REVIEWS OF BOOKS

J.C. Boyajian, *Portuguese Bankers at the Court of Spain, 1626-1650*, New Brunswick, N.J., Rutgers University Press, 1983. pp. XIV, 289.

The history of European finance has seen a spate of new works recently, building on the foundations laid by earlier generations of scholars. The work under review makes effective use of this wealth of scholarly work (though neglecting the classic study by J. A. Goris) to provide the framework of its analysis. The author's archival work is impressive in range and depth, most notably in documents from the Spanish Archivo General de Simancas, where recent analytical catalogues have made financial papers much more accesssible. In addition, since most of the bankers studied were from families known as New Christians — converted in recent centuries from Judaism — the Spanish and Portuguese Inquisition took a great interest in their activities and lineages. Inquisitorial and other records have enabled the author to trace the lineage and life history of many of his bankers, providing invaluable reference for others who encounter these families in their own work.

The essence of the story is told in a straightforward fashion. Once the spice monopoly of the Portuguese crown could no longer be maintained, Portuguese merchants turned to the trade with Goa and Brazil to maintain their profits. By the 1620s they became more interested in penetrating the more secure trade of Spanish America, hoping to benefit from the union of Portugal with Spain that began in 1580. Barred from Spanish American trade by entrenched Castilian interests, the Portuguese found an outlet for their capital in the transfer payments of the Spanish monarchy, that vast flow of New World bullion that financed Spanish power abroad.

The government of Philip IV and his powerful minister the Count-Duke of Olivares welcomed Portuguese participation in the system of financial contracts (*asientos*), since they were anxious to lessen their dependence on Genoese bankers. In return the Portuguese hoped to gain a relaxation of the social restrictions that came from their status as foreigners and New Christians in Castile. They settled for profit alone in their first contracts but gained their aims in the 1630s and even achieved a notable influence within Castile, briefly overcoming tradition and entrenched interests with Olivares's patronage.

During those crucial years Spain was involved in every theatre of the Thirty Years' War. Acting on their contracts, the Portuguese transferred by letter of exchange the regular payments that were needed abroad to fund Spanish policies. In the financial markets of Europe Portuguese bankers relied on their correspondents, most of whom were relatives or close associates. They had strongest ties in Antwerp and Venice but could deal with other major financial markets when necessary. The bullion to clear the accounts followed less regularly, and for bridging the gap between paper transfers and bullion transfers the bankers earned their fees and the profits that came from the right to export bullion from Spain.

The Portuguese rebellion against Spanish rule in 1640 began the end of the Portuguese era in Spanish finance. After 1640, only a few wealthy Portuguese with naturalization in Castile remained prominent, and there was no one to protect them. Olivares fell from power in 1643, the new regime in Portugal had no reason to help them, and they fell victim to the 1647 suspension of payments by the Spanish government. Though individuals who were established in Spanish society continued to participate in crown finance, the Portuguese bankers had gone the way of the Germans, Spaniards, and Italians before them.

Evaluating the Portuguese role in European finance, Boyajian seems to claim both too much and too little. He credits them with prolonging Spanish hegemony and with the creation of the Atlantic system of payments that saw permanent exchanges in Antwerp, London, and Amsterdam replacing intermittent exchanges based on commercial fairs, such as the Genoese maintained in Besançon. Yet the shift toward the Atlantic nexus of financial power was well under way by the time the Portuguese signed their first contract with the Spanish crown. It is no accident that the system hardly took notice of the Portuguese departure from Spanish finance. From their first contract they were controlled by the needs of the crown and manipulated by Olivares, who used them for financial leverage against the Genoese and political leverage against entrenched interests in Castile. If the Portuguese and their capital had not been available, some other group would have stepped in, though perhaps on less favourable terms for the Spanish crown.

If Boyajian tends to claim too much for his bankers, he also claims too little by not placing them in their full context in the evolution of international finance. Their status as New Christians and Portuguese certainly coloured their role in Castile, yet similar modes of partnership, reliance on family networks, and ambitions to rise to the status of traditional elites characterized a wide range of merchant groups, separated in time and space. The most successful among them were on the cutting edge of economic development, moving where their business sense led them, floating on the changeable tides of politics and trade, regardless of formal nationality, and in the process contributing to Europe's economic evolution. Portuguese bankers need to be set into this larger framework. Nonetheless, it is the value of this book to have identified some of them fully for the first time and to have traced in admirable detail their methods of financing the distribution of capital for the Spanish crown.

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CH. ISSAWI, *The Economic History of Turkey 1800-1914*, Chicago and London: The University of Chicago Press, 1980, pp. xviii + 390.

The nineteenth century witnessed the impact of the Industrial Revolution around the globe. For the Ottoman Empire in Europe's immediate periphery this was above all a period of rapid integration into the world markets. Between the Napoleonic Wars and World War I, foreign trade expanded by more than ten fold. By 1913 close to 15 percent of the gross product was being exported and the value of imports approached 20 percent of the same. While exports of agricultural commodities expanded, traditional handicrafts resisted but declined under the onslaught of imported manufactures. European commercial penetration was accompanied by direct investments in railways and other infrastructure. Indiscriminate external borrowing during the mid-century led to a default and to European control over Ottoman state finances. Through the century the standards of living remained very low, but there was some economic growth at least during the decades before World War I. All of this will be best understood in terms of the complex interaction between external forces and local economic, social and political forms. The fact that the Empire never lost its political independence completely should be a reminder against underestimating the strength and resistance of local response.

In the last two decades significant strides have been taken towards establishing the contours of this economic transformation and, as is the case with most other regions of the Middle East, Charles Issawi's pioneering work should take more credit than any other scholar in the field. This book which covers the geographic area comprising present day Turkey and Northern Greece follows two earlier volumes by the author, *The Economic History of the Middle East 1800-1914* (1966) and *The Economic History of Iran*, *1800-1914* (1971). The starting point for the series was Issawi's earlier assessment that "in the present state of knowledge the best way to study the economic history of the area is through a collection of documents... This kind of preliminary work has to be carried out before a coherent history of the... economy can be written... (Consequently, this book) is neither raw material nor a fully finished product", but it is much closer to the latter.

The emphasis of this tightly packed volume is on the less accessible sources. 100 of the 126 selections which were originally written in nine different languages are being published for the first time. More than half of these were obtained from the British archives, another fifth comes from the French archives. The Western origins of most of the selections inevitably affects what is looked at from which perspective. However, it should also be pointed out that the Ottoman sources have much less to offer for the economic historiography of the nineteenth century, particularly for the period before 1880.

The organization of the book is similar to that of the volume on Iran: after an introduction, there are six chapters on social structure, external and internal trade, transport, agriculture, industry, finance and public finance, and an epilogue. Most selections are presented by an introductory note; each chapter opens with a concise and penetrating essay. The reader can not help but regret that Professor Issawi did not offer more of his insights in the form of an interpretative, concluding essay.

There is a vast amount of empirical detail here on such diverse topics as population of towns, wage levels, value and volume of foreign trade, trade with Russia, the economics of camel transport, cotton production, land tenure, emerging textile factories, domestic prices and national income levels. The uninitiated reader needs to be cautioned, however, about the quality of most statistics in this part of the world. Topics such as public finance and the external debt which have received more than their share of attention in the past are treated briefly. The strength of the book lies in its justified focus on trade, agriculture and industry.

This is not a book for cover-to-cover reading and there is a danger that the general reader will lose the forest because of the trees. But the volume should not be judged on what it does not intend to do. As a complementary volume for teaching and as the basic reference for researchers, it performs admirably. It draws attention to the wealth of empirical evidence that had remained untapped for decades, points to the gaps in our knowledge and succeeds in stimulating as many questions as it answers. For instance, at least four areas have now emerged with substantial potential for future research: establishing the macro-balances of the economy; assessing the extent and timing of economic growth; exploring the distributional consequences of rapid integration into the world markets and inserting the Ottoman-Turkish case into a comparative framework within the periphery of the nineteenth-century world economy. In conclusion, it would not be an exaggeration to say that this volume will remain indispensable for decades to researchers in this field such as the present reviewer.

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R.C.O. Matthews, C.H. Feinstein, J.C. Odling-Smee, *British Economic Growth* 1856-1973. Stanford: Stanford University Press, 1982, pp. xxiv, 712.

This volume is a thorough study of British economic growth from 1856 to 1973, Britain's second century of sustained per capita income growth. Investigation is focused on the growth of labour and capital, and the growth of output per combined labour and capital inputs. Growth rates of labour, capital, output and their productivity are metered by trend movements measured from peak to peak of the business cycle. The 117 years are broken into periods of varying length according to important shifts in the trend growth rates of the inputs, output and productivity; these periods are 1856-1873, 1873-1913, 1913-1924, 1924-1937, 1937-1951, and 1951-1973. What interests the authors is the comparative performance of these periods, not comparisons with other advanced capitalist countries.

A principal tool organizing date description and trend analysis is the neo-classical growth accounting model pioneered by Abramovitz, Solow, Kendrick and Denison. However, the authors do not feel bound by the usual supply-dominated story told with this model. A simple Keynesian IS model is employed to account for longer term demand-side variations and influences. The analysis of aggregate supply and demand trends is achieved by comparison of average growth rates. In the authors' view, theoretically more precise correlation and regression methods yield results which are too fragile. The book is analytically organized. The first half is devoted to supply-side accounting, with chapters describing and analyzing the growth of aggregate labour, capital output and factor productivity. Two chapters then study the growth of inputs, output, and productivity in the principal sectors of the economy and the growth effects of structural change. The next six chapters are devoted to changes in demand, primarily with respect to investment and international transactions. The final chapter draws together the causes and consequences of the growth of the factor inputs and their productivity, first by their supply and demand analytical categories, and then by their period to period chronology. Given the depth, detail, and rigour of their accounting and analysis, it would be impossible in a brief review to summarize accurately the authors' views on even one period. Instead, I will try to touch on certain contributions which seem most powerful and original.

What are the principal measured phenomena? *Aggregate* British output grew at a rate of around 2% per annum (hereafter, p.a.) from 1856 to 1951; from 1951 to 1973 it grew at 2.8% p.a. *Per capita aggregate output* had more of a U-shape to its growth rates, the bottom stretching from 1873 to 1924, with the recent 1951 tail higher than the 1856-73 tail. In explaining this U-shape of per capita aggregate output growth rates, the neo-classical growth accounting framework juxtaposes the growth of inputs vs. the growth of output per unit inputs. Since the latter is measured as a residual, the measurement of the inputs becomes crucial, e.g., net vs. gross capital growth, man-hours of labour vs. quality adjusted man-hours, etc. Unlike the net vs. gross capital distinction, some amount of period to period variation hinges on how labour is measured. The authors make quality adjustments for changes in age, sex, nationality, education, and the intensity of work effort (in so far as work effort compensated for the bursts of falling hours per week in the 1870s, 1910s, and 1940s). Combining gross capital and quality-adjusted labour inputs, the growth of total factor inputs does not explain much of the U-shape in the growth rate of total output per capita, 1856-1973. The bulk of the U-shape in per capita output growth rates is explained by a U-shape in the growth rates of total factor productivity (TF_QP). Perhaps the most striking finding is that there was virtually no increase in TF_QP from 1873 to 1937! "Other sources of improvements were in net offset by tendencies inimical to productivity. Some of these operated in particular sectors, such as commerce and mining, others perhaps more generally (p. 506)". After a great deal of rigorous economic analysis and counterfactual testing, the authors tentatively suggest that entrepreneurial failures, as well as difficult industrial relations, were among the most important general factors contributing to the 1873-1913 TFP growth failures.

Much of the book is devoted to studying the growth rate of the capital input. The treatment is authoritative, with savings and investment behaviour each subject to multi-variate influences and interactions. Unlike neo-classical treatments of accumulation and growth, this volume regards the accumulation of capital as a mixed consequence of both exogenous and endogenous processes. For example, the low saving rate of the interwar period is largely seen as the consequence of the period's low demand and incomes. The high domestic investment rates of the post-WWII era are viewed, to an important degree, as the outcome of accelerator mechanisms and high output growth rates. At the same time, the authors find significant autonomous investment forces at work. The low interwar domestic investment rate is influenced by the capital-saving aspects of the period's electrification while the higher post-WWII domestic investment rate is partly due to reduced imperfections in the market for corporate common stock. Most interestingly, the authors argue investor "animal spirits" had an important influence on the low investment rate of the interwar years and the high rates of the post-WWII years. Why else would cyclical upswings of the interwar period hit capacity limits so early in their expansion phases and find no such limits in the post-WWII era, despite lower post-WWII rates of return?

The authors' argument is often compelling, particularly because they are willing to perform counterfactual thought experiments explicitly, open to a variety of potential influences. On occasion, however, I wondered at their unwillingness to quantify their experiments more precisely. For example, the authors attribute much of the low interwar savings rate to low demand and incomes. There is, however, no quantitative estimate of the effect of these influences, despite years of work on what most economists consider the most stable aggregate demand behavioural nexus, the consumption (savings) function. Similarly, I would have appreciated a discussion of the econometric evidence on interwar and post-WWII accelerator investment behaviour to confirm the existence of a shift due to changes in animal spirits. My sense is that if the data are good enough for the authors' fairly complicated non-quantitative judgements, they probably are good enough to undergo more precise and explicit econometric testing.

Low levels of structural shift are often given prominent attention in analyses of British growth during these years. One of the most valuable contributions of this volume is the rigorous attempt of Chapter 9 to quantify and analyze this factor. At a high level of sectoral aggregation, the authors found that shifts in the structure of the economy had little effect on the growth rate of TFP. Whatever the degree of shift, the main sectors manifested TFP growth rates too low to permit a large effect on the aggregate TFP growth rates. The authors construct measures of factor "quality shift", essentially an index of how much labour and capital increased in the sectors with the highest wage and profit returns. Again, these measures show sectoral shift had little effect on total TFP growth; for Britain, within-sector TFP growth explains most of the total TFP growth. Still, this exercise has great value. First, the measures of "quality shift" provide a means of dating inter-sectoral changes in labour and capital inputs; old and new labour moved fastest, 1937-51, and capital, 1951-73. Second, it is hoped that growth research on other countries will provide similar measures so that some perspective can be gained on sectoral shift behaviour in growing capitalist economies. Was it faster elsewhere, as sometimes alleged? Does more rapid sectoral shift coincide with more rapid intra-sectoral growth rates than Britain or more dispersed ones? Was the depression generally a period of low sectoral shift, *contra* Schumpeter's ideas of creative destruction? Interestingly, the "new" industries of the XXth century (vehicles, chemicals, electrical engineering) did not manifest especially high rates of TFP growth in Britain. They contributed to the mild recovery of manufacturing TFP growth rates between the wars largely by their greater weight. And standardizing for their weight, their rate of shift was no greater interwar than 1873-1913.

Another contribution of this chapter is to test the Kaldor-Verdoorn hypothesis that manufacturing is particularly open to economies of scale, learning-by-doing, and other productivity enhancing effects which are induced by rapid demand growth. The authors found that demand pressure fell from 1873-1913 to 1924-1937 but TFP in manufacturing grew faster in the 1924-1937 period. Furthermore, although the growth rate of manufacturing output was the same 1924-1937 and 1951-1973, TFP growth was much faster post-WWII. They conclude that the hypothesis might hold to a small extent but it was not a major factor in any period, 1856-1973.

Finally, even as the authors were beginning their work some twenty years ago, a number of economists raised major objections to the neo-classical growth model and its accounting framework. First, the Abramovitz-Solow growth model and accounting framework was criticized for ignoring demand forces, a matter of some importance for a generation of economists raised on Keynes. Clearly, the authors have done a very able job of investigating the role of demand in the British growth process. Second, J. Robinson, L. Pasinetti and others raised major logical objections to the supply-side of the neo-classical growth model. The objections concerned certain problems with marginal productivity theory and the measurement of the aggregate capital stock. The authors have not dealt with these objections.

Alternative frameworks exist to investigate the supply-side of modern economic growth. For example, one might make an analysis of the evolving Leontief input-output and labour requirements matrices, taken in sufficiently fine detail to identify changes in underlying engineering process production functions and other influences. Or, as with many standard treatments, one might avoid any general equilibrium model and examine changing technologies, finance, labour productivity, etc., on a sector-by-sector basis, making very limited conclusions about how the pieces fit together. Given these alternatives, I would have appreciated a statement by the authors of the precise logical, evidential, or sheer economic rationale for their assumed neo-classical supply-side model.

In sum, this volume demonstrates skilful and exhaustive scholarship and it deserves a prominent place on the shelf of any student of modern economic history.

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M. Olson, *The Rise and Decline of Nations*. New Haven - London: Yale University Press, 1982, pp. xi + 273.

Forty years ago, when the time I was mis-spending might have been called youth, I contemplated a major attack upon restrictionism and what I labelled as "pressure-group economics". (Traces of that contemplation survive in occasional obscure articles, but the vision perished in a futile attempt to keep pace with disciplinary fashions of rigour in i-crossing and t-dotting.) It is from this jealousy-inspiring background that I consider Mancur Olson's venture into similar territory.

If we discount his first two chapters as a bit crabbed and pedantic, and likewise his final Chapter 7 as a bit hasty and underdeveloped, Professor Olson has done a masterly job which deserves a more profound consideration by the general public than it has yet received. He writes better than 95, or perhaps 98, percent of American economists, a percentage not quite high enough to overcome the anti-economic biases of our café intellectuals or even get him read by them. Olson the expository essayist and economic stylist is neither a Keynes nor a Galbraith, and more's the pity!

The thesis of *The Rise and Decline of Nations* has been summarized well in numerous earlier reviews, and I propose rather to translate it into the "necessary and sufficient conditions" of elementary logic. A substantial degree of freedom from pressure-group restrictionism — cutting output and-or employment to raise prices and-or incomes — is a necessary but not a sufficient condition for a country to grow rapidly. (Growth is particularly "miraculous" after restrictionism has been disestablished suddenly, as by a lost war and foreign occupation.) On the other hand, an increasing degree of pressure-group restrictionism, such as can be expected in any long period of growth and stability, is a sufficient but not a necessary condition for a country's economic stagnation or decline. (It makes no difference whether these pressure groups are called big business, big labour, big agriculture, or big bureaucracy — in any case, the fewer and the weaker the better.) It does however make a difference whether or not these groups are «encompassing". Thus one union open to 90 percent of the labour force (youth, unemployed, and minorities included) will probably be less restrictive than a set of 10 unions of 5 per cent each in separate crafts or industries — with youth, unemployed, and/or minorities excluded.

To all of this I say "Amen, but *ceteris paribus*„. By this I mean that the condition of a country's economic rivals might be taken into account more explicitly than there are taken here. Japan, for example, is (except for its weak labour movement) a highly pressure-group ridden economy; something called "excessive competition" is a dirty word there, especially in Liberal-Democratic (!) Party circles. But if compared with the U.S. or the U.K., its "miracle" of 1953-1973 fits neatly into the Olson thesis. I also think that the Western European Renaissance would have come approximately a century earlier than it did, but for the Black Death and the Hundred Years' War, and that the Tokyo-Yokohama earthquake of 1923 had rather more than any ***Zaibatsu*** malefactions to do with Japan's lack-luster economic performance in the 1920's. And at the opposite extreme, suppose that the 1972 Club of Rome analysis in *Limits to Growth*, alias "Models of Doom", were to prove basically correct. In this event, Olson-optimal countries would suffer the same general worldwide collapse as Olson-pessimal ones, and the entire Olson exercise would be irrelevant.

Some of Olson's historical illustrations may seem a trifle forced, and *ad hockery* may not always be avoided, but I profited by three insights in particular.

(1) I was subjected for a decade, on the bonny, bonny banks of Lake Mendota, to the then-current local superstition that the U.S. labour movement represented the public interest, so that parties injured by its occasional excesses need only organize on their own and exercise what Galbraith was calling "countervailing power". Since I could not handle such arguments effectively, I could have profited by Olson's "Implication 1" (pp. 37, 73) and its supporting case that no countries can "attain symmetrical organization of all groups... and optimal outcomes through comprehensive bargaining".

(2) Henry Havemeyer's dictum, "The tariff is the mother of the trusts" is well known, but I had to learn the converse from Olson's Chapter 5. To him, the major benefit of free trade is to check or at least delay the onset and advance of the pressure-group economy.

(3) I was impressed by Olson's linkage, in Chapter 6, of the caste system of Hindu India and the racism of Afrikaner South Africa, by postulating similar economic bases for both of them in connection with the strengthening of pressure groups. I know too little about the economic history of either India or South Africa, however, to do more than hope that Olson's analysis is correct.

At the same time, the Olson analysis brings up, but I think dodges, a major issue of equity and economic ethics which I rather wish he had tackled head on. Assume that I was correct in stating, a few paragraphs back, that apart from its weak labour movement, Japan is a pressure-group economy not greatly different from the U.S. or the U.K. It would follow by the logic of Olson's argument that the "Japanese miracle" is largely explicable by the weakness of its trade unions. The question then arises, should the Japanese unions be encouraged to become more aggressive, to rise and shine? Considerations of symmetry and conventional equity suggest that they should, and such is the burden of much current (1984) anti-Japanese propaganda. But considerations of efficiency suggest the contrary. Olson mentions the late Arthur Okun's *Big Trade-Off* between equality and efficiency, but he hesitates to apply it to such specific problems as this one.

The ideological threat of Olson's argument is clearly conservative, but his worship of free markets and *laissez-faire* is less marked than his disdain for the interferences of collective groups. There is not enough medicine in the market mechanism to check automatically the monopolistic and monop-sonistic activities which it tolerates in the short term, or to keep Adam Smith's "people in the same trade" from meeting together for "merriment and diversion", including conversation ending in "a conspiracy against the public, or in some contrivance to raise prices". Presumably for this reason, Olson favours in Chapter 7 tax-based incomes policies (TIPs) to end persistent inflation with less risk of depression and unemployment than be associates with fiscal and monetary tightness. His goal is full employment without inflation, and his policies might impede rapacious trade unions, trade associations, etc. At the same time, they would (in my view) increase greatly the regulative and supervisory powers of the administrative bureaucracy, if enforced over any protracted period. Olson's final chapter does indeed attempt to reconcile his policy preference with the earlier chapters of his book. I do not think the reconciliation quite comes off, but Olson sees the difficulty and tackles it boldly.

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J.K.J. THOMSON, *Clermont-de-Lodève, 1633-1789: Fluctuations in the Prosperity of a Languedocian Cloth-Making Town* (Cambridge: Cambridge University Press, 1982), xii, 502 pp.

The title of this splendid monograph does not do justice to its importance and relevance to the larger questions in French and even European economic development. It is much more than a chronological treatment of the vicissitudes of the wool industry in a hill-town in southern France in the Old Regime. As a step-by-step analysis of a pre-industrial cycle extending over more than a century, J.K.J. Thomson's study raises economic questions and proposes answers that are applicable to France as a whole. Although I am not convinced by all of his conclusions, there is no question about the quality of Thomson's research and the astute blending of rich local sources with comparable materials discussed in the extensive secondary literature about growth by French, English, German, and North American economic historians. The local materials include not only the copious administrative surveys and correspondence provided by an efficient central bureaucracy, but also private papers and notarial contracts that make it possible for the author to track down three-quarters (26 of 35) of his woollen clothiers in considerable detail. These in-depth case studies provide a solid basic for Thomson's "qualitative" interpretations and for his emphasis on entrepreneurship as a crucial factor in the economic cycle. In addition, for a social historian like myself, the immediacy and credibitity of these families — the Galtié and Martin, Desalasc and Pelletan, Flottes and Berthomieu — bring a special bonus, not as part of a process of isolating "factors" and "causes", but, quite to the contrary, in elaborating nuance and demonstrating human variation in a group that cannot be reduced to economic robots whose actions are subject to numerical manipulation.

Thomson describes a pre-industrial cycle which fails to alter economic or social structures, but nonetheless exemplifies a creative short-term response to technical and organizational challenges and market conditions. Stopping short of a direct attack on the reigning orthodoxy of French economic history, namely, that the traditional Old-Regime cycle must be understood in terms of the absolute "Malthusian" limits of demand and of production techniques, Thomson still makes place for "voluntarism" in the form of entrepreneurial talents and government intervention. He even identifies an "event" — the horror of most *Annalistes* — as the spark igniting the new cycle. In 1693 an Anglo-Dutch convoy of 400 ships laden with cloth for the Levant was destroyed by the French fleet, giving the clothiers of Languedoc an unexpected four-year monopoly of the woollen trade in the Near East. They were able to make good on this temporary *entrée* by producing woollen exports of sufficient quality to compete successfully with Dutch and English rivals for the next sixty years. Colbertian quality controls, privileged pilot manufactures, training of specialized wage-labour, the formation of a new type of entrepreneur, the *marchand-fabricant*, and a quota system to prevent overproduction — all these held the Levant market and produced an elite of clothiers far different from their predecessors in the hill-towns of seventeenth-century Languedoc. Colbertism, the bane of all free-traders, had prepared French industry for new marked opportunities. And the French entrepreneur responded and performed well within that regulatory framework.

At least for two generations. But, says Thomson, the Colbertian injection had unfortunate long-run effects. After 1725, regulation became ever tighter, tied Languedoc textiles to a single market, alienated labour (or the least-skilled part of it, weavers and cloth-finishers), prevented new blood from entering the entrepreneurial elite, and eroded the "primitive energy" of the first generation of clothiers. By 1755 the entrepreneur of Clermont, Carcassonne, or Saint-Chinian (the three privileged towns) was not the same resilient capitalist he had been in 1695 or 1725. Then came the new ideology of *laissez-faire*, imported from England, and transformed into policy by Vincent de Gournay and the physiocrats. The results were disastrous for the Languedoc cloth industry. As regulations were dismantled and ignored, a mass of newcomers, usually undercapitalized, entered the export trade, overproduced cheap cloth, and replaced the old elite of clothiers. The quality of French cloth fell dramatically at a time when competition from England and even from Central Europe raised a new challenge in the Levant. The outbreak of the Seven Years' War cut the French off entirely from the Levant market; the British Navy had its revenge for 1693. The cycle was over. In the last half of the eighteenth century Languedoc textiles largely reverted to what they had been before 1690. The scale of operations was sharply reduced; labour shifted from textiles to the land or the vine; the number of *marchands-fabricants* rapidly declined and most of those who did not go bankrupt wisely bought land, offices, and *rentes* in the time-honored continental manner.

This rapid summary cannot do justice to the care with which this process is examined. The argument is never lost in the richness of detail and comparative examples from other areas of France and other countries are employed to reinforce each conclusion. My only quarrel is with Thomson's harsh final indictment of Colbertism. Near the end of his study, the author writes:

"Greater, not lesser, competitiveness between entrepreneurs was necessary, and, in that the major impulsion for this came from the domestic economy, a generalized economic expansion, increase in well-being, and conversion to a consumer economy was required" (458).

This may well be the chief characteristic of the new English economy that we associate with the Industrial Revolution of the late eighteenth century. But it seems eons away from the economic and social possibilities of Languedoc in 1700 or 1750 or even 1800. Let us not forget the limited-demand structure within France. Given this obstacle, is it fair to conclude that the clothiers of Clermont were "overcommitted" to the Levant market? Was there any other? In any case, granting the risks and dangers of managerial routinization, the Colbertian solution, a mix of government guidance and individual entrepreneurship, was an economic success from 1695 to 1755. Further pursued and properly "fine-tuned", Colbertism might have even turned such cultural determinants as the "Buddenbrooks effect" to economic advantage by making cloth-making "privileged" and hence respectable. Greed is not always enough, nor is competitiveness, in those parts of the world where elites consider wealth as merely instrumental.

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R.W. Tucker, D.C. Hendrickson, *The Fall of the First British Empire. Origins of the War of American Independence*, Baltimore - London: The John Hopkins University Press, 1982, pp. xii + 450.

Following the Peace of Paris in 1763, the First British Empire reached its zenith. Within a dozen years, however, the British Empire began to collapse as war broke out in the North American colonies. In this carefully researched book Tucker and Hendrickson re-examine the origins of the American War for Independence. Rather than repackaging a variety of textbook explanations, the authors in an ambitious effort provide a new framework of analysis which is used to shed light on this major historical issue. They point out that the bulk of current historiography assumes the emergence of a new attitude toward empire in Britain following the Seven Years' War which in turn led to policies and actions inevitably leading to conflict with the colonies. Tucker and Hendrickson challenge the explanations derived from the Eurocentric focus of the standard literature by arguing that the collapse was the consequence of a series of profound upheavals on the empire's periphery and not changing attitudes in Britain.

One of the many strengths of this work is the authors' demonstration of how changing circumstances led to changes in the expectations of the players at the periphery of the empire. One of the most significant events in this respect was the outcome of the Seven Years' War. With the removal of the French and Spanish threat to the colonies, the Americans began to reinterpret their position within the British Empire in terms conformable to their immediate interests. Colonial spokesmen, particularly Franklin and Paine, now perceived the Great War for Empire as one fought for British and not colonial interests. This in turn changed the American's perception of the French and the Spanish. In the new view both of the European countries had fought in the war not as enemies of the Americans but as enemies of the subjects of Britain. The outcome of the war also fundamentally altered French expectations concerning the role North America would play in the balance of power in Europe. Prior to the war, France had viewed colonial interests as inseparable from British interests. Accordingly, fighting with colonists was compatible with the French desire to strike hard at the British. Sensing a growing divergence between British and American interests following the war, France realized that by supporting the colonists it could once again strike back at Britain. According to the authors the French having been beaten at one game would now play another. For the Americans, the growing awareness of their increasing importance in the European balance of power served to stimulate a desire to establish a relationship with Britain on an equal footing. The authors also attribute great importance to the impact the Stamp Act crises had upon American expectations. It conveyed to the colonists a confidence in dealing with the British which they never before possessed and would never lose. The lesson learned was that the authority of the imperial state could be openly and successfully defined.

To a large extent this book is about a theme common in revolutions — perception versus reality. Over and over again Tucker and Hendrickson report on this problem. One very important example was the British perception that the colonies were almost indispensable to its industry, commerce, and security. "The expectations of a rupture with the colonies", Adam Smith wrote, "struck the people of Great Britain with more terror than they ever felt for a Spanish armada, or a French invasion" (p. 42). According to the authors this perception explains to a very large extent British policies toward both the Americans and the French.

In this book Tucker and Hendrickson have made a major contribution to furthering our understanding of the origins of the American War for

Independence. They have done this by refocusing our attention from the "core" to the "periphery" of the British Empire. The study is extremely well written and has been carefully researched. It is a first rate critique of the current historiography on the decline of the First British Empire.

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J.H. Weiss, *The Making of Technological Man, The Social Origins of French Engineering Education*, The Mit Press, Cambridge and London, 1982, 377 pp.

Under this somewhat mysterious and anonymous title, the author has attempted to cover the two first decades of the Ecole Centrale des Arts et Manufactures, founded in 1829, to fill a gap in the training of the French engineers for the new industries, mainly metallurgical, chemical and textile. Unlike the British who had not been interested to train people in the new techniques, the French had already started with the foundation of the Ecole des Ponts et Chaussées en 1747 (bridges and roads), and later on with the mining schools (Ecoles des mines). But bridges, roads and mines were all under the supervision of the state, which left a vacuum in the field of manufacturing industries, especially at a time when the impulse came from England. The French Revolution had been deeply involved in building a new system of education, an important part of which was the creation of the Ecole Polytechnique, the name of which recalls its aims, to develop technology. But it shifted to other directions, for Napoleon turned it in a military school devoted to the application of the new techniques to warfare and also to mathematical abstractions which had nothing to do with the practical knowledge of industry. At a lower level, the Ecoles des Arts et Métiers were engaged in training foremen, but not engineers.

So there existed a gap in the training of "technological men" for the practical needs of the new industries, and this was precisely what the Ecole Centrale was supposed to do. Some contemporaries challenged this purpose, taking for their example England, where technicians were trained on the spot. The answer of the artisans of this school was that the French "genius" was different from the English one, and in order to close the gap between the two countries, it was absolutely necessary to give a formal training. This debate about the necessity of creating such a school helps to explain why the Ecole Centrale was started as a private venture, without any participation of the state, which differentiates it from the Polytechnique. The students had to pay rather high fees once admitted, and the faculty was not a full-time one, which had many obvious advantages, especially that of hiring prominent scientists, such as J.-B. Dumas, Colladon or Péclet. But, also, the recruitment of students was socially different from other schools, usually confined to the sons of the middle class, professionals or already established industrialists. It did not really open the industrial career to new layers of society.

From the beginning, the curriculum was geared to practical studies and technological experimentation, with its emphasis on physics and chemistry, including laboratory exercises, factory visits, problem sets, experiments, drawing and design. To assure good standards, a rigorous policy of admission was decided, limiting to about 120 the number of students selected every year. The new techniques, such as the railroads, were at once taught by such distinguished men as Auguste Perdonnet. On the whole, the founders of Centrale were successful in developing the industrial system, but there were also limitations which are stressed by the author. One was the absence of any social concern, since the Saint-Simonians, who had been insisting on the links between the economy and the society, had been entirely ignored and exerted no influence on the curriculum. The school had been framed by scientists and industrialists, to the exclusion of social thinkers or positivist philosophers. Another weakness was the lack of any teaching in political economy, with its consequences, the total ignorance of the economic environment, at a time when economic crises disturbed periodically the rhythm of industrial production. The narrowness of the culture given by the Ecole Centrale remains surprising, even in the technological field which had been chosen, according to the author.

In many ways, these conclusions are a little distorted. Political economy appeared very late in the curriculum of French institutions of higher education, and was in fact confined to the Collège de France with Auguste Blanqui and Michel Chevalier, a former Saint-Simonian, until almost the end of the XIXth century. It was even worse for the study of society, entirely ignored during the same century. The Ecole Centrale, developed as an institution devoted to applied technology, succeeded in this field and fulfilled the aims of its founders, to substitute industrial competence for industrial amateurism.

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