
REVIEWS OF BOOKS

R. CHAZAN, *Medieval Jewry in Northern France*, Johns Hopkins, Baltimore, 1973, 238 pages.

Robert Chazan, Melton Associate Professor of Jewish History at the Ohio State University, is known for a series of articles on the history of the Jews of Northern France from the tenth to the twelfth centuries. His new book is an attempt to present a detailed narrative of that community's development in post-Carolingian France until the expulsion of 1306. Such an effort is long overdue for while the history of Southern French Jewry during the same period has attracted a good deal of scholarly attention, there has been no concerted effort to reconstruct the history of the northern Jews although there are references to them in several general works on Jewish history.

Chazan divides his narrative into three major parts: a period of prosperity and intellectual achievement under the reign of Louis VII, growing problems highlighted by increasing difficulties with the church under Philip Augustus and Louis IX and a period of cynical exploitation, growing personal restriction and final expulsion during the reign of Philip the Fair.

Although Jews had been settled there from Merovingian times and even before, the eleventh and early twelfth centuries were a period of extraordinary growth and splendour for the Jewish communities of northern France. Intellectual life flourished and the Jews developed extraordinarily rich communities complete with their own courts, social welfare institutions and local government. Economic life benefited from the growth of lending based on contracts that were registered by special government officials and enforceable in court. It is during the mid-twelfth century that Bernard of Clairvaux used the expression "iudaizare" as synonymous for usury.

This seemingly favourable situation began to change under Philip Augustus. Spurred by pressing financial needs the king resorted to the confiscation of Jewish property and the remission of debts owed the Jews in exchange for 20 per cent paid to the royal treasury.

The long reign of this monarch also saw increasing church hostility to Jewish moneylending practices. The IV Lateran Council which strongly condemned Jewish usury and demanded that secular authorities force the Jews to remit debts to crusaders pointed the way to restrictions on usury that were introduced under Louis IX.

It was under the reign of Louis that the ruin of the Jews culminated. Pious and concerned with making French society conform to Christian principles, Louis was convinced that Jewish usury was a blight on Christian morality. His reign was characterized by a series of laws that progressively withdrew royal support from Jewish lenders and ended by entirely prohibiting Jewish usury, the principal economic activity of the Jewish community. Moreover, under the influence of Pope Gregory IX, Philip banned the Talmud and other Jewish books.

The long history of Northern French Jewry ended during the reign of Philip IV with the expulsion of 1306. Preceded by a period of ruthless taxation, the expulsion was the final failure of the experiment of allowing considerable Jewish communities to develop in medieval France.

Professor Chazan's account will be useful to students of Jewish history but it is marred by an unusually dull literary style and constant and unnecessary repetition. His treatment would have benefited by a broader analysis of economic developments in the larger Christian community. Several chapters are headed by brief discussions of general economic trends but there is little specific indication of how these might have affected the Jews. Based on relatively laconic sources his work contains very little specific information on the size of the Jewish population (we are told that the Jews of northern France numbered between 45,000 and 125,000) or on such important matters as the exact amount of Jewish loans or taxes.

Perhaps Chazan's most significant contribution comes at the end of the book in the appendix which contains a useful map of Jewish settlement in northern France.

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R. E. COONS, *Steamships, Statesmen, and Bureaucrats. Austrian Policy Towards the Steam Navigation Company of the Austrian Lloyd 1836-1848*, Wiesbaden: Franz Steiner, 1975, pp. XIII, 209.

The subject is interesting. Steamships convey a feeling of romance; statesmen add aloof policy and bureaucrats suggest unpleasant regulations,

obstructionism and written verbosity. All this is to be found in Mr Coons' book. Alas, the bureaucrats carry the day. Left out of the title, unfortunately, were the businessmen. They, too, played a prominent role.

The founding and the early history of the Austrian Lloyd steamship company, located in Trieste, is the topic with which this monograph deals. Begun as an information bureau and a maritime insurance company, the Austrian Lloyd branched into the shipping business in 1836. Despite the bias of some government officials in Vienna in favour of economic liberalism, the central government did indeed get involved in the fate of the company.

Trieste, Austria's major outlet to the sea, is the key to the story. Austria's claim as a maritime power depended on the success of the port. Ever since the second decade of the eighteenth century the hopes of Austria for maritime commerce as well as for an economic penetration of the Levant revolved around Trieste. In the nineteenth century its commercial prowess was to be strengthened by the introduction of steamships, whose main functions were to trade with the near East and Italy.

As part of this story the statesmen Metternich and Kolowrat, the bureaucrat-statesmen Bruck and Kübeck von Kübau and a number of out-and-out administrators, such as the head of the postal service enter the narrative. Metternich, for example, saw an opportunity to strengthen Anglo-Austrian relations by having English mail to India carried from Trieste to Alexandria rather than by way of Marseilles. The project was abortive but still interesting. Kübeck, whose *Tagebücher* are such an important source for the *Vormärz* period in Austrian history, is depicted as a semi-doctrinaire proponent of *laissez-faire*, pragmatic enough, however, to see the need for an Austrian maritime commerce that required government support.

Since the Austrian Lloyd was a private company, it is to be deplored that the activities of owners and managers are given such a small place in this work. This is especially the case since the major owner was no less a personage than Solomon Rothschild of Vienna, who may, to be sure, have participated in this venture because of his other business activities with the flotation of Austrian state loans and because of his close ties with Metternich and other government officials. But, unfortunately, documentation for the businessmen is usually sparse and the pragmatic historian therefore tends to neglect their contribution. Nevertheless, the author could well have made a more determined effort to place these men in their relevant historical positions.

This book, which seems primarily to have served as a doctoral dissertation, appears to be seriously constrained by documentation. The author probably adhered to the archival documents too much, spending considerable space in paraphrasing particular memoranda. This mars an otherwise good presentation because often the minutiae of the bureaucrats' position papers and their recommendations are neither important nor the basis for

especially good reading. Nevertheless, the book is a useful exercise and provides the interested reader with some valuable details on the important story of the Austrian Lloyd, one of the primary instruments by which the Austrians tried in the nineteenth century to escape from the restrictions of a largely land-locked empire.

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M. DEL TREPPO, *I mercanti catalani e l'espansione della Corona d'Aragona nel secolo XV*, (Università di Napoli, Seminario di Storia Medievale e Moderna n. 4), Napoli 1972 (2nd edn), pp. 870.

Even though it is only four years since the this second (1st edn. 1967) edition was published, in view of the growing interest outside Italy in M. Del Treppo's study of the Catalan economy in the XVth century, it is worthy of further attention. The focus around which the various problems examined in the study centres is of course the Aragonese conquest of the Kingdom of Naples. Throughout the book the political, military, and economic consequences of this major event in the history of the Mediterranean are kept in the foreground. The author is concerned to explore the broader motives which lay behind Alfonso the Magnanimous's venture, as well as the decisive influence which it had on the structure of the Mediterranean economies in the middle of the century. With regard to the latter, attention should be drawn in particular to the way in which the author attempts to assess its importance for Aragonese economic policy. For Alfonso, especially, this involves setting it against the "economic policy" (in the term which Del Treppo has elsewhere used to describe the Aragonese dominions) which he was creating and establishing. The event was of such importance and had such great consequences that it is entirely justifiable to take it as a basis for studying the development and particular characteristics of the Catalan and Barcelonan economy.

The author begins by describing the development of the Catalan economy from the last decades of the XIVth century. The XIVth century, therefore, remains largely unexplored. It was not accidental, then, that at the first historical conference on "Liguria and Catalonia" (Bordighera - Genoa, Oct. 1969), Del Treppo should have suggested that the documents from the Genoese Customs House for 1376-77, which had been edited by J. Day, made it possible to draw an interesting comparison between the late XIVth and XVth centuries, in particular in terms of the maritime trade between Barcelona and Genoa and its balance (Atti del I Congr. Storico Liguria-Catalogna, Bordighera 1974, pp. 621-667).

At the end of the XIVth century trade with the Levant was not considerable, but in the first decades of the new century it began to expand.

From 1433, during the conquest of the Kingdom of Naples, this Levantine trade began to slow down, but after 1454 there was a fresh revival which was only halted by the outbreak of civil war in 1462. The development of Catalan trade in the XVth century was then deeply influenced by political and military events. But from his analysis of the structure of Catalonia's trade with the Levant, Del Treppo argues that political factors (which, as he emphasises, cannot in any case be divorced from economic considerations) produced results which conformed to the structural characteristics of the economy. Barcelona was not like Venice, and had no extensive hinterland; and she was also excluded from Bruges by the Italians and from southern France by the economic policies of the French monarchy. Her area for reexporting Oriental goods was then limited, and her trade balance with the Levant would have remained, as a consequence, hopelessly in deficit had she not been able to offset this with credits coming from Rhodes and Sicily—and in the same way, of course, Naples. The conquest of Naples, in fact, served to restore equilibrium and to balance Catalan trade with the Levant. What was at stake was the integration of the Neapolitan market into the Catalan trading system, which, in another respect, as Rycraft has pointed out (*English Historical Review*, July 1975, p. 635), makes it possible to modify the traditional notion that European trade with the Levant was permanently in deficit.

The principal source material on which the study is based is unedited, and is provided by the marine insurance contracts kept in the protocols of the Barcelona notaries. This might seem an unusual source for reconstructing the development of sea-borne trade as a whole, especially as the contracts provide information which is not statistical, and which might easily give a distorted or partial picture. In order to avoid such traps, the author explicitly disclaims to have produced absolute figures, but has taken great pains to construct relative series which can be compared. The source material on which the study is based is, then, totally convincing. Furthermore, the methodology adopted is particularly interesting. The problem of the inter-related links between economic structure and political and military events, which is not dealt in Carrere's study, (*Barcelone centre économique à l'époque des difficultés [1380-1467]*, Paris 1967) is never lost sight of. As in the 1969 conference, Del Treppo deals with the problem of the existence of different possible outcomes, none of which could be predicted beforehand, to particular issues, by comparing them dialectically. This avoids, or rather rejects, falling back on vague "either or" statements. The approach in fact means that something of a paradox emerges from the study. While the author seems to pay particular attention to the individual, be it a political or military event or be it the diversity of traditions and mentalities, at the same time the essence of the economic structure as a whole with which he is dealing is revealed unmistakably and unambiguously. His conclusions about the

conquest of Naples provide a clear example of this. But his disagreement with those who have attempted to see the problems of Catalonia, and of the other great Mediterranean cities in this century within a single economic structure is also interesting. Del Treppo emphasises the peculiarities which distinguished one from another, but it is from these particularities that a structural explanation of the Catalan economy emerges.

In terms of structures, the study has been criticised by Bisson (*Speculum* Jan. 1976, p. 174) for failing to describe the regional conditions of agriculture and of the industrial production, so that the position of the "merchants" within Catalan society remains unclear. Certainly Del Treppo's book does not explore the relations between the countryside and the sea, and he was concerned in the first place to study Catalan trade as an essential component of the Mediterranean economy. In so far as this perspective permits, the problem raised by Bisson has not been neglected, and in fact the chapter on Johan Torralba is concerned precisely with this. It shows the variety of activities in which the merchant was involved, his interests and his ties both with the land and with politics.

Although this is a book of major interest and very broad scale, it is perhaps still only the first part of an even broader study of the age of Alfonso the Magnanimous and of Naples under the Crown of Aragon.

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S. HOWSON, *Domestic Monetary Management in Britain, 1919-38*, Cambridge, Eng.: Cambridge University Press (University of Cambridge, Department of Applied Economics, Occasional Paper 48), 1975, pp. ix-213.

This prize-winning dissertation reflects the extent to which the teaching of economics at Cambridge University has been modified by the doctrines that have come to be known as monetarism. Ten years ago a study of domestic monetary management in Britain prepared at the seat of Keynesian economics probably would have completely ignored the behaviour of the money supply and discussed only the level of interest rates. The present study at least pays lip service — but not much more — to the notion that current changes in monetary growth are related to later changes in the rate of growth of income measured in pounds sterling.

In five chapters plus introductory and concluding chapters (but no index) the author examines in chronological order, during the interwar period, domestic monetary policies of the Treasury and the Bank of England. The basic outlines of the period are well known: the post-World War I boom was followed by a deep business contraction from which the economy barely

recovered before it was subjected to further deflationary pressure to enable Britain in 1925 to return to the gold standard at its pre-war parity; then after a brief respite, the economy was plunged into a deeper contraction in 1929 that abandonment of the gold standard in 1931 made it possible to cut short; finally, the recovery after 1932 proceeded until the third quarter of 1937 when a brief but sharp business contraction intervened.

The purpose of the study, according to the author, is to try « to assess whether monetary factors contributed to the timing or development of particular cycles rather than to provide a monetary explanation for each cycle » (p. 6). For each episode the author describes the Treasury's debt management operations, often quoting unpublished internal Treasury documents bearing on the decisions that were made, as well as the Bank of England's actions, here drawing on secondary published sources for corroborative detail.

The view presented in the study is essentially Keynesian: to judge monetary policy, one looks to long-term interest rates « on the grounds of the larger effect of monetary policy on investment and on a Keynesian view of business cycles » (p. 5). Such a view forecloses the issue before it is investigated. From a monetarist standpoint, a far wider range of marketable assets than investment as recorded in national income accounts and of interest rates, whether or not recorded, must be taken into account. On this alternative view, the particular Keynesian sequence from money to bonds to interest rates to investment expenditures and, thence, to income, is one, but only one, possible channel of transmission of monetary change to income change. For a monetarist, a discrepancy between actual and desired money balances — usually resulting from a change in the rate of money creation by the monetary authorities — may be eliminated by initial spending effects on all manner of goods and services, including durable and non-durable consumer goods, investment in education, in financial assets of the wide variety available, including not only bonds but also equities, mortgages, life insurance and so on. The effects are then further diffused as demand shifts for current output of goods and services and new sources of productive services.

This non-Keynesian view of monetary effects on cyclical change does not receive a hearing in the study. The only obeisance to this view is the presentation of monthly estimates of deposits of ten London Clearing banks plus currency in circulation (Southern Ireland is not excluded), with no adjustment for seasonal variation. The author attempts to date turns in this money series in relation to cyclical turns in the British economy, excluding June and December estimates because of window-dressing in those months. The reliability of monthly turns in the circumstances seems dubious. Even so, the author dismisses her finding that the money series leads the cyclical turns because she deems the lead either too short or too long relative to the

average that work by others has indicated. She overlooks the fact that the lead has always been described as variable.

This exercise done, the author then concentrates on the movements of interest rates: money was either "cheap" or "dear". And in Keynesian style, she finds little evidence that monetary factors played an important role in determining the fortunes of the British economy. For example, she argues that the post-World War I boom was breaking due to a fall in consumer demand, unrelated to the increase in interest rates in 1919. « What could have helped was a continuation of wartime controls, which, thanks to the government's fear of unemployment, had been all too hurriedly relaxed, and which had given the signal for the boom to commence » (p. 24) — again, the attachment to controls is a Keynesian response. Recovery came "naturally" in 1921 rather than following with a brief lag a decline in interest rates and an increase in the rate of monetary growth. Similarly, she attributes the expansion of 1921-24 and the contraction of 1924-26 to « conditions abroad » (p. 55) rather than monetary factors. Even the acknowledgment that the appreciation of the pound in preparation for the return to gold might have played a part in affecting export behaviour after 1924 is qualified by the argument that it was forces other than monetary policy that led to the rise in the exchange rate of the pound.

The author's overall conclusion is that the first postwar cycle was determined by domestic factors but the downturns of 1929 and probably 1937 were determined by external forces. She does not confront the question as to what differentiated the first from the other cyclical experiences. More important, she does not discuss systematically the way in which influences from abroad produced cyclical changes in Britain. Had she done so, she might have been led to examine the role of the balance of payments in affecting high-powered money, the sum of bank reserves and currency outstanding that the monetary authorities control.

There are a number of slipshod comments in the study. The author seems to be confused about the effect of a reduction in the amount of Treasury bills held by the market. In one place (p. 104), she says it reduced interest rates; in another, it raised them (p. 144). She refers to « a drastic downturn in investment » (p. 24), 1920-21, on the basis of current price figures in the source (C. H. FEINSTEIN, *National Income Expenditure and Output of the United Kingdom, 1855-1965*). She should have cited the constant price figures in the same source, which show real investment rising in 1920-21 from £ 284 to £ 326 million; even excluding dwellings, not all of which were local authority investment, real investment at most was unchanged. Velocity is a relation between nominal income, not real income, and money stock, as the author seems to suggest (p. 47).

Unquestionably the study reflects a prodigious effort on the part of the author. Unfortunately, her product is standard Cambridge, England, Key-

nesianism, barely responsive to the intellectual currents that have challenged that orthodoxy in the past decade and to the questions about theory and policy the British have been living by that the present plight of their economy should be raising.

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D. P. O'BRIEN, *The Classical Economists*, Oxford University Press, 1975, pp. xiii, 306.

This is a "sensible" book, as distinguished from an inspired book such as Schumpeter's *History of Economic Analysis*, or a work of Herculean scholarship like various writings by Jacob Viner, or a collection of penetrating and pungent insights, à la George Stigler. Sensibleness should not be underrated, however. It is by no means universal, or perhaps even common, in writings on the history of economic thought. *The Classical Economists* can be assigned to students without fear that they will come away with a lot of cute nonsense (Heilbroner) or mathematical manipulations of historically inaccurate doctrines (Blaug, Samuelson, and a paragraph of *et ceteras*). The book is also written in literate English, with the obvious intention of its being read — again, not something to be taken for granted in this field, or in economics in general.

The Classical Economists is a quietly perceptive book. A phrase such as « a labour standard of economic welfare » tells much about the author's insight into what others call a « labour theory of value », and about his understanding of the difference between a philosophical *measure* of value and a substantive economic *theory* of value. It also tells something about Professor O'Brien's modesty that this is all encapsulated in a passing phrase rather than being announced by a rhetorical blare of trumpets or roll of drums.

Classical economics is presented in a topical sequence — value theory, public finance, international trade, etc. — rather than according to the usual historical parade of greats and near-greats, beginning with Smith, Malthus, and Ricardo. The first two chapters, however, give an historical overview to orient the reader to the chronological sequence and to contemporary developments in the economy and society. The presentation of classical economics by topic rather than by chronology has both benefits and drawbacks. Among the benefits are that each topic can be analyzed more fully in one place, the insights of one classical writer can be used to fill in gaps and repair clumsy phrasing by another, and the historical development of particular doctrines can be readily shown. But there are both inherent draw-

backs to the topical presentation and special problems created by the particular way Professor O'Brien uses this approach.

The chopping up of Smithian, Ricardian, etc., systems and reassembling the components topically, out of context, offers a fertile ground for misunderstanding — especially if the *number* of writers included goes beyond a few whose general systems can be easily kept in mind. But this book has a generous — even sweeping — definition of classical economics. Senior, Torrens, and even Lauderdale are included as “classical”, as are almost all the economists between Smith and Cairnes. This makes for an unwieldy conception and for a loss of connection between specific theories and the general systems of which they were part. It also offers many pitfalls for misinterpretation of classical economics by allowing the author arbitrarily to designate certain doctrines of A, B, and C as “classical” when the same designation might as validly apply to the opposite doctrines of X, Y, and Z. For example, according to this book, the classical economists favored labour unions as an effective and necessary means of ensuring the worker an equitable share of economic reward. The reference? J. S. Mill and J. R. McCulloch. Are these two men “classical economics”? For this purpose, in this book, they are. Had other writers (including Adam Smith) been chosen, it would have been possible to “prove” that “classical economics” regarded unions as futile. The difficulties of defining classical economics once and for all should not be under-estimated. Neither should the arbitrariness of calling changing subsets of economists “classical”. But as a history of the classical *era*, this is a very good one.

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E. J. PERKINS, *Financing Anglo-American Trade, The House of Brown 1800-1880*, Harvard University Press, Cambridge, Mass. and London, 1975, pp. XI+323.

The House of Brown was crucially involved in America's international trade in the 1800s. Edwin J. Perkins, using the firm's records, reconstructs the history of the House, from its Baltimore origin in 1800. His book emphasizes the significance of Anglo-American ties. Americans used sterling bills of exchange; most letters of credit were sterling obligations; the Bank of England was the lender of last resort, the Central Bank, for American international houses in the nineteenth century.

In 1810 the Browns established a Liverpool branch. In 1820, 1830, and 1839, their firm imported 3, 5, and 7.2 per cent of the Liverpool raw cotton imports. Through Liverpool, American cotton was fed into the basic

industry of the British industrial revolution. The importance of the House of Brown lay in the way a fledgling, Baltimore merchant in the linen trade (actually an Irish immigrant) went about stimulating, spurring, and profiting from the opportunities opened by two coincident and interrelated events: the rise of a major cotton textile industry in England and a major cotton growing region in the United States.

The Browns imported as well as exported and in 1831 the partners decided to employ a permanent representative in Manchester to obtain consignments from British textile producers. By 1831, the United States had its own textile industry. Was this representative seeking to move nearer to the British suppliers to reduce costs and to meet the new American competition? The book never answers this question.

The volume is organized in a unique manner. There is first a chronology, and then a functional treatment of advances and merchandising, letters of credit, and foreign exchange. What seems missing in the chronology can often be found in the second part.

Brown's early extensive use of letters of credit in the 1820s was for imports of dry goods and hardware into the United States. The Browns' New York branch — opened in 1825 — was central to the partners' activities in the import sector. Perkins has fine data on the use and management of letters of credit.

New York became the principal market for foreign exchange along the Atlantic Coast, surely in the United States, in the 1820s. The Browns paced this development, becoming active in the foreign exchange business. It would be useful for Perkins to have provided a table on rates of exchange and the fluctuations. Perkins' data on the specifics of the foreign exchange transactions are, however, valuable.

With the decline of the Second Bank of the United States in the late 1830s, the Browns led in the foreign exchange business in the United States. They made advances on techniques first introduced in the American market by the Second Bank. By 1837, the firm was committed to concentration on financial services in the foreign trade sector. It moved its headquarters from Baltimore to New York.

The American Civil War created difficulties. The House closed its southern branches. The Liverpool partners tended to be pro-Southern and urged New York to give the joint interests of the firm the highest priority. In 1863 the House opened a London office, and the two key British partners moved there, leaving Liverpool a sub-branch until it was finally closed in 1888.

Meanwhile, wheat became the principal export from the United States financed by the Browns. Their southern connections took a subsidiary role. In the 1870s the firm was far less dependent on the south for its supply of sterling bills than it had been before the Civil War.

The Browns failed to adapt to the changing environment in the 1880s and 1890s. The House had always been concerned with "safety". Now, it was cautious in dealing with such new industrial enterprises as Standard Oil and American Sugar Refining: as a consequence, it lost « good customers and much good and safe business », one partner concluded in 1897.

In Britain, the grandson of a partner, Montagu Norman, injected new vitality into the London branch in the early twentieth century. The Anglo-American partnership split apart in 1918. The British enterprise in time (in 1946) was incorporated as Brown, Shipley and Co., Ltd. The American partners merged with the Harriman family operations to form in 1931, Brown Brothers Harriman & Co.

The influence of Harvard University's business historian Alfred D. Chandler permeates this book. Perkins asks of the House of Brown questions on management structure that Chandler has posed on railroads and industrial corporations. Perkins concludes that the Browns « developed an informal administrative structure which may be viewed as a transitional form between the prevalent family enterprise and the more professional management system created by the railroads and later adopted by large industrial firms ».

The idea is interesting, but students of European economic history are bound to wonder whether the East India Company's administrative structure would provide an earlier, probably superior example of such a transitional form. Chandler's questions on strategy and structure need to be asked of many early enterprises. One of the major values of the book is the attention it pays to the management structure of the enterprise.

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G. RUDÉ, *Robespierre Portrait of a Revolutionary Democrat*, New York, Viking, 1977, pp. 254.

For the student of the French Revolution George Rudé is probably best known for his seminal studies of the crowd during the French Revolution. Now he has turned his hand to writing a biography of one of the most fascinating characters of the French Revolution Maximilian de Robespierre. Robespierre is a magnificent choice of subject for a biographical study because in the minds of most readers he is associated with being the bloodthirsty architect of the Reign of Terror. Furthermore, the last major study concerning Robespierre was written over 35 years ago by J. M. Thompson and so there is a great need for a new up to date study.

This book is not a biography in the ordinary sense of the word, but rather a political portrait of Robespierre. Only the first part of the book is

a narrative-descriptive discussion of the salient parts of the life of Robespierre, showing how he grew from a minor personality and matured into a great political power and political theorist. The second part of the book is one of the most interesting and helpful to the student of history because it treats with the changing image of Robespierre from his death in Thermidor down to the present date. In the course of almost two centuries there have been great fluctuations in the attitude of the French towards Robespierre. There have also been changes on the part of the English as well as the writers of other countries in their attitudes toward Robespierre over the years. In most cases the attitudes concerning Robespierre was a direct reflection of the writer's feelings and opinions concerning contemporary events. The political or social bias of the writer had a very strong influence upon the evaluation placed upon Robespierre. Thus no English or American author is ever politically neutral about Robespierre. If they were conservatives, they manifest little sympathy for him and if they were radicals or socialist, Blanquists and Trotskyists, they would tend to reject him. It is, however, rather interesting that Karl Marx viewed Robespierre as neither a hero nor a villain, but as a Utopian who played a limited but necessary historical role.

The thematic and analytical parts III and IV are of the greatest interest to the specialist in the field. These sections concern themselves with Robespierre as an ideologue and a practitioner of revolution. As an ideologue Robespierre was an uncompromising champion of democracy and people's rights. He was unbending and unyielding on these principles and there is no doubt that it was these so called flaws in his character that played such an important role in his eventual downfall. Robespierre could never bring himself to bow to political expediency. He believed in certain fundamental truths which for him were eternal, universal, unalterable and imprescriptible and that therefore belonged to no particular time or place. Thus he believed that « the end of politics must be the embodiment of morality in government ». This morality emanates from the people and it is their will that must prevail. This may explain for why his speeches say so little about social democracy.

In the matter of Robespierre the practitioner of Revolution there are several diverse opinions. There are those who maintain that he was no politician at all, but rather an unpractical dreamer who seldom came down to earth. There are those who, however, feel that he was a shallow opportunist ready to trim his ideological sails to meet each new political wind. Rudé holds that Robespierre's actions are best explained by the fact that he had the ability to choose the right moment and means of attaining his objective.

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L. F. SCHNORE, (ed.), *The New Urban History*, Princeton University Press, 1975, pp. IX-284.

The New Urban History, Edited by Leo F. Schnore, is a collection of essays originally presented at a conference on the use of quantitative techniques in urban history held at the University of Wisconsin in 1970. The topics and techniques are mainstays of recent research in urban economic and geographical phenomena. The primary distinction from recent work in urban economics is the perspective and a muting of possible policy implications. The crisis-orientated urban economist usually slights the origins and recurrent nature of the problem with which he is concerned. Technological change, particularly in the areas of communications and transportation always has resulted in changes in the structure of individual cities and the entire network of communities. The ascendancy of the one city or district has often been at the expense of another. The correlation between urban poverty and instances of ethnic or racial discrimination is of equally long standing. A policy-orientated, urban economist often slights the past and in the rush to propose cures fails to note how previous prescriptions failed. There is little need to defend the relevance of historical analysis of American urban development.

The nine papers are classified into three groups. The first concentrates on the structural problems of urban development. Allan R. Pred develops a model of interurban relationships for early XIXth century America. In the period prior to electronic communication, communities with favourable transportation characteristics were able to outpace their less favourably located competitors within the urban network because of their time advantage in receiving and distributing information. Cities served as conduits for the dispersal of new techniques and institutions in what Pred classifies as only a roughly hierarchical fashion. A model, with assumptions of economic rationality, is more implied than fully specified. The author does not present a rigorous statistical analysis of hypotheses. Martyn J. Boyden presents a most interesting extension to central place theory. He examines the development of the central business district, first in XVIth century London, then in the early major cities of North America mainly those along the Atlantic coast, and finally in the cities of the Western United States. Boyden's hypothesis is that a distinct central business district appeared at a progressively lower population threshold in each succeeding group of dominant cities. This conclusion could not be substantiated for cities of less than the first order. As society became more complex, external economies and economies of agglomeration appeared more quickly in the larger urban centres. Boyden's second observation concerns the lack of homogeneity in central business districts; that is, within one district it is possible to identify different sub-districts. Boyden credits this to the une-

wenness of the growth process and the durable nature of buildings, though this is a conclusion that can be easily derived from central place theory. If external economies result in clustering by commercial activities, they must also cause smaller scale clustering by specific activity as well; e.g. apparel or theatre districts within the central district. Data are marshalled to substantiate the hypotheses presented, though they are not analyzed in a rigorous fashion. Kenneth T. Jackson's work is a discussion of the history of urban geographical growth. He demonstrates successfully the early development of the process of residential and commercial dispersal in American cities. Using Philadelphia as an example, dispersal was evident in the early 19th century. The most obvious difference between XIXth and XXth century suburbanization has been the inability of the core city to expand its political control with the outward flow of population and economic activity.

The second section of papers is composed of studies of group adjustment to the urban environment. Kathleen Neils Conzen employs early Milwaukee to examine patterns of residence in a frontier city. In the period 1836-1860, Milwaukee was a totally new community with few established residents or neighbourhoods. Professor Conzen observed that high status families were centrally located with the periphery being inhabited mainly by low status families. Ethnic groups tended to cluster, with population increases leading to growth occurring in an outward direction along sectoral lines. Factor analysis suggested family status was as important as solid economic status in determining residence. Finally the author notes that a high degree of residential mobility was obvious. Zane L. Miller analyzes black migration from rural areas to five southern cities. Noting increased residential segregation with a larger black population, Professor Miller observes the development of a substantial black bourgeoisie and concurrent demands for better treatment within the context of the separate-but-equal doctrine. The southern urban environment, though increasing exposure to discrimination and providing generally sub-standard accommodations, did provide enhanced opportunities for the young black. Gregory H. Singleton observed the growth of fundamentalist churches in rapidly growing Los Angeles during the first third of this century.

The final group of essays provide economic analyses of urban-historical events. All three share an acceptance of standard economic assumptions concerning scarcity, profit maximizing behaviour of firms and utility maximization by individuals. Claudia Dale Goldin offers an economic explanation of the relative decline of urban slavery. The market for slaves is assumed to be competitive and slave owners profit maximizers. As the price of slaves increased, slaves were sold from urban employment to rural, where the demand was relatively inelastic. The author does not conclude that slavery would have disappeared from cities, simply that the slave market was flexible and behaved in a predictable fashion. Higher prices would

channel slaves into their most productive use. Professor Goldin presents an excellent, rigorous analysis. Robert Higgs, employing a linear production function and multiple regression analysis, tested the hypothesis that « an increasing linear relation existed between inventiveness and the proportion of population in cities ». Using cross-sectional and time-series data and allowing for the social structure of southern cities, Higgs substantiated this hypothesis and found it to be of greater significance than an alternative that a positive relationship existed between inventiveness and the production of the labour force engaged in manufacturing. In addition, Higgs observed that southern social structure could account for some of the lack of inventiveness only in the post 1900 period. The results are what would have been expected considering that cities have always been viewed as centres of innovation. Joseph A. Swanson and Jeffrey G. Williamson construct, but do not test, a model to explain plant location in the United States between 1865 and 1917. Locations with varying characteristics provide inputs — labour, capital and public services) at different prices. A relationship between factor price and population density, a proxy for size, is developed. Prices paid by workers for goods and services are a positive function of city size, interest on capital a negative function and public services at first a decreasing and eventually an increasing function of city size. If wages increase nationally and interest rates fall, optimal city size for plant location should decrease. Some predictive value for the model is claimed by the authors but only under their rather limited assumptions. The work is an interesting exercise and prototype, but ignores a multitude of crucial regional and technological variables, thus severely limiting its wide applicability.

The foreword, written by Eric E. Lampard, provides a defence of "Historical Accountants" and "Cliometricians" as well as providing a summary of each essay.

This volume presents an appropriate cross-section of issues and techniques applicable to urban research. Though the results of the research presented are unexceptional, the text would serve admirably as a case study book for a graduate course in statistical analysis for the social sciences.

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A. SERRA, *Breve Trattato delle cause che possono far abbondare li Regni d'oro e argento dove non sono miniere*. (Edited with an Introduction by C. Trasselli), Reggio Calabria, 1974, pp. 209.

This is a new edition of Serra's *Breve Trattato* which appears at a very opportune moment as the earlier edition by Graziani (Bari, 1913) has now

become very difficult to find while there are, as is well known, very few copies of the original version published in 1613. Using the information on Serra's biography which Luigi de Rosa has supplied, the economic and cultural environment of Serra's home town of Cosenza in Calabria is described in the Introduction. The town's cultural life was lively and it maintained close contacts with the Venetian publishing world. The town's economy was based on silk and was quite rich, although one must qualify this by pointing to a parabola of prosperity, the waning of which in the late XVIth century is indicated both by a number of important bankruptcies and also by the heavy imports of foreign goods. Trasselli argues that it was this experience which Serra lived through in his home town (De Rosa has shown that Serra was probably born in Dipignano, one of the "Casali" of Cosenza) which provided the basis for his first thoughts on economics.

Serra had the opportunity of publishing his *Breve Trattato* as the result of a polemic with a rich Neapolitan merchant named De Sanctis who in two brief tracts had argued that exchange operations could be controlled by governmental measures. Serra claimed on the other hand that exchange was subject to the laws of supply and demand, so perceiving that some form of balance of payments existed. Possibly from Botero, who had already written extensively on the subject, he derived the statement that it was essential not to export raw materials in order to import manufactured goods if the "labour" was to remain an asset for the country. Serra however went beyond this to condemn the investment of foreign capital on the grounds that the rents and profits which it created had then to be exported again in good gold or silver coin.

In the Introduction Trasselli argues that a similar situation had developed in Sicily and that several writers — who preferred unfortunately to remain anonymous — were at the same period dealing with the problems of foreign exchange and manufactures, and reaching conclusions similar to those of Serra.

In addition to the place which awaits it in the history of economic thought, the *Breve Trattato* also provides a document on the conditions of southern Italy and Serra should perhaps, as has been suggested, be ranked as the first of the economists who have dealt with the problems of the Italian south. But Serra should also be remembered as the pioneer of a method of research — he is in fact the first to have applied statistics to economic analysis in his research on the silver coined in Naples in the preceding decades, which makes him a precursor of the methodology of Cantillon.

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R. V. WELLS, *The Population of the British Colonies in America before 1776: A Survey of Census Data*, Princeton: Princeton University Press, 1975, xii+342 pp. Tables, notes, and index.

The past decade has witnessed a rapid growth of interest in the demography of British colonial America. Historians, working with the technique of family reconstitution, are providing a fast expanding store of data on colonial populations. Most of this work has a local focus. Reconstitution requires the painstaking assembly of family histories from lists of vital events. It demands that the student devote attention to a small, well-defined area covered by a register. It thus discourages generalization across larger regions than a parish, town, or county. Furthermore, both because of the biases of early American historians and the availability of detailed registers, scholarship has centred on New England, although very recently studies of other areas have begun to appear. In this book Robert Wells attempts a broad and regionally balanced demographic history of British America before the Revolution, based on 124 censuses covering 21 colonies stretching from Newfoundland to Tobago.

The book fails in its task because of a decision to focus on census returns to the exclusion of other types of population data. Colonial censuses vary in quality, were not taken at all in some colonies, rarely in others, and frequently in a few, and differ in detail. Their variability and the failure to use other evidence deprive the book of unity and focus, leaving both author and reader at the mercy of colonial officials. Wells tries to impose order, but the sources proved intractable. He is unable to pursue issues systematically, to test hypotheses, or to follow arguments to their logical conclusions. Wells has created a useful reference tool, extended our knowledge of colonial populations in many particulars, and opened new questions. But he has missed an opportunity to survey early American demography and tell us where we stand and where we ought to go from here.

The book is divided into three sections. It opens with an account of how and why censuses were taken that is, along with a bibliography of the returns, one of the most valuable parts of the study. The first section also reflects upon the reliability of the censuses, concluding that they are adequate if not perfect sources. While my work with Chesapeake records confirms his judgement, Wells does not build a strong case. He relies on general considerations of the manner and purpose of counting when what is needed is a check on internal consistency, an assessment of whether patterns are reasonable, and a comparison of the returns with other records for confirmation or contradiction. Since the value of the book rests on the quality of the censuses, failure to build as strong a case as possible for their reliability is serious indeed.

The second section, which forms the bulk of the book, provides a detailed account of each colony for which a census survives. Wells moves

from north to south with an analysis of the size, growth, and geographical distribution of the population, the racial mix, extent of servitude, and age-structure of the inhabitants, their sex and child-woman ratios, and the size and composition of their households. While Wells assembles a vast amount of data and raises interesting questions, the section is a disappointment. No theme holds the parts together and the writing is repetitive, mechanical, and difficult to follow. No maps are included and the reader is often lost in detailed descriptions of regional patterns. The censuses are not printed, forcing those interested in questions Wells ignores to turn elsewhere. Most important, Wells fails to treat the returns with the care they deserve, apparently often relying on transcripts, and at times on transcripts of transcripts, instead of turning to the original in each case. In consequence, at least for the region with which I am familiar, the book is marred by numerous errors, most of them minor, a few seriously misleading. With the Maryland returns, for example, Wells misdates censuses of 1700 and 1707 as 1701 and 1708, fails to note the omission of Baltimore County from the 1700 return, and misses a minor transcription error in the 1704 census. More seriously, he does not catch an inconsistency in the 1707 return, apparent in the original but not in the printed version, which indicates that the census substantially understates the colony's population. In Virginia his record is no better. Here the most serious error is in the use of a "census" of 1703 found in Oldmixon's *British Empire in America*. Oldmixon took the return from Robert Beverly's *History and Present State of Virginia*, where it is dated 1702. Beverly compiled the census by combining counts of persons not tithable taken in 1701 with what appear to be tithable lists of 1702. The 1701 census is itself unreliable: the count of tithables is low by several thousand, while the figures for those not tithable repeat totals from a 1699 census for 9 of Virginia's 23 counties. Thus, Wells offers a history of Virginia population at the turn of the century based on a 1699 census that seems reliable, a 1701 census that is inaccurate, and a 1703 census that is fictitious. No wonder he is puzzled by the pattern! Perhaps Wells has been more careful in dealing with the materials of other regions — his work with New York returns suggests that he has — but his treatment of Chesapeake censuses does not inspire confidence.

The concluding section brings the accounts of the particular colonies together in an effort « to describe, explain, and consider the significance of intercolonial differences and similarities in population trends ». (ix) This too is flawed. Wells is insufficiently sensitive to temporal patterns and often makes inappropriate comparisons, especially when discussing the course of population growth. His failure to consider other evidence and to integrate the work of demographic historians into the analysis prevents a thorough exploitation of the censuses. However, the section does possess organizing

themes (chiefly the differences between mainland and island colonies) and it does bring the evidence of the returns to bear on important questions in population history. But these merits are not sufficient to overcome the sense of disappointment one has with this book. In a series of articles on Middle Colony Quakers, Wells demonstrated that he is a demographic historian of talent and industry. It is unfortunate that this book does not measure up to the standards of his earlier work.

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M. WILKINS, *The Maturing of Multinational Enterprise: American Business Abroad from 1914 to 1970*. (Harvard Studies in Business History, No. 27). Cambridge: Harvard University Press, 1974, pp. xii + 590. \$ 22.00.

This volume, the sequel to the author's 1970 study, *The Emergence of Multinational Enterprise: American Business Abroad from the Colonial Era to 1914*, describes and analyzes the overseas expansion of United States corporations from the onset of World War I to the beginning of the Nixon era. Like its predecessor, the work is based on extensive research in the United States and abroad. Dr Wilkins has probed deeply in both public and private archives, investigated a wide variety of published and unpublished materials, searched the secondary literature, periodicals, and newspapers, as well as interviewed scores of corporate and government officials. No one has made so comprehensive a study of such widely different types of evidence to reconstruct the story of American business abroad during the last six decades.

World War I, as everyone knows, transformed the United States from a debtor to a creditor nation. The wartime change in America's international position was due principally to the precipitous decline in the amount of foreign-held investments in the United States (chiefly portfolio) and to American loans to the Allied governments rather than to an increase in direct United States investments abroad, which grew by some \$ 1.2 billion between 1914 and 1919. Far more important than this increase, in Wilkins' view, was the fact that the war made it possible for American business to enlarge significantly its direct investments not only in the western hemisphere but throughout the world. By the time the armistice was signed, Wilkins writes, « most American businessmen could look to the promise of international investments ».

That promise was realized in the next decade, with a great « surge of U.S. business worldwide ». In 1922 the value of American investments in Canada surpassed those of Great Britain, and by 1929, asserts the author, « it seems probable that . . . for the first time in history, U.S. investments exceeded those of the British in Latin America as a whole » (p. 155). During the 1920's the total book value of United States direct foreign investments nearly doubled, from \$ 3.88 billion in 1919 to \$ 7.55 billion in 1929. By the latter year some 62.5 per cent of these investments were concentrated in four regions of the world, distributed as follows: South America (\$ 1.72 billion), followed by Canada and Newfoundland (\$ 1.66 billion), and Europe (\$ 1.34 billion). Neither the long depression of the 1930's, which proved so discouraging to many companies doing business abroad, nor the decade's resurgent nationalism, militarism, and political unrest caused most American corporations to abandon their international operations. By 1940 the total book value of United States direct investments overseas still stood at about \$ 7.0 billion, some \$ 550 million less than what it had been in 1929. United States direct foreign investments, Wilkins shows, survived the world economic crisis of the 1930's « far better than did the nation's portfolio holdings » (p. 205). The respite in American business expansion abroad lasted only until the close of World War II, when there followed the greatest outflow of all time. Between 1946 and 1970 direct United States foreign investments soared from \$ 7.2 billion to nearly \$ 78.2 billion. With the exception of a few years (1956-59 and 1961), when American holdings in overseas petroleum properties led all other categories, manufacturing accounted for the largest share of United States overseas direct investments. Most of these manufacturing investments abroad, measured in terms of book value, were in European enterprises (almost 31.4 per cent), followed by those in Canada (29.1 per cent) and Latin America (18.9 per cent). Asia, Oceania, and Africa, in that order, trailed far behind. Dr Wilkins' carefully culled statistics reveal that in 1970, as in earlier years, American business has been « most attracted to countries where the living standards are high and the growth potential great » (p. 438).

But this work does far more than document the huge increase in America's direct investments abroad, important as it is to have reliable and up-to-date evidence of this type easily available. The great merit of this study is the way the author uses quantitative and traditional evidence to explain the nature and patterns of American business expansion abroad, the geographical areas and economic sectors into which it moved, and the impact of direct overseas investments on both the United States and the host countries. Relations between government and business, both at home and abroad, constitute an important part of the study, and the author's analysis of this controversial topic is particularly well-informed and balanced. All in all, the study illustrates how business history benefits from and

enriches other historical specialties. Dr. Wilkins' work adds depth and new insights not only to the economic and business history of the post-1914 decades, but to the political, social, and diplomatic developments of those years as well.

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J. M. WINTER (ed.), *War and economic development. Essays in memory of David Joslin*, Cambridge, Cambridge University Press, 1975, 8°, VIII - 297 pp.

David Joslin died in 1970, as the result of a heart attack, at the age of 45. He had held the chair of economic history in the University of Cambridge since 1965, when he succeeded Professor M. M. Postan. Indeed, his whole career had been centred in Cambridge, where he had been previously an undergraduate, a research student, a fellow and tutor at Pembroke College and a University lecturer. He was a devoted and stimulating teacher, an able administrator, who was much involved in College and University affairs, and an outstanding scholar, who made important contribution to the history of banking in England and in Latin America. His premature death, at the height of his powers, was a sad loss to economic history and to the many friends whom his generous personality and his brilliance in conversation had attracted. A number of them have contributed to the present volume of essay which is dedicated to his memory.

The volume is organized around the theme of the impact of war on economic development, a major problem which economic historians have too much neglected. The essays range in time from the XIIIth century to World War II and, although most of them are concerned with England, three of them deal with aspects of Dutch and German history. The main task of the reviewer of such a collective work can be only to draw attention to the various subjects which are treated and to comment on the most interesting contributions.

In a short, but stimulating introduction, the editor, J. M. Winter, surveys the historiography of the relationship between war and economic development (he has also compiled a select, but substantial, bibliography of works which have dealt with it). One of his points concerns the distinction between an "internal" and an "external" approach to the subject, the former concentrating on the short-run impact of war, such as the mobilization of resources and the changes in economic activity and policy, the latter concentrating on the long-run effects and their relationship to economic and social trends which are in operation before hostilities break out.

To the first category belong three essays which deal with war taxation and its consequences. E. Miller analyzes the severe increase in the tax burden which took place in England in the late XIIIth and early XIVth centuries (with his emphasis on the 1290's), one of its effects being the stimulation of the native woollen industry, thanks to a *de facto* protection resulting from heavy duties on wool exports. G. R. Elton describes how the Tudor monarchs succeeded, from the 1530's onwards, in levying, during peace-time, direct taxes, which previously had been imposed for war purposes only. In an important paper, S. Schama shows that the crushing burden which war and occupation inflicted upon Dutch finances, during the times of the French Revolution and Napoleon, made imperative profound changes in an antiquated financial and political system. Those changes laid the foundations for the modern Dutch state, which emerged after Napoleon's fall.

Three other papers are concerned with the problem of economic mobilization in Britain during the early XXth century wars, its techniques and impact upon industrial progress. C. Trebilcock demonstrates that the breakdown in munitions supply to the British army in 1914-1915 had a striking precedent in the failure of industrial mobilization during the Boer War, the lessons of which had not been assimilated by the civilian and military authorities. R. and K. Macleod study the intervention of government to develop, during World War I, the science-based optical industry, concluding that it liberated an unexploited capacity for technological innovation, but that the large industry which was thus created was fragile. D. C. Coleman narrates the "Dope Scandal" of 1918 — dope being a chemical which made resistant linen fabrics used for the outer skin of aircraft and balloons. Its manufacture in Britain was started by a consortium in which the Dreyfus brothers, from Basle, and a Canadian financier, W. G. Morden, played a prominent part, and it resulted eventually in the foundation of British Celanese Ltd (the later stormy financial history of this firm was not unrelated to its origins). The author makes some fascinating remarks on the « moral dilemma of the capitalist economy in war » and « the typical relationship between governments and business, compounded of patriotism and profits, of opportunism and muddle », which this curious story illustrates. There is also a short paper by J. Lee on a discussion about war-time agricultural policy, which took place in Germany in 1912.

The other papers deal mostly with the impact of war in the long run. G. Parker studies the "economic costs" of the Netherlands' revolt against Spain and reaches pessimistic conclusions: the Southern Netherlands were ruined for two centuries, Spain and Portugal also suffered heavily, while there is no proof that the economic growth of the United Provinces was stimulated by this costly and protracted warfare. P. Deane has a quite different point of view in her paper on the "Napoleonic" wars and British industrialization: the financing of the wars did not involve a significant

redistribution of income to the detriment of groups which were developing industry, heavy public expenditure was more growth-promoting than growth-retarding and altogether the wars were not a serious brake on industrial progress. The paper, however, is quite short and does not fully discuss this important problem.

On the same period, P. Mathias has made an excellent contribution to a more limited but interesting topic. During the French wars, the medical services of the British Army and Navy greatly expanded and a good deal of research and experiment was devoted, for obvious reasons, to the prevention of disease among soldiers and seamen. Despite continued ignorance about the basic origins of disease and the ways to cure it, some progress was achieved in understanding the relation between disease and environment and this had "spin-off" effects at the national level on hospital practice, hygiene and preventive medicine. Finally, J. Harris criticizes the accepted view of the famous Beveridge Report of 1942. The committee which prepared the report was created in Churchill's government by accident. Beveridge had not wanted its chairmanship and his appointment was a way for ministers, who disliked him, of "kicking him upstairs". He contributed little that was original to the report, which was mostly a synthesis of secondary opinions, but, as a skilled manipulator of the media, he gave the report tremendous publicity and aroused popular interest. The Coalition government, though the report did not express his views in the field of social policy, was eventually forced to commit itself to a large-scale reconstruction of social services. Many of the report's proposals were eventually dropped by the post-war Labour government.

There is clearly much variety in the scope and thoroughness of these essays. The most ambitious in their subject-matter are not always the most convincing, while some of the monographic essays are footnotes to the general theme of the book. Moreover, one could often wish for a firmer grasp of the methodological and theoretical problems involved in the study of « war and economic development », and especially of the difficulties in isolating war as a factor of long-term economic and social change. The resort to econometric techniques might have been both possible and useful in some instances. It is also a pity that the numerous footnotes have been relegated to the end of each chapter, and there are many mistakes in the spelling of foreign names and words (p. 116, General Augereau has become Augureau). However, there is much that is new, useful and stimulating in this book, which is a worthy tribute to the memory of a brilliant economic historian.

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