

---

## REVIEWS OF BOOKS

---

R. AMINZADE, *Class, Politics, and Early Industrial Capitalism. A Study of Mid-Nineteenth-Century Toulouse, France*, Albany, State University of New York Press, 1981.

This could be quite an interesting book, but it reads dully for anyone less than fascinated by Marxist theorizing. Dr. Aminzade has a good subject and knows a good deal about it, but his writing is turgid and he never uses one word when several will do. That is a pity, because he is interested in the sources of collective action at social levels often discussed but seldom plumbed, about which it would be fascinating to know more. And, in fact, behind the clichés and the leaden style, the persistent reader can still find information — not exactly new, but useful in fleshing out assumptions that historians have already made about the difference between the relatively more conservative politics of France's XIXth century countryside, and the more advanced political stance of its urban centres, small and large.

Aminzade investigates a large provincial capital, Toulouse, where the patronage, and hence the power, of landed nobility soon declines; where "industrial capitalists" are practically non-existent throughout the century; and where merchants and professionals vie for national or municipal office with representatives of the lower and middle orders: small shopkeepers, tavern keepers, and master artisans. As the century advances, the "working class" — a rather inclusive term for most nonagricultural wage labourers — is increasingly mobilized to assume an ever more active role in municipal and national politics. Its growing political weight is shown in the part it plays in the dissolution of an early Republican-Legitimist alliance against Orleanism, in the divisions of

the Republican camp under the Second Republic, in the reddening tinge of republicanism as the Second Empire draws to a close, finally in the short-lived and bloodless Commune of Toulouse in the spring of 1871.

Aminzade starts off promisingly, when he considers the networks of patronage on which Legitimist dominance briefly rested, but he does not go on to apply the same suggestive speculation to the evolution of republicanism or of social republicanism. He does not attempt to locate any networks of family alliance or hostility, crucial in local politics. The details he provides confirm the increasingly active role of skilled and unskilled workmen on critical occasions after 1848, but leave us wondering about their relative weight, and just who provided the leadership on most of these occasions. The general facts are too often skewed or out of context. Thus, around mid-century, capitalist production did not define the context of the French economy, peasant producers were not being expropriated, anonymous investors were not numerous, the organization of credit was far from systematic; manufactures were not particularly more efficient than handicrafts – they just used cheaper labour; it was not greater capital concentration in factory production that made workers' access to property unlikely, but the precariousness of their estate; it was not the bourgeois fear of socialism that plunged the nation into economic crisis in early 1848, when the crisis had been there for some time; and so on.

Aminzade blames the Republican government of 1848 for acting like the moderate bourgeois they were, and for not adopting socialist policies, or at least Keynesian ones: a fine example of anachronism in action. He also frowns on police surveillance of cabarets and cafés under the Second Empire, as part of action against the working class. I suppose that I show my colors when I express doubt about the applicability of the singular, "class", to a population so diverse. More important, it goes without saying that restrictions imposed on drinking places were also, perhaps mostly, political in inspiration, just as their liberation would be in 1881. But a negative attitude to dram shops was also based on economic and hygienic reservations. The poor man's search for solace and escape from horrid conditions is understandable. Yet no friend of the poor stops long to ask the workmen's wives just what they thought about the costly conviviality of *assomoirs*. I mention this because it illustrates opportunities missed by conventional treatment of complex situations that can yield food for thought if examined more searchingly. Nor is it enough to show the sharpening or acceleration of workers' political activities, without comparisons to put them in perspective.

A Dublin story tells of the stranger asking his way to Trinity College, only to be told: "If I were you, I wouldn't start from here". I'd say as much about this book.

EUGEN WEBER

University of California, Los Angeles

M. AYMARD (ed.), *Dutch Capitalism and World Capitalism*, Cambridge: Cambridge University Press, 1982, published for the Fondation de la Maison des Sciences de l'Homme, pp. 312 + vii.

There are certain problems in economic history which will continue to fascinate professionals as well as amateurs regardless of how much is written about them. One of those is the miraculous rise of the Dutch United Provinces from a primitive little backwater to a prominent, wealthy, powerful empire, a cultural and technological Mecca of the West, and the first Welfare State in history. Equally interesting is the subsequent decline of the United Provinces back to a relatively modest place among the European nations. Interestingly enough, the questions posed by the "Dutch Miracle" have attracted the attention of foreign scholars as much as that of the Dutch themselves. The collection under review here is a case in point. Most of the eleven papers in this volume are by French or American scholars and to judge from their content, it seems quite clear that much interesting work on this subject is carried out by foreigners.

Unfortunately, this particular volume is not an especially valuable addition to this debate. It contains the proceedings of a conference held six years prior to its publication in Paris. Eleven papers plus the minutes of the discussions are reproduced. Although the volume clearly demonstrates that a good time was had by all, this reviewer fails to see why libraries or individuals should pay close to forty dollars to relive the event. Part of the failure of this collection is the failure of most conference proceedings: the papers are of uneven quality and depth. Some are expositions at the level of undergraduate textbooks, others are serious research papers replete with documentation and extensive bibliographies. But most of the fault lies with the central idea around which the conference was organized, namely, a discussion of the Dutch Miracle as part of the emergence of a so-called "World System". The explanation of that peculiar emphasis is found in the fact that in 1976 one of the most widely debated books on early modern Europe was Immanuel Wallerstein's *The World System* (1974). By now, Wallerstein's "theories" have proved to be the flash in the pan that some historians had predicted all along, and the six years' delay in the publication of the present collection make much of the discussions (as recorded in the minutes) sound quaintly old-hat.

It is neither possible nor necessary to criticize Wallerstein's broad interpretations of history here. The sequel to the above-mentioned volume has appeared in 1980, and many reviewers have pointed out the fallacies of fact and interpretation with which his work is studded. Some of them are pointed out in two contributions to this collection, by Jan de Vries and P.W. Klein. Wallerstein's own contribution to the conference consists of a chapter from the second volume of his *World System*. The uninitiated can see at a glance the strengths and weaknesses of Wallerstein's work. It is this sort of writing that will give the use

of footnotes a bad name. In less than thirty pages he uses up almost two hundred of them and his list of references alone is as long as some of the other papers. But there is less there than meets the eye. The sources are largely textbooks and well-known articles in English and French. Moreover, Wallerstein's understanding of economics is pitiable. One example: on p. 111 (pp. 57-58 in his *World System* Volume II) Wallerstein points to the financial soundness of the United Provinces government, which managed to avoid the defaults of the Habsburgs and the Bourbons. This is explained by him by the fact that commercial advantages in shipping created a surplus in the balance of payments, allowing the United Provinces to "balance a deficit on current account with incoming financial flows". It is possible, of course, that beneath this reasoning lies a revolutionary new model developed by Wallerstein (and withheld by him, mysteriously, from publication) which provides a direct link between the balance of payments and the government's cash flow. It is possible, perhaps, to think of various ways in which such a model could operate, but Wallerstein never makes the nexus explicit, and one cannot suppress the disturbing thought that the intellectual father of the World System has confounded the current account with the budget of the central government. For the rest of his discussion of the Rise of Holland, Wallerstein has extended the tripartite division of his World System (core; semi-periphery, periphery) to include a fourth element, a hegemony, which is something like a supercore. Holland, in this view, was a hegemony. Using this framework, he proceeds to paraphrase most of what is well-known about the Dutch in this period without adding any novel insight.

The further away the papers in this collection get from "World Capitalism", the better they generally are. The most useful part of the book is the third section entitled "Dutch Capitalism and Asia". In it, Niels Steengaard presents an interesting account of the formation of the Dutch East India Company, one of the earliest examples of a large joint-stock company with separated management and ownership. Denys Lombard, in a fascinating paper, provides a good example of the irrelevance of Wallerstein's "empty sociological boxes". He demonstrates that the impact of the Dutch colonizers on the East Indian islands they controlled was slight before the middle of nineteenth century. The technology of political hegemony and economic exploitation used by the Dutch was a direct continuation of the methods used by Moslem and Indian traders before them. So much for a classical "core-periphery relation".

Despite its title, this collection does not deal with capitalism. Its focus is around the economic rise of the United Provinces. Michel Morineau, in the closing paper, points this out and clearly believes that capitalism is not even a useful concept here. Perhaps. More to the point, Morineau accuses something he vaguely defines as "modern historiography" (Wallerstein?) as failing to come up with explanations of the "Dutch Miracle". Maybe he should have listened a bit more attentively to B.H. Slicher Van Bath's paper given earlier at the conference, who succinctly points to four or five central factors in Holland's

prosperity. After producing some interesting but essentially irrelevant back-of-the-envelope calculations on the Dutch current account in 1667, he himself can do no better than to note that economic development was accomplished by the hands and brains of people, admonish us to stay away from monocausal explanations, and suggest that the explanations are to be found in grasping the interaction between potentialities and activities (p. 303). Whatever that means.

JOEL MOKYR

Northwestern University

R. W. FERRIER, *The History of The British Petroleum Company. The Development Years 1901-1932*. Volume 1. Cambridge, Eng., Cambridge University Press, 1982, pp. xxx, 801.

The history of the British Petroleum Company (BP) has long been a mystery to students of the oil industry. The rough outlines are well known, but not the details. There was one poor historical study. Now, however, we have a feast. Volume I, on 1901-1932, has 634 pages of text, plus another 140 pages of appendices and notes; Ferrier promises two more volumes.

In June, 1901, the Shah of Persia granted a wealthy Englishman, William Knox D'Arcy (who had made his money in gold mining in Australia) a concession for oil exploration in practically all of Persia. D'Arcy's deputies handled the negotiations; the investor never visited that "socially disturbed, politically troubled, economically deprived" country. By 1905, when Burmah Oil and Lord Strathcona invested, D'Arcy had spent £ 225,000 and his personal resources were near exhaustion.

On May 26, 1908, oil was struck. In 1909, the predecessor of BP, the Anglo-Persian Oil Company, Ltd. (APOC) was formed to develop the concession; Lord Strathcona was chairman; D'Arcy and Burmah Oil representatives were on the board; the next year, Charles Greenway became managing director. The company then employed about 2,500 people. While D'Arcy continued as a director (up to his death in 1917), Greenway was the principal executive, and would be so until his retirement in 1927.

Under Greenway's administration, the Persian oil properties were developed. At first, APOC used a managing agent, with poor results; also Burmah Oil's chemists were unfamiliar with the properties of Persian oil so they were of little help. The Abadan (Persia) refinery construction took far longer than anyone could anticipate, not coming on stream until 1913. Under an October 1912 ten-year contract Anglo-Persian sold crude oil, kerosine and benzine to the Asiatic Petroleum Company (part of the Royal Dutch-Shell group), while at the same time Greenway worried that Royal Dutch-Shell "do their best to smash us". Greenway was *not* appreciative when Waley Cohen of Shell offered, in August 1912, to assist APOC in raising capital.

Greenway was, however, desperate for added financial resources for APOC and in October 1912 turned to the British government for aid. The British Navy, converting to fuel oil, wanted to control its source of supply. When Greenway sought British government help, he denigrated Royal Dutch-Shell and warned lest that "foreign" combines gain control over "British oil". Finally in February, 1914, the British cabinet (spurred to action by Winston Churchill) agreed that for roughly £2 million the government would acquire a controlling interest in Anglo-Persian Oil Co.; Parliament gave its assent in June. The outstanding ordinary shares of APOC were tripled; in the process, the British government obtained a 66.7 per cent interest; Burmah Oil's ownership dropped from 97 to 32.3 per cent; while the Strathcona estate held the remaining one per cent.

Gradually, the British government's role took form, a consequence of a series of compromises. Greenway, as managing director (located in London), set his own course. He was far from a puppet of the British government. Likewise, he was not subservient to Burmah Oil: thus for example, Burmah Oil's relations with Shell were far closer than APOC's. Greenway seems to have disliked Marcus Samuel of Shell. While Greenway did make agreements with Shell, he never wavered in his determination to make APOC a rival of the latter and to make his company a fully integrated enterprise.

During World War I, APOC moved from having purely Persian operations (oil production and refining) to acquiring tankers, buying in 1917 the British Petroleum Company (a marketing firm that had been in German hands before the war), and setting up a research department in Britain. Finally in 1916-1917, APOC was making profits.

When the war was over, APOC's growth in Persia accelerated. By 1930, the company employed 31,246 people in that country, two-thirds of them Persians, 1,191 Europeans, 2411 Indians, and 7,549 "others". When its contract with the managing agents in Persia had expired at the end of 1922, APOC took over direct management of its Persian operations. As APOC's capital needs had risen, the British public was offered shares; in 1923, the British government owned 55.9 per cent, Burmah Oil 26.5 per cent, while the public held 17.6 per cent of the shares outstanding. During the 1920s, the Company developed its own selling organization in the United Kingdom, Europe, and elsewhere. Its personnel explored for oil in many parts of the World, with little success. It remained dependent on Persian oil, albeit not simply one Persian field.

John Cadman, in 1927, followed Greenway as the company's chairman. Thereafter, Cadman, a former professor, government petroleum consultant, and since 1923 an APOC director, would (until his death in 1941) be the principal figure in APOC. He was a man of stature, concerned with matters of management, budgetary controls, as well as technical efficiency. Moreover, he did not harbour Greenway's antagonism toward Shell. Under Cadman's leadership, APOC participated amicably with Shell in the Turkish Petroleum Company. In September, 1928, with the Achnacarry Agreement, Standard Oil of N.J.,

Royal Dutch-Shell, and Anglo-Persian Oil Company agreed to divide world markets. Ferrier postpones consideration of the accord and its consequences to Volume II.

APOC's main business was in Persia, and Cadman visited that country in 1924, 1926, 1929, and 1931. In 1922, a representative in Persia was writing:

“What we wish the Persian Government to realise... is that this [the Company] is the goose that lays the golden eggs and the only laying goose that they have in the country, that the way to stimulate it to lay more eggs is not by holding on to its throat and strangling its progress, but by feeding and protecting it” (p. 388).

The company saw the Soviet Union as stimulating hostility to it in Persia. Very early, the company had disputes with the Persian government. While the concession contract specified the percentage of profits to go to the Persian government, it did not prescribe how the “profits” were to be determined. As the Company became more than a strictly Persian producer and refiner, and as it became an integrated enterprise, it felt it should pay the Persian government only on its Persian and not on its worldwide profits.

Not long after Riza Khan came to power in Persia in the mid-1920s, Cadman thought it opportune to seek to extend the APOC concession, to create a clearer basis for payments to be made the Persian government (based on tons produced rather than profits), and to reduce the company's vulnerability to political pressures. Concession renegotiations were lengthy, but then came the world depression and fall in world oil prices that limited APOC's flexibility and sharply reduced the Persian government revenues. On November 26, 1932, the Shah cancelled the D'Arcy concession. This ends Volume I.

Ferrier gives rich and necessary detail. His presentation is balanced and based almost exclusively on original documents in British Petroleum Company files. The book has excellent maps for the novice on Persian geography. I would have liked a little more on the remarkable Lord Strathcona, the first chairman of APOC, who at 89 years of age assumed that position. Ferrier explains that he provides only minimal information on Burmah Oil, because of a work in progress by T.A.B. Corley. The reader does not learn when Anglo-Persian grew larger than its parent (in assets, employment) and what part of Burmah Oil's profit came from Anglo-Persian. We must wait for Corley's book. Ferrier frequently notes restrictions in the 1912 APOC-Shell contract, but never makes them explicit; it is not clear exactly what happened when in 1922 this ten-year contract was not renewed. Likewise, the January 31, 1928, the “Heads of Agreement” between Henri Deterding of Royal Dutch-Shell and Cadman is mentioned, but with no details (was this for eastern markets only?). The Appendix would have been enriched by an abstract of the 1914 Agreement on the British government purchase of APOC shares. The book could have contained a little more on Mesopotamia. I found it interesting that

APOC, while it searched for oil worldwide in the 1920s, never (or Ferrier never mentions it) explored in the United States. Shell, by contrast, had large American operations. The reasons can be guessed at, yet might have been made explicit. These additions are not, however, absolutely essential, and this excellent volume explains practically all of the former mystery. For example, the reader learns the precise role of D'Arcy. Likewise, Ferrier is splendid on the behaviour of the British government as the major stockholder in APOC. Greenway and Cadman are well-defined as business leaders. So too, the Persian background is effectively elaborated. I liked this book very much and recommend it to all students of business history, government-business relations, and of the oil industry. I look forward with pleasure to the 1,600 pages to come.

MIRA WILKINS

Florida International University

W.D. KAMPHOEFNER, *Westfalen in der Neuen Welt. Eine Sozialgeschichte der Auswanderung im 19. Jahrhundert* (Beiträge zur Volkskultur in Nordwestdeutschland Heft 26). Münster: F Coppenrath Verlag, 1982, VIII+ 211pp.

The book cover shows a photograph of the author's greatgrandparents. His greatgrandfather emigrated from Westphalia to Missouri in 1844. Motivated by digging out his own roots the author achieves a model study on the causes of migration from Europe to the United States during the XIXth century.

His exemplary approach fulfills what has often been demanded by scholars like Marcus Lee Hansen and Frank Thistlethwaite, that is, to study emigration by examining the migrants' social, economic, and cultural situation in Europe and later in the United States.

The author concentrates on the hitherto neglected emigrants from the north-western part of Germany, in particular on the Prussian administrative districts of Münster and Minden, on the Hanoverian area around Osnabrück, on parts of the Grand Duchy of Oldenburg, and on Brunswick. Between the 1830s and the 1860s the emigration intensity here was among the highest in Germany. A centre of the emigrants' settlement in America was rural Missouri. For his analysis, the author chooses the two adjacent counties of St. Charles and Warren.

Information about other areas on both sides of the Atlantic is used frequently to control and supplement the findings. In his dominantly quantitative approach Kamphoefner draws on list of emigrants, regional economic and demographic data, and on the original manuscript returns of the Seventh (1850) and Eighth (1860) Census of the United States. In combining American and German sources Kamphoefner is able to study the social mobility of a representative sample of 129 individual families.

First he focusses on the causes of emigration. He argues that pull-factors were constantly at work, except during the Civil War and severe depression

years. Push-factors in Europe, however, varied from year to year and more importantly from region to region and even from village to village. Thus Kamphoefner tries to find out by which specific regional economic and demographic characteristics people were pushed into emigration. An inheritance system with the division of real estate among the heirs has often been set forth as a main cause for emigration in south-western Germany. The North-West, however, with its comparable emigration intensity had the completely different system of *Anerbenrecht* (one heir to the farm).

In search of a valid explanation Kamphoefner runs a number of cross-sectional correlations between emigration-intensity and socio-economic data. The main result is that protoindustrial regions produced the highest emigration rates. The combination of a rural linen industry with the widespread *Heuerlings-system* (a *Heuerling* was the tenant of a very small plot of land, roughly 3 acres) had led to an enormous growth of the rural lower-class population in past centuries. The protoindustrial system collapsed in the second third of the XIXth century due to the ever-growing dominance of factory-produced cotton fabrics on world markets. Thus this lower-class population was pushed into emigration. To avoid the trap of an ecological fallacy Kamphoefner controls these findings by using personal social data from emigration lists. And indeed, greater part of the emigrants were rural lower class people dependant on proto-industrial activities.

Kamphoefner goes a step further when he claims that in these regions social tension, class conflicts and finally the peak of social conflict in 1848 were not the cause of emigration but rather the outcome of the same event, namely the breakdown of the proto-industrial system.

Chain migration led to a high regional concentration of migrants from the same geographical origin. Missouri was a preferred destination for people from north-western Germany. Three quarters of the Germans in the two analyzed Missouri counties came from a German region with a radius of only 50 kilometres. They settled in stable communities and married almost exclusively within their ethnic group. It was Westphalia in the New World. This meant no uprooting and of course no assimilation with other ethnic groups.

The group as a whole was to Americanize gradually in the course of two or three generations. The 129 families identified by personal, social and economic data on both sides of the Atlantic reveal a clear upward social mobility. Though there was no jump from "rags to riches" there was one from "rags to respectability". The formerly landless poor emigrants became land-owning farmers. So the American dream came true for lower class people pushed out of north-western Germany.

I look forward to seeing this excellent study published in the language of the country of immigration.

RAINER FREMDLING  
Freie Universitat, Berlin

H. KISCH *Die hausindustriellen Textilgewerbe am Niederrhein vor der industriellen Revolution. Von der ursprünglichen zur kapitalistischen Akkumulation*, Göttingen, Vandenhoeck & Ruprecht, 1981 pp. 373.

This study of the textile industry in the lower Rhine valley before the Industrial Revolution was published shortly after the author's death in 1978 at Michigan. The text has been re-translated into German from the original English essays which had been published (those comprising chapters 2-5) in various economic history reviews, including the *Journal of European Economic History*. These include the author's most recent research on the textile industry in the lower Rhine region in the period after 1815, which was incomplete at the time of his death.

This final volume of Kisch's studies includes a valuable short introduction by Richard Tilly (who together with other colleagues organized the publication) describing the author's life and work. Kisch was born in Prague in 1924 and became a naturalized American citizen, and Tilly describes the breadth of his interests, his political attitudes and particularly his views on Marxism, and suggests that there were four principal themes running through his work. Firstly he was concerned to develop the study of industrialization at a regional level; secondly, he devoted particular attention to the problems posed by the chronology and dating of industrialization in Germany; thirdly, he was interested in analysing the nature of the capitalist structures in German industry; fourthly a more general study of the bases of modern social and economic change. The fundamental problem running through all these different perspectives was the relationship between industrialization and capital accumulation. And, as the present volume demonstrates, one area in which this problem could be studied particularly well is provided by what has become known as "proto-industrialization".

The book consists of five chapters which provide: a general overview of the textile industry in economic history; the origins of the Krefeld silk industry in the XVIIIth century and Prussian mercantilism; the Wuppertal industry from the guild system to *laissez-faire*; cloth production at Aachen before 1790; an analysis of the impact of the French Revolution on textile production in the lower Rhine region. Using the examples of the textile industries of the region — those of Krefeld, Wuppertal and Aachen — Kisch develops a more general argument on the nature and significance of the process of proto-industrialization which emerges from the specific themes treated in the chapters described above. The French Revolution was finally to play an important role in the growth of the textile industries of the lower Rhineland, which benefited from the decline and stasis of their neighbouring French competitors.

Kisch argues that there were a number of specific features in the process of proto-industrialization, and claims that rather than constituting an earlier form of industrial revolution it was itself the first phase of the Industrial Revolution. At the same time, like the Industrial Revolution, proto-industriali-

zation had its own particular patterns of development, and the wider processes of industrial growth were largely dependent on these.

While it could be said that the examples studied here are too narrow to permit generalizations of this type, it should be remembered that they formed only a part of Kisch's wider researches. It is also worth remembering that at the recent 8th International Congress of Economic Historians (Budapest 1982) a whole section was devoted to the problem of proto-industrialization, and that the conclusions reached in those discussions — which established that proto-industrialization provided the mechanisms for triggering off wider processes of industrial growth — generally confirm Kisch's own theoretical arguments on the significance of proto-industries. At the same time there were many at Budapest who had reservations about the unduly mechanistic relationship inherent in such a theory of proto-industrialization, and their criticisms would be equally pertinent to Kisch's conclusions. But as far as the German case is concerned, Kisch's arguments are supported not only by his own very detailed research, but also by many of the studies on early manufacturing in other countries. And the tendency in the past for attention to be directed mainly towards England, the home of the first Industrial Revolution, has for a long time meant that the dynamism and scale of the process of proto-industrialization in Germany in the XVIIIth century has been overlooked. Kisch's work on a very important sector of Rhineland industries in this period serves to correct this imbalance. This study is therefore an important contribution to a growing branch of research in economic history and also provides reflections and suggestions on the early stages of the Industrial Revolution that are of more general interest.

ANTONIO DI VITTORIO

University of Bari

R.F. KUISEL, *Capitalism and the State in Modern France. Renovation and Economic Management in the Twentieth Century*, New York and Cambridge: Cambridge University Press, 1981. Pp. xvi, 344.

Kuisel's book offers a history of the movement, literally and figuratively, of the French economy into the twentieth century. As his title indicates, Kuisel conceives of the state and French capitalism as quite distinct and separable entities, or sets of institutions, rather than as different dimensions or particular aspects of a single phenomenon. This conceptualization permits him to examine the interplay between the various political actors on the one side, and the economic institutions on the other. His story interlaces the lives of the heads of state, the politicians, and the functionaries manning the ministries, bureaux and agencies, and delineates the origins of and evolution in their ideological and pragmatic views regarding the proper and the necessary relationship between the state and the national economy.

Coming from a nineteenth-century tradition of firm, virtually doctrinaire allegiance to classical economic liberalism, the French political and business communities shared a common reluctance to see the state intrude upon the private sector. Historically, the single significant exception to this solid consensus proceeded from Saint-Simonian socialism, a tainting which itself served only to reinforce the prevailing view. From the late nineteenth century on, however, this mainstream perspective began to yield to a growing anxiety about the capacity of the French economy to maintain its position in the face of intensifying international competition, most particularly that arising from the looming industrial prowess of Germany and the United States. Concern that France might fall — or fall further — behind economically, and thus also politically and militarily, finally began to generate a reluctant willingness to contemplate some form of directed or at least organized capitalism.

The debate, then, and the principal focus of Kuisel's study, centred upon the question of what would be the nature, the specific shape and content, that intervention would take. The discussion moved only slowly toward specifics. Kuisel demonstrates quite convincingly that the decisive impulses for this change in direction came essentially from negative experiences, i.e. the two world wars of this century, and the Great Depression; the interlude of the 1930's occasioned only a pause in this central tendency which, if not constant, was nonetheless persistent and cumulative. Accurately, I believe, Kuisel sees the lessons which the French read for themselves from their unhappy fate in the two wars, as the truly decisive and galvanizing factors within this process of conversion to a policy of introducing a quotient of economic management. Indeed, I believe that the author could further have emphasized the role of the Allies and their insistence, given force by the leverage of the French desire for Allied assistance, that the French bring more order and a greater degree of coordination to their economy's functioning.

The critical turning point comes in the 1940's with Vichy, the Resistance, the Fourth Republic, and the first national economic plan. This period forms the crucible from which finally emerges that peculiarly French form of "indicative" planning. French planning is the product of the hesitant, tentative, but ultimately consummated courtship of the economic institutions by the bureaucrats; and in its institutionalized unwillingness to command explicitly, indicative planning in France embodies the attitudes of its creators and faithfully reflects its origins, so capably and perceptively described here by Kuisel. The author captures the outcomes most aptly: of the nationalizations of 1945-1946, he writes "The left's reforms made the French economy more managed and dynamic but not more socialist" (p. 202); on the Liberation reforms, "Certain capitalists — the trusts, black-market profiteers, and collaborators — were to be expunged but not the capitalist. The thrust of reform was to create socializing institutions within a capitalist economy. Yet the key socializing agents ... turned out to be far different from the wishes of their boldest champions. They were products

of compromises that reflected technocratic and *étatist* goals as much as they did socialist or syndicalist ones" (pp. 217-218); and, of the Monnet Plan, "Planning, first of all, became an agent of economic growth rather than a step toward socialism" (p. 246) and "Planning became not a method of command but a way of facilitating collective decision-making and encouraging communication, forecasting, reflection, and coordination" (p. 247). In Kuisel's judgement, finally, the contributions of planning to the post-World War II performance of the French economy have been significant and positive, but not central or overarching.

In Kuisel's view, it was the state which took the initiative and the decisive actions in moving the French economy toward managed capitalism. It is appropriate, then, that his story is told in terms of the individual actors from the political ranks. It is they who occupy centre stage here, and amongst them that the heroes of the cast are to be found. The critic may argue that the story would have been better told had these players instead occupied only a portion of the stage, so that those whose roles proceeded from the economic institutions might have emerged from the wings, to which Kuisel has largely relegated them. Kuisel sees as one of the original contributions made by his study, this highlighting of the roles played by the individual actors, above all by the politicians and the bureaucrats who, in his conception, were the movers and the shakers in the French political economy (e.g., at p. 279). Again, the critic may depart the theatre not wholly convinced by the playwright's interpretation of events. Indeed, as Kuisel himself acknowledges (p. 275), there were at work certain larger, more general forces inclining all of the major industrial economies in basically this same direction, from the later nineteenth century onward. In France these forces had, as Kuisel notes, their own "peculiar Gallic blend" (p. 277). Each national experience, of course, was unique, in its own way, in the manner of its transition and in the details of the path it trod toward managed capitalism. What Kuisel has done — ably, carefully, with meticulous research and documentation — is to afford us a peek at the French script, so that we may see how the sets were constructed, how the players interpreted their roles, how the acts were sequenced. In doing this, he has rendered a valuable service.

RICHARD ROEHL

University of Michigan - Dearborn

D.N. McCLOSKEY. *Enterprise and Trade in Victorian Britain*. London, Boston and Sydney: George Allen and Unwin, 1981. pp. xix, 211.

In order to assess the impact of the studies collected in this volume, it seems useful to review what a summary of the British economy's aggregate performance might have contained if written in the late 1960s. First, productivity growth

across these forty-four years was markedly slower in Britain than in the United States and Germany, the two nations which were challenging British manufacturing products in markets all over the world. Second, British productivity growth rates during the 1870-1914 period were probably slower than trend rates in the mid-XIXth century, a period when Britain was the "workshop of the world". Third, the two hypotheses most frequently cited to explain these phenomena were slackening demand impulses from overseas markets and entrepreneurial failures affecting both old and new manufacturing industries. Entrepreneurial failure took the form of relaxed cost minimization efforts in the short run and, more significantly, an overly slow rate of invention and adoption of new techniques in the long run. Each of these three points has been severely challenged since the late 1960s and, if the state of the art has moved considerably since then, it is in no small part due to the monographs collected here.

Beginning with two chapters arguing the virtues of the "new" economic history for historians and economists, the volume then settles down to McCloskey's wide ranging defence of late Victorian enterprise. Chapter 3 reprints McCloskey and Sandberg's essay on the late Victorian entrepreneur. This is followed by McCloskey's study of U.K. and U.S. productivity differences in coal and steel before 1914. Chapter 5 contains McCloskey's essay on the aggregate growth performance of the late-Victorian economy, very usefully followed in Chapter 6 by his exchanges with Aldcroft, Kennedy, and Crafts.

In these essays on late Victorian enterprise, entrepreneurs are found to have reacted with a profit maximizing calculus in their short-and long-run decisions. If, at times, they did not shift to new markets and new technologies that attracted others, careful sifting of output, cost, and pricing data suggest that entrepreneurs were reacting rationally to their present and prospective cost and profit environment. They appear to have known, and rationally employed, considerably more information than is suggested by the casual second-guessing of their decisions in the pre-1970s economic history literature.

With regard to aggregate growth performance, 1870-1914, the idea that slowing overseas demand slowed British growth rates is shown to ignore the constraints imposed by the natural rate of increase of the population and plausible savings rates. An often hypothesized capital market bias is demonstrated to have little effect if hypothetically eliminated. The possibilities of redirecting homeward the massive flow of British savings going abroad is suggested to have the likely effect of lowering the domestic rate of return on capital and thus not likely to lift national income. In sum, the British economy did the best it could given its resource constraints and little evidence of poor entrepreneurship. If the U.K. growth rate of total factor productivity slowed, its decline was a turn-of-the-century event paralleled by a similar hiatus in the United States.

It is just at this point that McCloskey's revisionism is most controversial. In McCloskey's judgment the U.K. productivity hiatus occurred at the turn of the century but his critics have steadfastly and, not implausibly, spotted the

downward trend beginning two decades earlier. In addition, seemingly irreducible international differences in aggregate and industrial productivity growth have continued to appear in new data collected by Floud, Allen, Lazonic and others. At the moment the most plausible hypotheses offered to explain these trends are Britain's low rate of investment in technical education, research and development; increasingly complicated labour-management relations; legislative inhibitions on some new industries; and the absence of infant-industry tariff protection. However, two explanations distinctly absent are slowing overseas demand and entrepreneurial failure and as much as anyone else, if not more, this is McCloskey's value-added.

The last third of the book is devoted to the Victorian international economy. In Chapter 8 McCloskey shows that mid-Victorian tariff reduction must be seen in the light of the government's reduced expenditures. But, perhaps more importantly, he demonstrates that by moving towards free trade Britain may have moved away from the tariff rate which would have maximized national income. As a dominant buyer and suppliers in many international markets, tariffs could have been employed to capture a lion's share of the gains from trade. The argument is compelling but one wonders whether the handsome gains from Britain's international shipping, insurance, distribution, banking, and capital market service were not dependent on the expansion of the market with freer trade, particularly through Britain's role as a buyer of last resort during cyclical downturns. In Chapter 9 McCloskey estimates the losses from foreign industrialization, 1870-1914, and finds that they were probably a small portion of GNP. This is one of the author's most striking contributions and well deserves the greater audience afforded by this book.

In the final chapter McCloskey, joined by J.R. Zecher, shows how the well known factual anomalies in the traditional accounts of the workings of the gold standard can be comprehended by hypothesizing that prices and interest rates were internationally determined through regular arbitrage. In turn, these internationally set prices and interest rates had effects which overrode central bank policy. There is some danger that McCloskey and Zecher are too enthusiastic on the constraints imposed by XIXth century arbitrage. Arbitraders moved quickly to close price differences in markets for some primary products and financial assets. But, in less well organized markets, rational producers could afford to make certain that price pressures were permanent before changing their prices and undergoing the attendant costly disruptions to purchasing, production, marketing and investment arrangements. Still, the point that international price and interest rate movements constrained XIXth century national economies warrants much attention and there can be little doubt that this last essay, like the others of this section, will have a significant impact on the way the XIXth century international economy is understood and studied.

MICHAEL EDELSTEIN  
Queens College, Cuny

G.V. SCAMMELL, *The World Encompassed: the first European maritime empires c. 800-1650*, Berkeley and Los Angeles, University of California Press, 1981, pp. XIV, 538.

This book complements an already huge bibliography on empires. It ambitiously takes in eight and a half centuries of expansion from the "first Europe" at the coronation of Charlemagne in Aachen (800), a formidable task of compression for the field is extremely rich. In consequence, the choice of departure is crucial in setting the lines of what can be achieved and what by the same token must be skimmed. There are the early initiatives, the consolidation and development, the converging motives of rulers and ruled, the transformations and not least the impending declines. Layer after layer of enterprise create an archeology of relationships and settlements; and in retrospect, studies opt to review the cost-benefit of commercial liaisons, the structures of interlocking political systems, the social co-ordinates of succeeding generations which mingled languages, creeds, races, and colours. Some prefer a type of analysis to biography or business history subject to mutation and engineering; others go for a multi-variate spatial survey of metropolis and periphery. For all, in the end, empire represents a dialogue of civilisations.

Before this ample choice, Geoffrey Scammell quickly sets out his aim. Empire is "the dominion exercised by one people or state over other peoples, lands or states, whether this arises from armed conquest or from the establishment or imposition of economic domination" (p. 1). His undertaking thus takes in concepts of both formal and informal empire. It is unashamedly Eurocentric. It deals with the different sorties into empire from the slant of national or quasi-national political systems, without acceding to the structures proposed by Immanuel Wallerstein. But he brings to the task a salty expertise on ships, their environment and navigation. This remedies shortcomings to put the informed comment among the better surveys in the field.

The book develops in stages. The first is formed by a quartet of chapters which grow in importance: the Norse (34 pages), the Hanseatic League (45 pages), Venice (67 pages), and Genoa (66 pages). The Norse succeeded in creating the "first empire of the North Atlantic" by colonising Iceland and pushing on to settle in Greenland where communities managed to survive perhaps even to 1500. Elsewhere, the Scandinavians dominated the Baltic, North and Irish Seas and formed another network of protection. After them came the Hanse, different in character in establishing not so much territorial settlements as trading posts and *Kontore*. Thus, by stretching the definition, their "empire of trade" comes into the category of informal empire, but it had a fundamental weakness in not creating a focal market. Lübeck long remained the centre of Hanseatic affairs and *Hansatage*, but the associated ports of the Wendish Union gradually shifted gravity to Hamburg and Antwerp. The latter was in trouble after the 1550s, finally succumbed to the rising power of Holland.

The chapter on Venice is firmly based in the current literature, not least

the studies of Frederic Lane. As a maritime empire, however, Venice is not a simple case for it possessed not only trading posts and a few settlements in the Levant but also an important hinterland conquered in the Po Valley. The resulting archives spell out the nature of the problem by classifying administrative papers into a tri-partite *Mare*-, *Terra*, and *Misti*, the last for documents combining both. After the Crusades, Venice concentrated on the lucrative trade-routes through Syria and Egypt. The Portuguese punctured this system by turning the Cape.

The chapter on Genoa is valuable in view of the habitual secrecy of the Genoese about their intricate affairs. However, some aspects of their expansion are glossed over: the acquisition of Corsica, for example, to which the Serenissima was almost driven by shortage of timber. The comparison with Venice is evocative and the closing section of this chapter is devoted to a comparison of the merits and defects of the two republics. Genoa with a foothold in Seville makes an appropriate bridge to the next stage which opens the oceans. After all, Columbus was a native of Genoa.

The opening of sea trade to the East and of settlement in America by Portugal and Spain have pride of place with 74 and 69 pages respectively. Again, the literature is immense and the survey skims some of the big problems — aspects of the early slave trade and attitudes to it have received cursive treatment. But both nations established huge territorial empires in central and southern America; and by opposite routes reached the Far East to trade with China and Japan. They closed the circle of trade from Lisbon to Macau and from Seville via Acapulco/Manilla. Both systems had sizeable commitments to formal and informal empire, with the monarchy of Spain virtually presiding for a time over both (1580-1640). The chapter on Portugal ends in 1663 when “the futility of empire seemed only too clear”; and that on Spain “which succumbed to that disillusion and sense of futility expressed in *Don Quixote*”. Nevertheless, they retained and formed Latin America for over three centuries; and Portuguese survived as a *lingua franca* in the East long after the prime of trade. The problems of their formal and informal empires thus remained complex and clearly Geoffrey Scammell has been pressed for space.

The third part deals with the case studies of Holland (61 pages), France (21 pages), and England (43 pages) — together they hardly match the treatment allocated to the pairs of Venice-Genoa or Portugal-Spain. The new colonial powers nevertheless pushed through the revolution of sugar and black slavery; and after the downfall of Hormuz (1622) came to terms with the peddling trade of Asia. The major problem remained in the development of integrated marketing in Europe which preceded industrialisation.

In sum, the book uses political systems as successive leading sectors in empire-building. This tends to relegate some of the larger issues of world hierarchies of economic power and influence. The relationship of Europe or Christendom with the Middle East or Islam was as dominant theme as the success of Norse

and Hanse. Similarly, the rôle of China: it would be difficult to set in perspective the achievement of the Portuguese without commenting on the vacuum created in the Indian Ocean in the fifteenth century after the end of the great voyages of Chinese junks. These are, however, big issues, and limitations of space have restricted Geoffrey Scammell. He has countered by providing backward and forward comparisons for the nine systems: the Norse compared with Spain, Portugal, and England (p. 31); the Hanse, unlike the Dutch but similar to the urban settlements of Spanish America (p. 56); Venice compared with the Hanse and the Iberians (p. 95); and so on. The book is strong and accurate on the detail of seafaring. It has clear maps, although the Flanders galleys in the fifteenth century pass to Antwerp via Sluys (p. 89). The illustrations are an asset even though the format of the book invites more use of the abundant material available. A number of typographical slips not corrected in the errata mar the useful bibliographies. More serious is the confusion between publishing houses and editors as with Edouard Privat (Toulouse) and A. Giuffrè (Milan).

However, these are at the minor level. The book is easy to read, pleasant to consult, and generous with information and lively comment, now conveniently to hand.

FRANK SPOONER  
University of Durham

W.H. SEVELL, JR., *Work and Revolution in France. The Language of Labor from the Old Regime to 1848*. Cambridge: Cambridge University Press, 1980. Pp. X, 340.

Many readers of this journal will already be acquainted with Professor Sewell's work. His presentation in the *Annales* of some of this book's central assertions won the Koren prize in 1981 for the best article in French history. The book itself has been available since 1980 in both hardcover and paperback and has received numerous deservedly favourable reviews. It seems well on the way to recognition both as a classic formulation of some of the knottier problems of modernization and as a stimulating attempt to apply to history the methods of cultural anthropology. Yet at first glance, Sewell's thesis seems rather narrow. He "traces the organizations and the ideologies of French workers from the old regime to the Revolution of 1848 [and] seeks to demonstrate that themes and sentiments originating in the prerevolutionary corporate system remained central to workers' consciousness and experience" (p. 2). Thus continuity as well as change marks the workers' evolution from membership in the trade-and-localespecific corporations of the hierarchical and corporate old regime, through reincarnation as *sans-culottes* in the individualizing and universalizing world of the Revolution, to emergence as class conscious socialist workers in 1848.

In fact, Sewell's handling of this theme could hardly be more broadly suggestive. He has in a sense written a French version of E.P. Thompson's *Making of the English Working Class*. Sewell's workers, like Thompson's, did not undergo economic and political pressures bereft of intellectual baggage or a sense of moral community. Their ethos as artisans and their later class consciousness were neither mere superstructures erected by the determining forces of economic necessity nor "the ideas of bourgeois theorists [imposed] on an intellectually inert working class" (p. 11). They were active and creative tools with which the workers helped shape their own destiny. In one way, Sewell's book is even broader than Thompson's; in less than half the pages he covers twice the timespan and moves across the Great Revolution, the event that has too often loomed as the great divide in French historiography. One suspects that other themes might profit from this perspective. Again, like Thompson, Sewell provides around his thesis a galaxy of superb set-pieces on an astonishing array of topics. There is Diderot as the exemplar of Enlightenment thought on the concept of labour, or Villermé investigating working class conditions in the 1830's as "a nightmare of the bourgeois conscience, a negative projection onto the workers of the bourgeoisie's own preoccupation with cleanliness, sexual restraint, providence, and temperance" (p. 230). Best of all for the economic historian, there is the discussion of the old regime's conception of property. It included not only the absolute possession of things by individuals (the legal definition to which the Revolution reduced all property) but also (1) that held by the owner subject to "detailed regulation by the community for the common good" (p. 116), like land subject to compulsory rotation of crops and *vaine pâture*; or (2) property in public functions like venal office; or (3) property as all of these and more, like prerevolutionary mastership in a corporation. Mastership was a privilege and therefore a sort of (often) transmissible property producing both social and economic gain, for "it was his possession of a mastership — not his possession of capital — that made a man a master, investing him with a semipublic authority" (p. 118).

It is not a criticism of Sewell to point out that his book is also less broad than Thompson's in several important ways. This could hardly fail to be the case for a relatively brief *essai de synthèse* drawing mainly on the findings of other historians (especially Rémi Gossez, Maurice Agulhon, and the contributors to the volumes edited by John Merriman) as well as his own dissertation on the workers of Marseilles in 1848. Inevitably, *Work and Revolution* lacks the density and particularity, the sense of individuals and places that Thompson conveyed so well. This is especially true for work itself; Sewell tells us very little about that central concern of the new labour historians, the fine content and context of actual work experience. Up to 1848, the content probably changed very little for Sewell's workers, the artisans of the towns and especially Paris. (Manual workers explicitly excluded from the story include the swarms of unskilled casual labourers, proto-industrialization's semi-

skilled rural workers, and the small but growing minority of factory and mill hands). Numerically, at least, town artisans were among the initial beneficiaries of industrialization, which increased the demand for the goods and labour they supplied. But the *context* of their work experience deteriorated under the pressure of changes in economic organization and marketing that diluted skills, set up sweatshops, and spawned urban putting-out systems.

These economic pressures, the individualistic liberal ethos of the Revolution, and the spur of great events like the Revolution of 1830, together changed the artisans' consciousness and their expression of it in the "language of labour". Stimulated by the methods of cultural anthropologists like Clifford Geertz, Sewell views his artisans not through what they experienced at work but through what they expressed about the meaning of labour. They present themselves brandishing not tools but proclamations. They speak in public through petitions, broadsides, newspapers and poems, or they act out "the rituals of insurrection or revolutionary celebration" (p. 102), or they codify their conceptions in the statutes of corporations, *compagnonnages*, confraternities or mutual-aid societies. This "discourse", these "texts", the artisans' "idiom" consisting of key phrases like *corps d'état*, must be "construed" by the historian in order to extract their changing meanings. Powerfully and subtly, Sewell demonstrates that the corporate elements in the artisans' idiom of 1793, 1830-34, 1839-40 and 1848-51 were not merely inhibiting and archaic remnants. They were a means of transmitting and transmuting usable concepts of moral community and the value of labour.

Two possible weaknesses intrude, one at either end of this excellent book. First, my non-expert guess is that the corporation of the old regime was no more a healthy moral community than it was a fiscal tool of the government or a hotbed of internal conflict between masters and journeymen or large and small masters. The other problem is a matter of tone; by the time Sewell reaches the 1840's it becomes difficult to distinguish his "discourse" from that of the artisans as he roots his heroes home. Their idiom, one feels, has become his. But that itself speaks eloquently of the distance his workers have travelled.

REED G. GEIGER  
University of Delaware

A. SLAVEN - D.H. AIDCROFT (eds.), *Business, Banking, and Urban History. Essays in Honour of S.G. Checkland*. Edinburgh: John Donald Publishers, 1982. Pp. XIV, 235.

I liked very much this *Festschrift* for Professor Checkland, who was for twenty-five years professor of economic history at the University of Glasgow. I liked it from the foreword by Sir Alec K. Cairncross, which is a brief biography of Checkland, through all of the twelve essays by students and colleagues of the man whom they were honouring. I liked it for the knowledge

that it gave me of the development of economic history in Scotland, for the range of interests which Professor Checkland had, and for the evidence which was presented of the excellence of instruction to be obtained at the feet of this master.

We learn that Checkland was from Canada and had his first brushes with business affairs as a bookkeeper and accountant in his home country. In 1938 and with a reserve of \$ 250.00 he left his native land for Birmingham, England, to pursue studies in economics. He graduated with first class honours in 1941, went on a 3,000 mile bicycle trip on the Continent before the fall of France, joined the British military, and finally became a member of an armoured brigade of the Canadian army. He was severely wounded after the landings in Normandy. Then, during a long convalescence, he turned again to economics and upon being discharged went back to Birmingham to study with W.H.B. Court. Subsequently he held academic posts at Liverpool and Cambridge, then in 1957 moved on to Glasgow.

One of Professor Checkland's major interests, when he assumed his chair, was in the field of business history. Therefore, it is quite appropriate that the first three contributions of this collection are devoted to studies in this area — to businesses along the Firth, such as coal mining, the production of salt, and shipbuilding. In the preparation of these essays the usual difficulties were encountered, especially the over-abundance of records, or such a great sparsity of them, that the writing of a continuous account was barely possible. These contributions illustrate well the problems which early industrialists always faced -- the irregularity of production owing to the breakdown of machinery, the absenteeism of workers on Mondays and on days when the "fishing" looked good, and to difficulties in estimating the demand of the market, which was becoming ever more impersonal and distant.

Another of Checkland's main interests was banking, and this field is represented by the next three essays. The first of these is by Rondo Cameron, the only American among the contributors, who devotes his attention to the role of banking in Scotland and England. His study is of particular interest because his views conflict with those of the very man he is honouring. In my opinion Cameron has the better of the argument in contending that the Scottish system was markedly superior to that of England in the period under consideration and that the two systems became essentially similar because the English adopted so many of the practices of their northern neighbour.

An essay on the development of joint-stock banking in Scotland between 1810 and 1845 traces the decline of private and provincial banks and the rise of institutions organized on the corporate form. The author of this piece dissects banking into its essential parts and explains how joint-stock banks performed all the tasks assigned to them better than other types. This is followed by an essay on the international flow of precious metals between the years 1848 and 1867, which would have warmed the heart of William Jennings Bryan.

The last four contributions are given over to studies in urban history. The first of these deals with a much overlooked issue — the argument that sanitary and health improvements should be supported by governments because of the high cost of raising humans to productive ages. The next two studies are concerned with the history of Glasgow and illustrate many of Professor Checkland's extraordinary insights into the past of Scotland. The last essay of this group turns our attention to the problem of transportation within the inner-cities of Western Europe. Urban planning faced a major task in trying to cope with narrow, winding streets and the enormous growth in the volume of people and goods which had to be moved through them. Few cities were fortunate enough to start from scratch, as did many American ones, and few had the benefit of leaders to undertake such face-lifting jobs as that begun by the Baron von Haussmann in Paris.

Lastly, I must mention an essay devoted to Lord Beaverbrook and to his role in increasing the output of aircraft during his brief stay at the Air Ministry in 1940-41. The author of this piece takes exception to the usually held view that Lord Beaverbrook performed a miracle during his short period in office and presents evidence to support his contention. Whatever the case, this essay illustrates still another of the facets of Professor Checkland — his concern with current political affairs. Indeed, I should mention that he once ran for political office and fortunately for the historical profession was defeated.

SHEPARD B. CLOUGH

Professor Emeritus  
of Columbia University

J.R. WINTON, *Lloyds Bank 1918-1969*, Oxford University Press, Oxford, 1982, pp. 210 + VIII.

The history of a great commercial bank like Lloyds is of interest in itself and for the light it sheds upon the impact of political and economic events on the behaviour of financial institutions and on the lives of the people employed in them. Professor R.S. Sayers, in his *Lloyds Bank in the History of English Banking* (Oxford, 1957) described the history of Lloyds from its founding in 1765 to the end of the First World War, when the structure of the bank which was to be maintained for the next 50 years was almost complete. Mr Winton takes up the story at that point and covers those 50 years.

Before the First World War Lloyds Bank had, in common with the other London clearing banks, grown rapidly by mergers and takeovers of smaller banks. After the war, and following the report of the Treasury Committee on Bank Amalgamations in 1918, the authorities began to discourage further amalgamations. The banks now had to seek approval from an advisory committee; with this approval Lloyds absorbed Capital and Counties Bank in 1918,

the West Yorkshire Bank in 1919, and Fox Fowler & Co. in 1921. These were the last mergers involving Lloyds until the end of the period covered by this volume, with one interesting exception. Lloyds unsuccessfully sought approval for two further amalgamations in the 1920s, but it did not seek to take over the "rickety" business of Cox & Co. and King & Co. (which had recently been acquired by Cox's) in 1923. Cox & Co. were Army Agents of long standing (since 1758), with branches in London, Egypt, India and Burma; Montagu Norman, the Governor of the Bank of England, was anxious that they should not fail and persuaded a reluctant chairman of Lloyds, J.W. Beaumont Pease, to take them (and King's, which also operated in India) over, with the help of Bank of England guarantees against losses. In this way Lloyds acquired one of its largest branches at 6 Pall Mall, with its Army and Air Force business, and an extensive, and unprofitable, business in Egypt and India. The reluctant acquisition was to pose many problems, at home and abroad, for Lloyds in the subsequent years, and the parts of this book devoted to them were among the most interesting to this reviewer (who has been a customer at Cox's and King's branch for the last twenty years). The Egyptian business was shed, again with the assistance of the Bank of England, in 1926, but that in India and Burma was retained, somewhat to the chagrin of the Governor, until 1960. (In the interwar years Lloyds also acquired substantial interests in banks in New Zealand, West Africa, Europe and South America, but the formation of Lloyds Bank International from these and the Bank of London and South America occurred after the terminal date of this volume).

At home Lloyds maintained its market share (17-20% of the total deposits of the London clearing banks) over most of the period covered by this volume in spite of the vicissitudes of depression and war. After the short-lived boom of 1919-20 a cautious attitude toward lending seems to have prevailed, reflected in the behaviour of deposits and advances, which is clearly described and related to the wider context of the behaviour of the British economy. Until the 1960s the growth of the bank came from extension of traditional commercial banking rather than from financial innovation and the provision of new services. In the 1920s many new branches were built, including the new head office in Lombard Street in 1926-30: here as elsewhere Mr Winton's eye for detail helps to enliven the story. The depression cut back the branch building programme along with advances, profits and dividends, and the recruitment of staff. There were some changes in the management of the bank in the interwar years, but these were mainly consequences of the 1918-23 amalgamations rather than of changes at the top: Pease was Chairman from 1922 to 1945, "a length of term that must be unique in English commercial banking". Much useful information is provided on the backgrounds and careers of the men who rose to senior management positions in Pease's time and in the years after 1945.

Mr Winton uses his detailed knowledge of the workings of Lloyds to

particularly good effect in recounting the role and operation of Lloyds in the Second World War (at home, in the East, and in the enemy-occupied Channel Islands), which is indeed a fascinating piece of social history. Postwar economic conditions and monetary policy provided a new environment for British banking and the account of Lloyds' business in the 1950s and 1960s provides a useful illustration of the effects of quantitative credit controls on banking practice. Lloyds shared in the rapid expansion of British banking in the 1960s, in which new types of business and the provision of new services to customers played a large role. The domestic activities receive most attention; Lloyds' expansion into international banking and the eurocurrency markets was a development of the 1970s and is discussed only in a short epilogue. The expansion was facilitated by the computer revolution: Lloyds was the first British bank to transfer its entire branch network to a common computer accounting system, using its large Cox's and King's branch to try out the system in the early 1960s. (The branch had already been provided with special machines to cope with the enormous increase in its business — a third of the total increase in current accounts for Lloyds as a whole — in the Second World War).

A good deal of the book is rightly devoted to the bank's staff: their recruitment and training, their pay and conditions of work. The changes here have been major and far reaching, although slowly introduced. At the beginning of the period there were no training schemes, no general recruitment of women (after wartime employees had left) or of university graduates, and little in the way of staff representation in the making of salary decisions. As for their work, in 1918 most Lloyds branches possessed a typewriter but book-keeping was done by hand. Mechanisation, and the employment of women in significant numbers, commenced at the end of the 1920s, as did some training arrangements and a new salary structure. A serious attempt to recruit graduates was not, however, made until 1969, and the appropriate form of staff representation was still an issue in Lloyds, as in the other clearing banks, in the 1970s. Lloyds' most famous bank clerk in this half-century of its history was, however, a Harvard graduate, some of whose special skills were utilised: two very interesting and informative pages describe T.S. Eliot's work in the colonial and foreign department and at head office in 1917-25.

SUSAN HOWSON  
University of Toronto