontractors, particularly in Japan, are in effect 'non-owned subsidiaries' of a single large corporation. On the other hand, partially or even totally owned subsidiaries, particularly of conglomerates or holding companies, may enjoy considerable freedom to compete, buy and sell in many different markets.

While these considerations qualify the use of simple measures like 'size', they also suggest that distinctions based on legal classification such as the 'enterprise-establishment' distinction stressed by S-J, are of less importance than they might appear initially.[7]

A *new* firm or legal entity is fairly unambig-

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uous, and it has long been known that only a few start-ups become large firms, many fail quickly, and most survivors remain small. As S-J point out, it follows that employment gains are concentrated in the small proportion of highly successful new firms, and blanket encouragement may attract less able entrepreneurs and generate social costs through increased bankruptcy rates.

Despite the data problems which are exhaustively discussed in S-J's first 5 chapters, there seems to be no doubt that the proportion of small-firm employment in both manufacturing and services has risen moderately in most industrial economies, whatever their macroeconomic performance. However the share of small-firm employment varies widely and is not simply related to macroperformance, which is hardly surprising in view of historical and structural differences, and the many other determinants of macroperformance which are ignored by S-J.

S-J describe three different developments behind the aggregate changes noted, in the form of stylized case studies. High tech firms and related services are associated with employment growth in the Boston and other areas favored by the Reagan administration's build-up of defence outlays. It should be added that U.S. deficits and public (foreign-owned) debt have increased correspondingly, and many economists argue that Keynesian demand expansion could also solve Europe's employment problems (Blanchard and Summers, 1987).

Small, 'low tech' but high quality firms have become important in parts of Italy, often exporting successfully, and relying on extensive family networks to facilitate flexibility and evasion of government regulation and union constraints.

In contrast to these success stories, S-J see the growth of self- and small-firm-employment at relatively low pay resulting from the decline of traditional manufacturing in the British Midlands in a less favorable light. Their main point seems to be that encouraging new start ups is not a substitute for appropriate macropolicy. They also note that skilled, 'core' workers have enjoyed rapid real wage growth while unemployment of 'secondary' workers increased, but S-J offer no discussion of collective bargaining and the union policies which many economists consider to have much more relevance for aggregate employment than the size distribution of firms.[8]

Turning to *quality* of employment, Chapter 6 of S-J addresses several interesting issues which *have* received attention in the literature, but again they ignore most previous relevant studies. S-J start by arguing that flexible production in small manufacturing firms is likely to require higher average skill levels (and less division of labour) than traditional mass production or assembly by semi-skilled operatives (p. 157). This point has often been made and scarcely disputed, so the opening sentence of their next paragraph is rather surprising:

> Finally, it may be expected that wages and working conditions of employment will be inferior in small firms to those of larger firms. This is partly because small firms are likely to employ more 'peripheral' workers, and also reflects the relatively low level of trade union membership in small firms (S-J, p. 157).

The contradiction is never subsequently resolved, nor even discussed, but S-J simply proceed to show that smaller firms in the U.S. had more female and part-time workers, fewer administrative and managerial employees, and more low-wage employees in 1979. Similar results hold for other countries, and in the U.K., "earnings in large firms are up to one third higher than earnings of comparable workers in smaller units" (S-J, p. 198). This comparison is based on plant-level data controlling for percentage of females and union members employed, but does not include individual characteristics or proxies for human capital and working conditions.

3. Interpretation and critique

In traditional economic theory, firm-size is a relatively uninteresting variable. Workers with given abilities sort themselves into their most productive occupation, and earnings depend only on total human capital (including experience), and compensation for work-place public goods or bads. In various models,[9] the firm-size distribution is determined essentially by the distribution of managerial ability, with highest ability individuals heading the largest firms. The notion of a competitive labor market has received its severest challenge from recent, seminal studies, by Dickens and Katz (1986), and Krueger and Summers (1988), of interindustry wage variation in the U.S. Using panel data, they find major wage differences across industries after controlling for individual worker characteristics. Neither unionization nor compensating differentials can plausibly explain these differences, which appear to be stable across countries and over time, and are much larger than union effects.

Some form of rent-sharing or generalized efficiency wage effect offers the most likely explanation, particularly because wage differences are correlated with industry concentration, firm-size, and profitability.

These results have far-reaching consequences for much of industrial and labour economics. In particular, we can now throw some light on the surprising results discussed at the end of the previous section. Note first that rent-sharing presupposes the existence of a firm-specific rent or surplus over all factor opportunity costs. This may be due to specific investments or mobility cost even when product markets are perfectly competitive, but of course imperfect competition or market power will generate rents even when there are no firm-specific investments. It is plausible that most small firms have less market power and lower rents than generally more capital-intensive, large firms, so that higher earnings in the latter could simply result from rent sharing. In this case, higher earnings in large firms do not contradict higher average skill levels in small firms in the same industry.

Furthermore, the results on earnings presented by S-J do not distinguish between blue- and white-collar employees. If the highest earnings category includes the administrative and managerial (white collar) employees who are overrepresented in large firms, then the differences are consistent with higher skills for blue-collar workers in small firms even *without* rent sharing. These two explanations are thus complementary rather than mutually exclusive.

In any case, if earnings in small firms are closer to competitive levels (for given worker characteristics), then this is an argument in favor of small firms, rather than the contrary, as seems to be implied by S-J. Other things being equal, employment is likely to be greater and prices lower if more competitive small firms replace rent-sharing large ones.

S-J and many others have noted an apparently increasing preference for the more flexible, less bureaucratic working environment presumably provided by small firms. On the other hand, they repeatedly castigate the 'low quality' of employment provided by small firms on various non-pecuniary dimensions, so that we seem to be faced with another unresolved contradiction.

Clearly, the working conditions and 'job-satisfaction' provided by an employer are important components of 'output', though they are difficult to measure and hence often neglected. Scherer (1976) and others have shown that employees are *less* satisfied with their work and strike more often in large firms than in small ones, after controlling for other relevant factors. Unionization is also negatively related to job-satisfaction for a number of reasons (Duncan and Stafford, 1982). Unionized employees may have to work harder to earn their pay differentials. Precise and detailed job classifications in collective bargains reduce flexibility and encourage simplification and routinization of tasks, so that bargains can be more easily monitored and if necessary enforced by third-party arbitration.[10]

Furthermore, unionization and other formal institutions of organized labor such as West German works councils and codetermination are typically *large*-firm phenomena, precisely because informal voice channels and direct worker participation in decision making are much easier to implement in small organizations. S-J ignore the extensive literature on productivity benefits associated with group incentives such as profit sharing in small firms and cooperatives just as they miss the relevance of Taylorist work organization and extreme division of labor for propagating traditional, adversarial industrial relations in large firms.[11]

An entirely different matter (not clearly distinguished by S-J) is the oft-noted concentration of 'secondary' employment in the small-firm and service sectors. Secondary employment is characterized by high part-time, female and minority representation, as well as low skills, wages and job-tenure, with lack of career opportunities. As an example of classification problems, typical secondary, service occupations are in fast food franchises and retail outlets. Fast food chain establishments with uniform products and working conditions have little to do with independent entrepreneurial firms, and so should better be classified as large firms like retail chains. Many part-time female workers are second family wage earners who may quit frequently for personal reasons, but prefer low-paid jobs to no job. In West Germany, for example, there is relatively little part-time employment, but surveys indicate considerable unsatisfied demand for part-time jobs, mainly by women with families. Even in Britain, the tendency to replace primary jobs by lower-paid secondary jobs may have been the only option to employers facing intransigent unions, without which unemployment would have risen even further. The expansion of secondary employment, as an alternative to visible or hidden unemployment, is not necessarily welfare-reducing as S-J seem to believe.

4. Policy conclusions

In their final Chapter 7, S-J discuss public policy towards small firms in various countries. They argue for targeting assistence to fast-growing, successful small firms, rather than encouraging new start-ups (most of which are likely to fail) as a general policy to alleviate unemployment, which is the preferred policy of the Thatcher administration in Britain. Again there is much confusion and omission in this chapter, as well as a kernel of truth, so let us consider the basic justification for subsidizing new or small firms.

First and foremost, and entirely ignored by S-J, is the historical fact of decades of government support for large firms and mergers of doubtful social value mentioned above. Artificial barriers to entry and exit have been errected on the basis of a misguided policy (Geroski and Jacquemin, 1985), the explicit reversal of which is required to attain a more competitive economic structure composed of smaller and more efficient units.

Rent sharing for efficiency wage and other reasons on the scale indicated by recent work[12] implies that the usual estimates of entry-barriers and monopoly rents based on observed profit-concentration measures are serious underestimates. Furthermore, the scope of government transfers and purchases in modern economies provides a strong incentive for unproductive, rent-*seeking* activity, where large firms also have major advantages. A disproportionate share of government grants and contracts in most countries go to the large firms which can support a major lobbying investment. The paper-work involved in applying for a small grant or contract may match the investment required for a major project, and small firms are thus disadvantaged.

S-J underestimate the informational economies of scale which also benefit large firms in the financial markets. Appraising the quality of a new equity issue or loan request by an unknown, small firm may be as costly as evaluating the quality of a much larger issue by a well known corporation. The bias of 'conservative' bankers against small clients thus has economic foundations. However the much more developed, private U.S. venture capital market suggests that 'conservatism' or institutional inertia, for whatever reasons, has exacerbated the problems in Europe.

S-J's main recommendation is to restrict support for small firms to successful, fast-growing enterprises. As we have argued, they do not appreciate the efficiency gains to be expected from replacing large, rent-sharing and -seeking corporations by more competitive small firms. It is true that self-employment and new firms cannot lead to full employment if macropolicy is 'too' restrictive. However, the costs of a higher failure rate in an expanded small-firm sector could be mitigated by lower unemployment and higher productivity (-growth) under more expansive macropolicy, and it is not obvious whether support for existing firms only could achieve Pareto-superior overall results with realistic policy response. Clearly, much more rigorous theoretical and empirical research in this area is required.

As an example, FitzRoy and Funke (1988) consider monopolistically competitive firms, with exogenous technical progress and a price-stabilizing monetary authority. The Nash collective bargain between unions and firms generates real wages growing at the rate of technical progress, and declining employment per firm. New entry in this model is thus crucial for even maintaining employment, and in view of the barriers to entry which past policies have enhanced, policy to encourage new entrants becomes more plausible.

Clearly there is a tradeoff between the costs of small-firms failure and the benefits in terms of efficiency, job-satisfaction and employment to be expected from increasing the share of small firm employment in any given economy. However the widely differing shares across countries reflect historical and other factors and are thus not very meaningful as exclusive explanatory variables. For example, Burda and Sachs (1987) show that wages *and* capital intensity in the service sector in West Germany have kept up with manufacturing levels, in contrast to the U.S., while service employment has grown much more slowly in Germany. This suggests a process of capital-labour substitution in German services as a response to centralized collective bargaining with uniformly high wages and declining wage differentials. S-J also fail to appreciate the remarkable powers of traditional trade associations in continental countries, which limit entry in most services by legally enforced price-fixing and training requirements which often bear no relationship to actually required skills on the job. There is a legitimate concern with the quality and safety provisions which are sometimes neglected under deregulation, as with independent truckers in the U.S., but this concern does not in any way justify the employment-reducing cartellization so prevalent in Europe.

5. Summary

S-J's book contains much careful and interesting discussion of small-firm employment data and related issues, and will be read by all workers in the field. However it also contains too much detailed quantification and international comparison of doubtful relevance, at least for most economists, and misses much background material of direct importance for evaluating the role of small firms. As a result, the authors seriously underestimate the contribution of small business to both employment and efficiency of modern economies, as well as the importance of policy to correct the traditional and still pervasive bias towards large organizations.

Notes

[1] Surprisingly, S-J do not mention Birch's widely read 1981 article in *The Public Interest*, and only refer to unpublished technical reports.

[2] See Acs and Audretsch (1987b), Acs and FitzRoy (1988), Hiller (1978), and FitzRoy (1988) for details and further references.

[3] See Piore and Sabel (1984), and Carlsson (1989) on declining plant size in manufacturing.

[4] Summarized by Schumacher's (1973) *Small Is Beautiful*. Surprisingly, neither Schumacher nor S-J refer to the much more rigorous, pioneering earlier work by Goodman (1969) on the size of organizations, although Goodman corresponded extensively with Schumacher before his book was written.

[5] Important recent exceptions in which rigorous methods are used to analyse aspects of small firm economics are Acs and Audretsch (1987b) and Brock and Evans (1986).

[6] See FitzRoy and Mueller (1984) and Williamson (1985).

[7] See the discussion of franchising in Section 4 below.

[8] See Burda and Sachs (1987), and FitzRoy (1988).

[9] Discussion of these models is provided by Brock and Evans (1986).

[10] See FitzRoy (1988) for further discussion.

[11] See FitzRoy and Kraft (1986, 1987a, b) for more details, and also Bonin and Putterman (1986), Bradley and Gelb (1983).

[12] In particular, work by Dickens and Katz (1986) and Krueger and Summers (1988) mentioned above. See also Karier (1988).

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