
REVIEWS OF BOOKS

M.M. BULLARD, *Filippo Strozzi and the Medici: Favor and finance in sixteenth-century Florence and Rome*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press, 1980, pp. x, 197.

Filippo Strozzi the younger (1489-1538) is best known as a tragic hero of Florentine republicanism, a suicide in Duke Cosimo's prison after failing to overthrow Medicean tyranny. Strozzi's exemplary end — he translated Polybius as a prisoner and modelled his suicide on Cato's — commended him to Italian nationalists and romantic dramatists. Professor Bullard's excellent study resurrects a more prosaic but no less significant figure — Strozzi as the international financier whose resources and ingenuity were dedicated to the ambitions of the Medicean popes, Leo X (d.1521) and Clement VII (d.1534). In fact, Strozzi only took up the banner of Florentine liberty when he lost his powerful patrons. For most of his life, he was an obedient satellite of the Medicean planets, an unscrupulous and successful subverter of Florentine independence.

Strozzi entered the Medicean orbit in 1508, when he married Clarice, niece of the future Leo X. The alliance initially caused great difficulty for Strozzi, for not only were the Strozzi old opponents of Clarice's clan, but the Florentine Republic had forbade dealings with the exiled Medici. But Strozzi's gamble paid off in 1512 when his wife's uncle gained power in Florence and in 1513 was elected Pope. Bolstered by his mother-in-law, the formidable Alfonsina Orsini, and by his brother-in-law Lorenzo, Strozzi rose to eminence. By 1515 he enjoyed unrivaled prominence in Lorenzo's government in Florence and had established a flourishing bank in Rome.

As a Medici *parente*, Strozzi gained access to inner circles in Florence and

Rome. His own talents and initiative translated that advantage into wealth and power. In Florence he effectively controlled the Depository of the Signoria, the central payment agency of the state, which was a crucial link between the principal credit agency (the *Monte Comune*) and the most important executive council (the *Otto di Pratica*). The vice-depositor was Francesco del Nero, Strozzi's henchman and "perhaps the brain behind Filippo's financial successes" (p. 132n). In Rome, Strozzi was the Depositor General of the Apostolic Chamber. The *Camera Apostolica* was the administrative wing of the Vatican, and as the central repository of cameral monies, the Depository General performed such banking duties as collecting revenues from annates and paying out salaries for curial officials and military personnel. Like the Depository of the Signoria, the Depository General was technically merely a passive channel for transferring monies. But inasmuch as there were rarely enough funds to meet payrolls, the Depository General extended credit from his bank to keep operations solvent; in return, the bank loans were secured by anticipated incomes of papal offices. At various times Strozzi had title to 258 venal offices and held nine administrative positions, including the lucrative posts of treasurer of Urbino and distributor of grain in Rome.

The heart of Bullard's book is a virtuoso examination of Strozzi's manipulation of his Florentine and Roman offices in the service of the Medici. Leo and Clement needed substantial funds to pursue their ambitions in the Italian wars, but the resources of the church were too slight and irregular for that purpose. Hence, through the credit mechanisms of the Strozzi bank, the Florentine Depository became an adjunct to the Roman Depository General. As the chief agency through which monies flowed to papal and Florentine armies, the Depository of the Signoria provided camouflage for extensive Florentine subsidies to the Pope, whose debts were thereafter written off on the commune's books. For example, from June to October 1527, Del Nero sent 458, 428 ducats to papal forces in Lombardy, and almost all of that astonishing sum was extracted from Florentine citizens and never repaid. Doctored account books insured that the true costs to Florence of Mediccan foreign policy remained unknown. Bullard makes it clear that Florentines paid dearly for the glory of having one of their own in the Vatican.

Strozzi's resources and legerdemain were indispensable to the Mediccan Popes. By 1524, however, Strozzi was reduced to only one mighty patron, and, as he admitted, his prosperity was "contingent on Clement's living breath" (p. 153). Clement VII expired in 1534, and Strozzi's fall was precipitous. A mob sacked his grain warehouses in the Trastevere; the new Pope declined to assume all of Clement's debts to Strozzi; and the Depository General was assigned to a new favourite, as were Strozzi's other positions. The new Medici rulers in Florence detested Strozzi, and the latter soon donned the novel mantle of a defender of liberty, only to have his motley army defeated by Duke Cosimo in 1537.

Bullard has demolished the historical stereotype of Filippo Strozzi, substituting fraudulent bookkeeping and currency manipulation for romantic gestures and heroic refusal. More than that, however, she has revealed for the first time the great price, both financial and political, that Florence paid for Medicean grandeur. Along with recent studies by Felix Gilbert and Peter Partner, her book provides a basis for understanding the intricacies and significance of papal financial policy in the Renaissance.¹

Robert Finlay

Institute for Advanced Study, Princeton

R.A. BURCHELL, *The San Francisco Irish 1848-1880*, University of California Press, Berkeley, 1980, pp. x + 227.

The last decade or so has seen the proliferation of studies of Irish immigrant life in nineteenth-century American cities. Short on folklore and oral history, but buttressed by frequency distributions and crosstabulations of raw censal data and by quotations from the contemporary local and ethnic press, such studies have made an important contribution to social history. Perhaps by now, though, the list (Boston, Detroit, Newburyport, Holyoke, Philadelphia, New Orleans, etc.) is long enough for individual case-studies to have reached the stage of diminishing returns? Comparative rather than purely local assessments of the Irish experience are called for at this point, and R.A. Burchell's work has the advantage of being always at least implicitly comparative. Both he and his blurb-writer evidently see the San Francisco Irish as exceptionally successful: their early political successes, their upward social mobility as inferred from manuscript censal data, and the vibrancy of Irish culture and communal life in the Bay City, are stressed to show "San Francisco's deviation from the norm of Irish experiences in the United States" (page 7).

Burchell does a good job of describing the growth of post-1850 San Francisco, and gets at least as much mileage out of his sources as earlier researchers into other city histories got out of theirs. The detailed discussions of individual politicians' careers and ethnic newspapers' editorial lines, though hardly scintillating reading for the non-specialist, are to be expected. Much is made of San Francisco's flirtation with Irish mayors and other elected representatives. The top brass of the local Catholic Church tended to be even more heavily Irish. The story of the common man must for the most part be inferred from occupational tables, and these confirm that from the start the Irish were more likely to be found in skilled and semi-skilled work in San Francisco than

¹ FELIX GILBERT, *The Pope, His Banker, and Venice*, Cambridge, Massachusetts: Harvard University Press, 1980; PETER PARTNER, "Papal Financial Policy in the Renaissance and Counter-Reformation," *Past and Present*, 88 (1980), 17-62.

in most other American cities — though New York beats all comers in this. Where the book disappoints, however, is in its failure to explain clearly why the San Francisco Irish were, by contrast with elsewhere, “comparatively successful and fortunate”. The impression is given that the city was at one end of spectrum that stretched from east to west: nativist prejudice and discrimination were most serious in New England, and ineffective or almost absent in California. Twice (pp. 4, 190) we are informed that native Americans did not “shed their prejudices at the Mississippi or the Isthmus of Panama”, but no alternative explanation for a spectrum is given. Surely it is more reasonable to argue that the speed of expansion of a city, the recency of its creation, and the share of the Irish community in the total population, were important considerations, quite apart from geography? Surely, too, the character of the particular Irish immigration had some bearing? On all these counts, the Irish in San Francisco had a great deal going for them. From the start they were numerically a major force in the city, and by 1880 Irish-born and second-generation Irish accounted for almost one-third of the entire population. At the outset, too, it seems that the Irish who came to San Francisco were not the Famine immigrants who flooded the eastern cities: many of them came from Australia, or had already spent several years in the eastern United States. Certainly, none of those who achieved prominence in politics — one of Butchell’s key indices of ‘success’ — was a recent immigrant. San Francisco, besides, was one of the fastest-growing cities in the nation, and it would have been surprising had the Irish not shared in that success.

As a sampler of the ‘new urban history’ this study is well up to par, though it breaks no new methodological grounds. As an introduction to the study of the post-Famine Irish in California, it will unfortunately have to compete for attention with P.J. Blessing’s dissertation (*West Among Strangers: Irish Migration to California, 1850 to 1880*, University of California, Los Angeles, 1977), and J.P. Walsh’s contribution in the recently published *Ireland and America 1776-1976* (Greenwood Press, 1980, edited by D.N. Doyle and O.D. Edwards), which argues much along the same lines.

CORMAC O’ GRÁDA

University College, Dublin and
University of British Columbia

N.K. BUXTON-D.H. ALDCROFT (eds.), *British Industry Between the Wars: Instability and Industrial Development, 1919-1939*, London, Scolar Press, 1979.

Little more than a decade ago, Donald Winch wrote of the interwar period in Britain, “It is only within recent years that the experience of this era has faded as an active influence on our economic dispositions.” In retrospect this judgement appears premature. The burning issues of the interwar years — fixed versus floating exchange rates, free trade versus protection, the efficacy of activist monetary policy, the effect of unemployment insurance

on the level of unemployment – are again the issues of the day. The quantity of new research on this period is evidence that interwar Britain increasingly is seen as a laboratory within which the effects of government policies may be studied.

Prominent among the issues now viewed as distant echoes of Britain's interwar experience is the crisis in the industrial heartland. In the 1920's and 1930's that heartland included the Northern regions and Wales, where heavy dependence on the ailing staple trades (iron and steel, coal, textiles and shipbuilding) gave rise to unemployment rates well in excess of the national average. At that time the dramatic contrast was with the Midlands and South, where the growth of new industries stimulated regional employment. At the time of writing this, it is the industrial West Midlands, today heavily dependent on industries which were new in the 'thirties' (vehicles, electrical engineering, and, of course, metal manufacture) that are suffering. Unemployment in the West Midlands is expected to rise at double the national rate until 1985, while earnings there already are below the national average. The comparison with the interwar era is obvious, and frequently drawn.

For this and other reasons, the appearance of *British Industry Between the Wars* is timely. This book is a collection of essays by different authors on specific industries. The volume exhibits the usual strengths and weaknesses of the genre: despite a lack of unity, it contains individual studies which are admirable in scope and detail and promise to serve as valuable reference works for future scholars. Buxton and Aldcroft have provided a unifying theme around which the evolution of individual industries can be organized: the relationship of economic instability to industrial development. This was the theme sounded by G.C. Allen in his classic *British Industries and their Organization* (1933). Here it features both in the book's subtitle and in Buxton's editorial introduction. Unfortunately, not all the authors have organized their contributions to reflect the editors' central concern.

It must have been tempting to follow tradition by dividing the industries of interwar Britain into two categories: the contracting staple trades (cotton, and woollens, coalmining, shipbuilding, iron and steel) and the expanding new industries (chemicals and rayon, motor vehicles, aircraft, electrical engineering). Except for ordering the studies in this sequence, the editors resisted the temptation to harp on this distinction. With good reason, for each contributor goes to some length to argue that the industry under scrutiny does not fit the mould of either the dynamic new or the decaying old trade. T.R. Gourvish, in his essay on mechanical engineering, makes this point with particular force. Each chapter contains examples of missed opportunity and exceptional achievement. Above all, the book succeeds in conveying the remarkable diversity of industrial experience.

Contemporaries were fully aware of the problems facing British industry. Therefore the contributors to the present volume have a vast array of official

inquiries upon which to draw, including those by the Committee on Industry and Trade (1928), the Board of Trade, the PEP, the League of Nations and the International Labour Office. Since much of this material has been available for some years, we have reason to ask what is added to our knowledge by these new studies of individual industries. How do they compare with Allen's work, or with two collections published by the British Association for the Advancement of Science, *Britain in Depression* (1935) and *Britain in Recovery* (1938), to which the present volume bears a striking resemblance?

First of all, the book contains few new findings. J.R. Parkinson presents valuable new estimates of productivity growth in shipbuilding, and Neil Buxton summarizes the results of his research into the relationship between mechanization and productivity in coalmining. Other studies are noteworthy for their application of modern theories of industrial organization to the interwar period. J. Harrop approaches the history of rayon in Britain in terms of the modern triad of industrial market structure, conduct and performance. By examining innovation, diffusion and imitation through the theory of the product cycle, he succeeds in placing the industry's development in an international context. W.J. Reader's study of the British chemical industry focuses on the relationship of strategy to structure; using the approach popularized by Alfred Chandler, Leslie Hannah and others, he relates the organization of the large enterprise to decisions concerning merger, diversification and foreign investment.

The remaining contributors generally follow in the tradition of earlier work by placing developments squarely in a supply and demand framework. They begin with factors affecting the capacity of British industry (such as productivity growth and investment), add the impact of rising competition abroad, and the trace out the growth and fluctuation of demand for British products. While few of these studies break new ground, they provide compact summaries of the available evidence on the changing composition of supplies and demands. W. Warren's chapter on iron and steel contains a particularly effective account of the development of industry during the interwar period. His argument benefits greatly from the attention paid to the earlier history of the industry and from the range of statistical evidence on supply and demand which is marshalled and presented in a readily accessible format.

One might quibble that some recent work of particular relevance, such as Foreman-Peck's analysis of scale economies in motor manufacturing, case studies by Peter Payne and others on the history of iron and steel firms, and von Tunzelmann's estimates of the overall importance of Britain's new industries in the interwar period, find no place in these pages. While much remains to be said about the development of industry in interwar Britain, *British Industries Between the Wars* provides evidence that good work is currently in progress.

BARRY J. EICHENGREEN
Harvard University

M. CASSANDRO, *Il Libro Giallo di Ginevra della Compagnia fiorentina di Antonio della Casa e Simone Guadagni (1453-1454)*, (Istituto Internazionale di Storia Economica F. Datini: Documenti, 3, Prato 1976, pp. 5-714.)

As J.F. Bergier points out in his introduction, the publication of this account book kept by a Florentine merchant banking house in Geneva in the mid-XVth century will greatly extend our knowledge of the activities of the Florentine merchants resident in Geneva and of their international contacts in Italy (in Florence, Rome, Milan, Venice and Genoa), in Avignon and Montpellier (and at the fairs of Pezenas and Montagnac), as well as in Bruges, Antwerp and London. There is no need to rehearse the vast network of contacts and exchange operations involved since these are described in detail by the editor in his lengthy introduction to the text (pp. 5-197), which is also accompanied by a number of analytical and summary tables. It should be noted, however, that Cassandro provides a detailed reconstruction of the fortunes and economic activities of the two families concerned (the della Casa and the Guadagni), and that his own description of the varied activities of the Geneva Company drawn from the records of the Italian merchants fully bears out the picture already drawn by Bergier in his well-known study of Geneva and the European economy during the Renaissance period, as well as the more detailed studies such as J. Combes work on the Languedoc fairs (1958) and B. Caizzi's description of the commercial relations between Lyon and Geneva as a form of 'economic cold war' (1953).

At the same time Cassandro's treatment of banking operations, and in particular exchange and exchange flows, creates certain problems, since the constant reference to the work of R. De Roover (although the research is in more general terms clearly inspired by the teaching of F. Melis) tends to obstruct the analysis and diminish the true importance of the developments studied. De Roover's view, which constitutes Cassandro's working hypothesis, can be summarised as being that the *prenditore* was 'first and foremost a borrower'. But this is only a half truth, and conceals a real contradiction. On one hand, it portrays something that is perfectly true — that exchange operations 'were not simply another form of banking activity'; with the result that they should not be seen only in their literal sense but also as 'an authentic financial operation' — yet on the other, it immediately reduces this important and well established financial mechanism to the simple and primitive level of a 'usurious loan' (p. 125), thereby completely overlooking the very modern type of credit systems tied up in the exchange operations. The contradiction becomes clear the moment that Cassandro tries to apply De Roover's formula, with the result that the exchange operations are reduced to a series of bilateral operations between individual markets which have little inherent consistency, but are described at one moment in financial terms (the Geneva-Bruges link 'lacked any possibility of coordination with commercial operations') and at another in relation to international payments (between Geneva and Florence

'there was a currency flow in the opposite direction'). But such an interpretation cannot be made to fit the situation which Cassandro himself is well aware of, whereby the movement of currency exchanges was effected (for the greater part by the circulation of invoices, transfers and balances, so that in the end only the outstanding credits or debits were ever settled in cash' (p. 125), and when the key to all these operations lay in the existence and the 'enormous importance' of current bankers' accounts which constituted the 'technical means on which all the operations of the bank were ultimately based' (p. 139).

What needs to be done, in short, is for the currency flow and the commercial balances to be related to one another, without overlooking the parallel need to set the bilateral relations between specific markets in the context of the broader structure of the international market. If this is omitted, then it still remains possible to interpret the activities of the medieval banks in terms of merely speculative or 'usurious' operations, which has the defect of leaving undefined and unidentified precisely those characteristics which lay at the heart of the whole system — in other words the complex credit functions and systems which inspired all international banking operations even in the hands of the smaller houses. These operations were essentially to do with credit since they involved a multiplicity of partners and were profitable.

If this is neglected, then the history of the medieval banking system risks becoming purely descriptive and largely superficial. Following his chosen premise, Cassandro for example claims that 'when it comes to reconstructing the overall volume of currency exchanges between two markets it is of no importance whether the operations are effected directly or by means of a third market' (p. 106). This completely rules out the possibility of assessing degrees of interdependence, with the result that it becomes impossible to reveal either the degree of articulation and the supra-regional structure of the international money market utilised by individual companies or the ways in which different bilateral relations were bound together in a greater organic system by the various operations of mediation and profit. Another indication of the inadequacy of the line of inquiry which follows from the position adopted by De Roover is the fact that exchange operations are treated largely in terms of profit and loss, supported by the evidence from individual transactions and in particular those which on account of their speculative character come closest to the 'usurious' model, while the way in which they both depended on and influenced the mass of currency movements — the direction of currency flows, and the related phenomena of the rise or fall in trading in particular currencies between international markets — is completely overlooked. And this is true even in the work of authors like Cassandro who are well aware of the complex network of the 'dispositions' (p. 135) and also the way in which the mechanisms of supply constituted the certain profit margin in the overall profit account (p. 124).

ALFONSO LEONE
University of Naples

S.B. CLOUGH, *The Life I've Lived*. With an introduction by Peter Gay, Washington: University Press of America, 1981, pp. 285.

This lively autobiography offers, amongst an *abondance de richesses*, a salutary reminder of how relatively recent the serious study of European economic history is in the United States. In 1940 Shepard Clough had been one of the "founding fathers" of the Economic History Association and its *Journal of Economic History*. Yet in the immediate post-World War II years, and this after the earlier publication of his authoritative *France: A History of National Economics, 1789-1939* (1939), Clough still had to make some unsettling shuttles between the departments of economics and history at Columbia University to accommodate his own interests in European economic history and those of students who wished to work in that field. By 1968, however, as president of the Economic History Association, he could look back on the maturing of a whole generation of economic historians, many of whom had been his students. But he had also to turn his attention to the schism in that organization between cliometricians and the more traditional or "qualitative" historians. He showed there was no reason why the two branches should not enrich each other rather than quarrel, and he reminded his listeners that "...economic history had begun as an adjunct to political history... it had gradually developed a *raison d'être* of its own in dealing with the way men provided for their material needs and drives and the problems they encountered in doing so, and that it had to include considerations from man's social, demographic, political and intellectual sides as they impinged on our economic lives" (p. 199).

I quote those remarks not to revive that old quarrel or because they were a laudable attempt to pour oil on troubled waters, but because they so appropriately reflect Shepard Clough's philosophy of history. This philosophy has a fuller development in two of his more notable works, *The Rise and Fall of Civilization* (1951) and *The Basic Values of Western Civilization* (1970). But it also accounts, he tells us in his autobiography, for the variety of his monographic work which, in addition to major contributions to economic history, has covered such diverse topics as Flemish nationalism, Italian fascism, philanthropy and the diffusion of technology. He confesses to an "intellectual Wanderlust" and wonders whether he might have acquired "more fame and fortune" if he had concentrated in one subject. He concludes, however, that "a professional rolling stope has more fun than a moss-covered one" (p. 198). Happily, I would add, for scholarship and his students, whom he has likewise encouraged in a variety of fields.

Research, teaching, writing, offices held are, of course, the stuff of life of the scholar-teacher, but they are not the whole story. This is "the life I've lived," and what it says on the label is what you get in the package, especially from an old-school Yankee. And so there are frank appraisals of colleagues good, bad and indifferent and lasting friendships with a variety of scholars at home and abroad. There is trenchant and witty criticism of bu-

reaucracy in academia and government. A chapter on a World War II stint with the Department of State captures the mood of the underside of Washington bureaucracy during that period better than anything I have read. And a chapter on the student riots of 1968 conveys better than anything I experienced or read what these meant to professors of Clough's generation. The pressures of climbing the academic ladder are also treated with candor. There is a tendency to think there are some unique ones today, including spouses who have research and teaching interests of their own. But *plus ça change...*; Shepard Clough has experienced them all, academic "booms" and "depressions" included. And he tells it all with verve, wit and a crystal-clear narrative.

Some of my favorite pages, however, are those devoted to the author's upbringing in a small New Hampshire town during the first two decades of this century. Here reminiscence becomes a part of history. That way of life and the talent and industry that supported it are gone or — the Clough family is an example — drained away to the city. Indeed, as I followed the author into the other periods of his life to the present, I found myself thinking "this is all history." No question about it, *The Life I've Lived* is a very warm, human and personal document. All the lumps and joys of professional and domestic life are in it. There is courage, sadness and great happiness, but somehow a significance larger than a life is conveyed. *The Life I've Lived* is also very much an account of what it meant to have been an historian and teacher in our times. Thomas Carlyle once wrote that "a well-written life is almost as rare as a well-lived one". Shepard Clough with his autobiography has effected the conjuncture that Carlyle seems to have sought.

CHARLES K. WARNER
Cambridge, Massachusetts

D.A. FARNIE, *The English Cotton Industry and the World Market, 1815-1896*, Oxford, Clarendon Press, 1979, pp. 399.

Like some other recent books on the English cotton textile industry, this work is more a collection of essays on related topics than a fully integrated, cohesive work. The vastness of the subject matter and the great deal of work already done on it makes it almost inevitable that new contributions to our knowledge of this industry and its role in the British and the world economy can only be achieved by the intensive study of fairly limited topics. Thus, a truly comprehensive history of the whole industry, or even of a major facet of the industry, would be unlikely to contain much new scholarship.

The topics discussed in this book are the role of the cotton industry in the British industrial revolution, the localization of the industry, the growth

of the world market for cotton goods, the great cotton famine that occurred during the American Civil War, the "Great Depression" (1873-96) and the rise of foreign competition, the development of joint stock organization in Lancashire, the emergence of the "Oldham Limiteds" and the development of specialized weaving firms.

All of these essays are of value, both to economic historians in general and to researchers working specifically on this industry. They are based on a thorough reading of available sources, both original and secondary. As a result they are very informative. Equally important, they display insight and intelligence. Thus the reader will gain in understanding of the history of the industry and its relationship to British society. Even if he does not agree with all of Farnie's observations and interpretations, the essays will give him much food for thought. Finally, the book is very well written, making it a pleasure to read.

The single most interesting and thought provoking observation made by the author is his claim that the cotton industry, especially compared to the native woollen industry, was "an unnatural, exotic and speculative graft upon English soil". As a consequence, it was "doomed, in so far as it catered to foreign markets, to be fast exported to other lands..." (p. 177).

Perhaps the most important scholarly contribution Farnie makes, however, is the "select" bibliography. Running over fifty pages it is a research tool that no serious student of the English cotton textile industry can afford to ignore.

On a somewhat more negative note, it must unfortunately be recorded that these essays are more in the nature of interpretative summaries of already available information than examples of original data compilation and analysis. Indeed, on a few occasions the author has a tendency to throw economic analysis around rather too freely. Thus, for example, the discussion of changing terms of trade in the cotton industry and their economic implications suffers from a total lack of any information on changing factor productivities during the same period (pp. 14-15).

Similarly there is the (unoriginal) claim that the "impersonal discipline of the market" "forced" the businessman to pursue short-term gains that cost the industry dearly in the long run (p. 88). This is a type of "market failure" which I have never encountered in economic theory. An excessive emphasis on the short run is not the result of competition, "excessive" or otherwise. Such an outcome requires that most participants in the market either have "excessively" high discount rates or that they consistently misjudge future technical or economic developments. Even a relatively small minority with sounder preferences and expectations would have an opportunity for substantial profits. In any case, such "failures" usually are only detectable ex-post.

As a last example, let me quote the following rather peculiar statement: "Foreign demand expanded markedly after 1815 in reaction to the unbalanc-

ed extension of the industry's spinning power beyond the immediate absorptive capacity of the internal market..." (p. 94). Unless I am missing something, this assertion fails to maintain the standard separation of demand and supply on which all partial equilibrium market analysis depends.

I should hasten to add, however, that I had to look long and hard to find these problem passages. Indeed, the hunt itself may perhaps be seen as nothing more than my reviewer's need to find something to criticise, even in this excellent book.

LARS G. SANDBERG
Ohio State University

J.K. FEDOROWICZ, *England's Baltic Trade in the Early Seventeenth Century. A Study in Anglo-Polish Commercial Diplomacy*, Cambridge, University Press, 1980, pp. 334.

J.K. Fedorowicz's dissertation is an excellent contribution to the extensive current research into the Baltic trade at the beginning of the XVIIIth century.

His work is not confined to the activities of the Eastland Company; he also investigates the role played by commercial conditions in diplomatic relations between England and Poland at the end of the XVIth century and during the first half of the XVIIIth century.

The establishment of the Eastland Company's staple in Elbing in 1579 was remarkable in that it was connected with Stephen Bathory's conflict with Danzig and constituted a direct challenge to that city. Thus the English were released from the Hanseatic "guest law" in Elbing, and the company's main objective was to create a staple for English cloth on the Polish market.

Fedorowicz traces the Eastland Company's development and assesses its importance in English foreign trade, and he also deals with the part played by the interlopers. Here his research is based mainly on the special account in the Danish Sound Toll Tables of the passage of English ships through the Sound to and from the Vistula area. He finds that the English cloth met with competition from Dutch textiles and domestic production in Poland at the beginning of the XVIIIth century, and that the imports of Baltic products (naval stores) soon became of greater importance to the Eastland Company than the sale of English cloth in Poland. In gauging England's imports of Baltic products he is largely restricted to STT's registration of cargoes on board English ships, but he is aware of the fact that some goods of this nature were carried by Dutch vessels either directly to England or via Dutch entrepots, although at this time STT did not record the ships' destinations. As a result an important problem arises which Fedorowicz is unable to solve in this context. This relates to the importance of the Dutch transit trade: the statistical treatment of STT's English ships' cargoes does not give accurate details of the

actual imports into England of naval stores and other goods from the Baltic area, nor of England's true trade balance with Poland. Subject to this limitation, his research on this topic is carried out with great care, and he clearly demonstrates the growing importance of naval stores for England's navy and her merchant navy. This change in English trade meant that the English merchants gradually transferred a large part of their demand for naval stores from the Vistula area northwards to the Baltic region, as Fedorowicz clearly points out.

Naturally Danzig looked with disfavour on the establishment of the staple in Elbing, and as Fedorowicz shows, Danzig tried successfully in various ways to counteract the Eastland Company's privileged position in Elbing, thanks above all to the fact that the Polish king and the Sejm were responsive to the suggestions put forward by the powerful trading city. The death-blow came later when Gustavus Adolfus II had the Vistula by the throat in 1626, and as a result extorted the truce in Altmark and the settlement in Tiegenshof in 1629-30, after which the whole of the trade in the Vistula area was subject to severe Swedish taxation until the year 1635.

Fedorowicz's account traces in an interesting way the gradual decline of the English staple, due to the changing character of the trade and Danzig's vigorous counter-measures, and also to Sweden's drastic intervention in the Vistula area.

Ever since the foundation of the Eastland Company in 1579 the English government tried to promote the interests of the English merchants through diplomatic channels, and Fedorowicz devotes a large part of his work to an analysis of the course of diplomatic relations between England and Poland. This has been accomplished through the skilful use of a large number of English and Polish sources, and Fedorowicz demonstrates how the English government, in spite of its significant efforts to safeguard the interests of the English merchants by diplomatic means, was powerless in the face of the great respect which the Polish authorities naturally accorded to the views advanced by Danzig. However, when Danzig finally got her own way, English commercial circles largely transferred their interests first to Königsberg and then to the ports controlled by Sweden.

Fedorowicz also shows how commercial interests were subordinated to higher goals during the 1620s, since England was anxious to support the forces which were prepared to intervene against the Habsburgs on the Continent, above all Sweden. He neatly illustrates this with reference to Sir Thomas Roe's diplomatic missions.

Fedorowicz's work is well organized and written with skill, and it stimulates awareness of the need for further investigations in this field, especially with regard to the Netherlands.

ARTUR ATTMAN
University of Gothenburg

M.P. HANAGAN, *The Logic of Solidarity: Artisans and Industrial Workers in Three French Towns, 1871-1914*. Urbana: University of Illinois Press, 1980, pp. 261 + xv.

Michael Hanagan's *The Logic of Solidarity* is a major contribution to French, economic, and labour history. In order to explain the evolution of strike militancy in France between the Commune and the First World War, Michael P. Hanagan examines changes in work structure during that crucial period. His *The Logic of Solidarity: Artisans and Industrial Workers in Three French Towns, 1871-1914* takes on large and important questions through the case study. Hanagan's three towns, Rive-de-Gier, Saint-Chamond, and Le Chambon-Feuergrolles, all in the *department* of the Loire, developed as small industrial centres in the hinterland of St. Etienne, itself the fastest growing city in France in the first half of the nineteenth-century. Hanagan aptly describes this region as "the cradle of the industrial revolution" in France.

That Hanagan found and has cogently studied, analyzed and described the predominance of artisanal workers in the strike movements of his towns in a *department* that might seem to have most closely duplicated the English "model" of large-scale industrialization offers further evidence of the continued importance of artisanal production in France. "Because of the marked slowness of the French economy to industrialize," Hanagan writes, "the history of the French labor movement is *par excellence* the history of the strategies used by artisanal workers struggling with employers armed with machines and new methods of work organization" (p. 13). "Slow change" thus contributed to the militancy of French workers in the decades immediately preceding World War I by leaving artisans as a skilled, organized, proud, and relatively highly paid force, even within large units of production. They resisted changes in the structure of work and the attempts of their bosses to wrest control over the work process and the shop floor. Historians of modern Europe have, of course, stressed the role that artisans played in social movements — notably the French Revolution, and the Revolutions of 1830 and 1848 — during the previous century. Unskilled workers played very little part in those events. Hanagan is concerned with the circumstances that led skilled artisans to coalesce with industrial workers. By the last years of the nineteenth-century, the encouragement and support semi-skilled workers (a category sometimes neglected in the literature on French workers) received from organized artisans largely accounts for their emerging militancy. Artisans, confronted by mechanization, an increased division of labour, and the hostility of employers and government officials joined industrial workers in strike and political action.

Hanagan first offers an extremely effective summary of the evolution of the French economy in the nineteenth-century, "an artisanal economy under stress," and the difference between artisans and industrial workers. He then introduces the Loire region and its working-class followed by an overview

of the impact of economic change on strike militancy in the Loire. Then Hanagan offers portraits of his three towns to make his case: Rive-de-Gier, where skilled glassworkers came to offer solidarity, leadership, and support to metalworkers; Saint-Chamond, where a marked lack of militancy may be explained by the elimination of skilled artisans from local production, leaving an essentially unskilled and largely female proletariat that lacked both organizational resources and artisanal allies; and artisanal Le Chambon-Feugerolles, a centre of strike militancy, where fileworkers, who worked in small shops and were relatively well paid, and semi-skilled boltmakers became allies. In 1910 fileworkers, who resisted a loss of control over the work process including their previous right to determine work methods, strongly supported the boltworkers' strike, while the town's industrial metalworkers remained quiescent.

The Logic of Solidarity is a stimulating, thoughtful, and important book. Hanagan understands the issues and debates in labour history, and has mastered the secondary literature. He has been influenced by the recent work of Charles Tilly, Ives Lequin, Rolande Trespé, Joan W. Scott, and others. The text and footnotes are alive with comparisons and ideas. The book is informed by theory as well as based on impeccable scholarly research drawn from the full and rich gamut of national, departmental and municipal archives. He handles both his qualitative and quantitative sources with equal dexterity, and the 32 tables and 11 graphs are extremely helpful, without overwhelming the reader. Clearly and smoothly written, Hanagan's study, unlike many local studies, never loses sight of the larger issues. I would fault *The Logic of Solidarity* only for a certain degree of needless repetition because of the author's eagerness to convince his readers of the essential argument; and for leaving rather vague and understated the political evolution of his towns — as where he tantalizes us with the assertion that syndicalists and socialists worked closely together in Le Chambon-Feugerolles without going into much detail.

Hanagan's investigation and description of how the uses of both urban and work space — residential patterns and the physical lay-out of shops and factories — facilitated or inhibited interaction and solidarity between workers is particularly well-done as he compares artisans and industrial workers. Just for this the book is *vaut le voyage*. The contrast between the fileworkers, boltmakers, and metalworkers is particularly compelling. Contemporary descriptions of *boulot*, photographs, and a drawing of a floor plan of a metal factory evoke the world of French work at the turn of the century. *The Logic of Solidarity* thus demonstrates that the concentration of artisans in certain quarters, even in these relatively small "road towns," promoted solidarity, whereas industrial workers tended to be scattered through the towns and even the countryside; and that concomitant patterns of leisure activity also aided in artisanal cohesiveness (although his documentation on café sociability is a little thin). Such solidarity and organization, formal and informal, formed the base of artisanal strike militancy in France. When artisans faced the on-

slaught of forced alterations in the organization of their work, as in Rive-de-Gier and Le Chambon-Feugerolles, they formed alliances with and lent support to less organized industrial workers.

JOHN MERRIMAN
Yale University

L.C. HUNTER, *A History of Industrial Power in the United States, 1780-1930*, Vol. 1: *Waterpower in the century of the steam engine* (Charlottesville: University Press of Virginia, 1979. Pp. xxiv, 606).

Waterpower is the first volume of Louis Hunter's planned three volume series on the *History of Industrial Power in the United States, 1780-1930*. Recognizing that much more attention has been paid to the mobile power used in transportation than to the stationary power used in mills, mines, and factories, Hunter has set out to explain the historical development and importance of power "generated in power plants within or adjacent to the industrial establishment in which it is used." This volume on the harnessing of energy from falling water and the mechanical transmission of power for manufacturing processes is an impressive beginning.

Hunter's examination of the American experience with waterpower is informed by comparative study of European practices. He tells us that "until the latter half of the nineteenth century mechanical power in the Western world was preeminently waterpower." Water as a source of energy was particularly important in America where potential mill sites were abundant and labour was scarce. Colonists depended heavily on watermills which were, in Hunter's opinion, little more than an extension of Old World technology. The flow of technological information from England and the Continent continued to influence the development of American waterpower after the formation of the new nation, but Americans soon made major advances of their own in hydraulic engineering. Hunter pays particular attention to American success in managing large waterpower systems in the nineteenth century and in improving that great French innovation, the hydraulic turbine.

Hunter attacks the popular belief that the stationary steam engine was primarily responsible for the Industrial Revolution in the United States. This belief is derived from the British experience but has been accepted without qualification by most historians. Britain needed steam power for large-scale manufacturing because almost all its waterpowers were small; the situation was far different in America. Hunter concludes that "Falling water was the chief source of stationary power at all levels, in most branches of industry, and throughout the greater part of the United States before the 1860's."

The broad, systems approach which Hunter applies to his subject is essential for an adequate coverage of waterpower. Patterns of rainfall and topography affect the flow of water in drainage basins and determine the maxi-

imum scale and best locations for water-powered manufacturing. A mill often required a dam and raceway in addition to its wheel and power transmission system. Building water control structures sometimes aroused the ire of farmers, or other mill owners and could lead to litigation and governmental intervention. Large industrial communities such as Lowell and Holyoke had elaborate networks of canals which affected urban planning and which were managed by prominent men with considerable knowledge of both engineering and business. Economic factors influenced technological choices, including decisions on types of power and forms of machinery. America's economic growth as an industrial nation was greatly stimulated by the power drawn from its rivers and streams.

In any attempt to cover so complex and wide-ranging a subject, even an exceptional historian is bound to show some areas of weakness. Hunter lacks a comprehensive technical understanding of waterwheels and turbines. His knowledge of civil engineering is limited, which may explain why he gives so little space to the form and construction of dams. Perhaps his greatest weakness as a scholar of waterpower is that he neglects the material evidence of our industrial heritage and is almost totally dependent on documentary sources and historic illustrations. The number of published works and manuscripts he has consulted in the preparation of this volume is staggering, but there is much to be learned in the physical examination of dams, races, mills, wheels, turbines, and shafting. Industrial archaeology is a rapidly growing field in Europe and in America; it is too bad that Hunter has not made more use of the evidence that can be gathered from artifacts.

Documentary and artifactual research by industrial archaeologists challenges a number of Hunter's conclusions about the dates of introduction and the diffusion of certain devices. The close-fitting "breast" and the fly-ball governor were introduced earlier and were much more common in water-powered mills than Hunter realizes. He ignores major technical developments with his statement that "For the waterwheels themselves the transition from the age of wood came not with the extension of the use of iron in wheel construction but in the gradual replacement from the 1820's of the old-style wheels by [iron] reaction wheels from the 1840's by the new turbine wheels." In the second quarter of the nineteenth century wheel shafts, hubs, and perimeter gear segments of cast iron were used frequently by millwrights, and wrought iron provided tensile strength in the ties and bolts which held many wheels together. Composite wood and iron wheels could be found in most textile mills by the 1840's and in a large number of smaller watermills as well. The Boston Manufacturing Company in Waltham and the Prescott Company in Lowell installed large breast wheels made entirely of iron before the widespread adoption of more efficient turbines. The Fitz Water Wheel Company in Pennsylvania was still making metal waterwheels in the twentieth century.

Waterpower, despite its flaws, is a very important book, one of the best

studies published to date on the history of technology. As a major work, it will be relied on by generations of historians and will probably be considered the grand synthesis of all preceding scholarship on American waterpower. Unfortunately, Louis Hunter has failed to cite the work, published and unpublished, of at least several historians who contributed to the success of his synthesis. In the acknowledgements, he thanks only eight historians and a selective list of the scholarly institutions which helped him. Pleading that there are too many deserving individuals to list is perhaps excusable, but omitting citations that would have been helpful to present and future students of industrial history is a disservice to the historical profession. In an extensively documented work of this nature the author should cite *every* source that clarified his understanding of waterpower or provided information which he used.

PATRICK M. MALONE
Slater Mill Historic Site and
Brown University

E. KAISER: *Economic Diplomacy and the Origins of the Second World War. Germany, Britain, France, and Eastern Europe 1934-1939*. Princeton, New Jersey, Princeton University Press, 1980, pp. 346.

Reading David Kaiser's book brings to mind a well-known aphorism in reverse: "politics (including economic policy or economic diplomacy) is warfare carried out by different weapons." Exciting new archival discoveries, and the clear structure and style of the book firmly establish that Britain and France lost the first crucially important round in World War II against Germany between 1931 and 1938. The battlefield was East-Central Europe.

In his first major publication David Kaiser has proved himself to be a mature and experienced scholar. He argues that the architects of German foreign policy, in spite of the critical economic situation of the country (or indeed because of it?), recognised that there was a great possibility: Germany could exploit the extremely deep agricultural crisis in East-Central Europe to further her own political ambitions. This policy lay behind the trade agreements of 1930 and 1931 and the proposal for a customs union with Austria. (pp. 23-26.) Direct economic interests, however, hindered for a while the realisation of political aims (p. 56). Radical changes began after Hitler's rise to power. Posse's new economic strategy and the German-Hungarian trade agreement of February 1934 then laid down the pattern for further agreements. (pp. 74-75.)

One can distinguish two separate stages in Germany's economic penetration of the region. The first (between 1934 and 1936) was characterised by the policy of Hajalmar Schacht. (The reader is given an impressive survey

of his economic ideas). (p. 133). The German economy, however, survived a particularly severe crisis in 1936-37. Food and raw material supplies became critical. Schacht would have responded by slowing down the rearmament drive, but now the answer was provided by the emergence of extremist Nazi policy: "guns instead of butter". Goering's well-known slogan was not an aphorism at all but a matter of fact explanation of the decision which had been made Germany had resolved to cope with her economic troubles by the 4 year plan of forced rearmament.

The expansion of the German economy started under the guidance of Goering from 1937 (pp. 151-153). The export opportunities of agricultural Eastern Europe changed in these years because of improving world market possibilities, and Germany began to dictate and blackmail. In the early thirties she had opened her markets to Eastern European food and raw materials, now she offered armaments in return for food and primary products. The temptation was great for countries which were preparing for or worried about territorial revisions. As a result, Germany had gained a dominant economic position in the region by 1938. She even became the main trading partner of Czechoslovakia and her percentage share in foreign trade was twice as much as the combined British and French share. The dominant economic position of Hitler's Germany was clearly shown by her 40-50 per cent share in the foreign trade of some of the countries of Eastern Europe. From this position, German economic power was already able to affect the industrialisation policy of these countries by 1939. (pp. 263-268). In the meantime, German arms deliveries slowed down or stopped because of the marked increase in German war preparation. But peaceful economic expansion was then replaced by military expansion. When a new economic crisis emerged in Germany in 1938-29, it was solved by a new policy toward the Soviet Union rather than by occupying other European territories. But if planned military expansion had previously served to prepare forced rearmament, now the situation changed and successful territorial expansion assisted further rearmament. (pp. 281-283). All in all, as the author emphasises, Germany *subordinated* her economic interests to her political aims in a masterly way after 1931, and especially after 1934. The possibility of doing so was offered by the similarity of the economic, financial and currency difficulties of the countries involved. (pp. 316-317).

It should be added that this last factor deserves greater emphasis. A detailed analysis of German-Hungarian economic relations in particular, shows in general terms that common economic difficulties and weaknesses offered great possibilities to Germany and that she exploited these so well that she had no need to *subordinate* her economic interest to her political ambitions, but by pursuing the latter could from the very beginning also gain major economic advantages from her so called "gestures". Even the opening of her internal market and the assurance of prices higher than those in world markets meant that she could mobilize the credits which had been frozen after 1931 in Eastern Eu-

rope, and could even obtain large quantities of hidden loans from the crisis stricken countries of Eastern Europe. Germany did not subordinate her economic interest, but skilfully *combined* them with her political aims.

One of the most interesting and original discoveries of the author lies in his reconstruction and analysis of the controversial French and British policies. The pattern here was quite the opposite from that which prevailed in the German case. Britain and France could *not* combine their economic and political interests. They realised the priority of their economic interests, and strengthened their economic ties with their colonies and inside their empires. This decision proved to be a crucial mistake, and according to the author it also contributed to their losing their empires in the post-war period. It was a similarly decisive mistake to have defended the status-quo and to have been unable to react flexibly (pp. 316-318).

The French, however, did from time to time try to strengthen their political influence by economic means. One of the most important efforts was the Tardieu plan (pp. 44-45.) There were also Massigli's efforts to introduce certain financial measures and to back the Hodža-plan (p. 205). Even after the Munich agreement, French policy tried to achieve a certain "balance", and this was illustrated by the agreements with Romania, Jugoslavia and Bulgaria (p. 284). But none of these could survive. There were crucial difference between French and British policy toward Eastern Europe from the early 1930's (p. 100). Moreover, French foreign policy was ambiguous. Mussolini's proposal for a new Four-Power agreement on territorial revisions following ethnic borders to meet Hungarian demands was backed by Britain but neglected by France (pp. 105-106). On the other hand, France's new Eastern policy met with a cold British reception (p. 117). France, because of her impotence and indecision, lost a great part of her prestige in the early thirties, and even Polish foreign policy led by Colonel Beck moved away from the traditional French orientation.

In Britain an unduly narrow interpretation of economic interests led to quite deliberate counter-measures which almost prohibited any form of co-operation with East-Central Europe, and caused bitter dissatisfaction in the Baltic states. (pp. 81-93). There were, of course, some able and farsighted men in the British foreign service, who had long-term ideas as far as Eastern Europe was concerned (for example Collier, who remained alone in his proposals p. 99). British foreign policy, however, was marked by a lack of interest in the region. The main architects of foreign policy regarded Eastern Europe as a region suitable to be sacrificed to achieve appeasement with Hitler (p. 170). Carr even opposed a declaration against the Anschluss (p. 171). Both Conservative Non-Interventionist economic policy and Chamberlain's appeasement policy together played a tragic role in helping Nazi penetration in Eastern Europe. That was judged to be unavoidable and moreover, probably advantageous for Britain by the Foreign Office. Leith-Ross even offered

a strange proposal for German-British collaboration in October 1938. In March 1939 it was already too late! (pp. 287-288).

The exciting new documentary evidence and the soundly argued analysis reveal a very interesting interrelationship between diplomacy and economy. The book is as interesting to read as a good novel, although the introductory chapter seems to be too short. But I should like to welcome the book both as a new and valuable work on interwar economic and diplomatic history and as a pioneering work as well. The historical lesson is that the role of economic diplomacy is still very important, even remembering Hegel's bitter remark that: "The lesson of history is that peoples and governments never learned from it". Let us see!

IVAN T. BEREND
University of Budapest

F.C. LUEBKE (ed.), *Ethnicity on the Great Plains*, Lincoln and London, University of Nebraska Press, 1980.

The message of this selected set of papers from the second annual symposium of the Center for Great Plains Studies is that ethnicity has left an enduring mark on rural as well as urban society. Minority groups — Native Americans, European immigrants, and Hispanic-Americans — who along with their descendants constitute a majority element in many parts of the Great Plains, have maintained their ethnic identity long after professional scholars concluded that they were assimilated.

Plains historiography in the twentieth century, following the lead of Walter Prescott Webb's influential book, *The Great Plains* (1931), has stressed environmental rather than cultural factors. The dictates of the harsh landscape, it was believed, required that immigrants adapt or fail. This book, however, and the symposium that gave it birth, testify that the conceptual pendulum has swung from the environmentalists to the culturalists, for whom ethno-cultural forces are predominant, even in such a physiographically unique and demanding region as the Great Plains.

Since American scholarship has long emphasized geographic and economic factors, this book provides a needed corrective by focusing on cultural and religious influences and institutions. Economic historians of the Middle West can no longer ignore ethnic factors in migration patterns, agricultural cropping and animal husbandry, and community formation. Some indication of the dramatic interpretative reorientation is evident in the origins of this work. When the Great Plains Center at the University of Nebraska-Lincoln announced the topic of ethnicity as the theme for the 1979 symposium, more than fifty scholars from many disciplines answered the call for papers. Because of this "unexpected richness of research" (ix), symposium director and editor Frederick Luebke enlarged the conference to accommodate thirty-three papers.

Hundreds of other scholars from the United States and Western Europe attended.

The twelve essays that comprise this volume represent nearly a third of the conference papers. Luebke selected them for their comparative treatment of ethnic patterns among major groups, and for their diverse disciplinary perspective. Historians contributed five papers, geographers and anthropologists three each, and one each came from sociologists and a folklorist. Economic historians and political scientists failed to respond to the call for papers, according to Luebke. This is quite unexpected, because the ethnocultural interpretation of American politics has become the new orthodoxy among political historians, and economic historians have documented the role of ethnicity in midwestern agriculture and in adjustments to industrial America by European immigrants.

The premier examples of the new research that are included here are surprisingly cohesive, thanks to Luebke's firm editorial hand and his integrative opening essay, which summarizes the individual efforts and links them to the larger issue of cultural pluralism and its determinants. Kathleen Neils Conzen's lead essay, "Historical Approaches to the Study of Rural Ethnic Communities," also serves an essential function by summarizing the meager existing literature and cogently setting forth the theoretical basis for studying types of ethnic adaptation processes within rural society. Conzen's work is deservedly the "pilot ship" of this armada.

The second section includes two pieces on Indian settlement and behaviour patterns. Averell Morgan Gibson, a specialist in Indian history, provides a thorough, if overly emotional, synthesis of the Great Plains as a colonization zone for Eastern Indians. With rare insight, Gibson compares Indian and European settlement patterns on the Plains and concludes that both groups of immigrants were ethnically heterogeneous, they both fled oppressive conditions, carried western civilization into the Plains, and viewed sceptically the intense promotional rhetoric that their destination was the "garden of the world." Hopefully, Gibson will develop this intriguing insight in future writings. Raymond De Mallic's insightful analysis of the contrasting cultural symbols and rituals involved in Indian-White diplomatic councils — smoking the pipe, "touching the pen," and gift-giving — will be of greater interest to ethnologists and Indian historians than to ethnic scholars. The rationale for including this paper in the book is not readily apparent.

The succeeding six essays examine ethnoreligious aspects of plains settlement among Volga Germans (Timothy Kloberdanz), Swedes on the Dakota frontier (Robert Ostergren), Old Order Amish (John Hostetler), Hill Country Germans of Texas (Terry Jordan), Hungarians in Saskatchewan (Linda Dégh), and Czech Freethinkers (Bruce Garner). Kloberdanz describes the very successful adaption of the German immigrants from the Russian steppes in the Great Plains and the Argentine pampas, whereas Hostetler's conservative Amish

refused to adapt for religious reasons and their prairie settlements largely failed. Ostergren's essay is a micro-study of the migration routes and timing of 206 Swedish settlers in Dalesburg, Dakota Territory. This innovative work, based on a unique local biographical directory, further demonstrates Ostergren's mastery of the social geography of midwestern Swedish immigration. Jordan, Dégh and Garver all stress the salience of religious culture for preserving ethnicity. Jordan, a specialist on German agriculture in Texas, here examines church affiliation, architecture, and rural practices, together with residential segregation, among eight German religious groups, where he finds a "remarkable" survival of religious ethnicity in a hostile environment (127). Dégh's Hungarians in Canada likewise preserved their ancestral heritage through the church, although the Highlander-Lowlander cultural clash led to enduring divisions within the church and community that affected assimilation rates. Czech freethinkers, by contrast, maintained their language and culture through distinctive fraternal organization, which functioned as surrogate churches until after World War I.

Two essays on economic facets of ethnicity follow the religious case studies. Bradley Baltensperger compares farming practices among four distinct groups of Nebraska immigrants, based on agricultural census manuscripts in the 1880s and 1890s. As earlier scholars have found, he observed a general convergence but with some degree of cultural rebound and retention, especially by the Germans and Russian Germans. Joseph Barton, an expert in eastern European immigration and a literary stylist of some repute, contrasts Czech and Mexican immigrants in southwestern Texas from 1880 to 1930, based on World War I draft records. His lucid and conceptually stimulating essay concludes that Czechs and Mexicans *chose* to be Czech farmers and Mexican labourers rather than merely farmers or labourers because of ethnic considerations.

A number of themes recur throughout the book. Ethnic group settlement patterns differed widely across the Plains, but every locale had its examples of neighbourhood ethnic clusters. The northern states and Canadian plains had the highest concentration of ethnics and the southern states the lowest levels. Secondly, while all immigrants altered their cultural heritage in response to the Plains environment, ethnicity survives in varying degrees among every group. Third, religion is the dominant force in ethnic communities and the institution that perpetrates ethnic identity for generations. Fourth, assimilation has been most extensive in agriculture practices and least evident in social and religious life. Finally, in terms of the debate between environmentalists and culturalists, which had long favoured the former, the contributors to this revisionist volume overwhelmingly conclude that kinship, religion, and cultural heritage far surpass environmental forces in determining ethnic patterns. Cultural maintenance rather than assimilation is the common denominator. As Conzen asserts, scholars now have the research task of building

explanatory models of the origin and persistence of rural ethnicity, as they have long attempted in urban studies. Any who take up the challenge will find this book to be an indispensable starting point.

ROBERT P. SWIERENGA
Kent State University

S. REINHARD, assisted by M. TYBUS, *Wachstumstrends und Konjunkturzyklen in der deutschen Wirtschaft von 1820 bis 1913. Quantitativer Rahmen für eine Konjunkturgeschichte des 19. Jahrhunderts.* With 58 figures and 30 tables. Göttingen (Vandenhoeck und Ruprecht-Verlag) 1978, 215 pp.

Spree's study is an attempt to analyse systematically the "growth cycles" in the German economy between 1820 and 1913, by means of the unitary criteria of 18 business cycle indicators on the basis of long time series. On the one hand, it is a summary of the findings of Spree's doctoral dissertation (*Die Wachstumszyklen der deutschen Wirtschaft von 1840-1880*. Berlin: Duncker & Humblot 1977). On the other hand, it goes beyond that book with regard to issues and periods discussed.

The authors combine the instruments of the "classical" time series analysis (fitting polynomial trends of various degrees to time series; decomposition of time series; calculation of diffusion indices; dating of turning points of cycles; lag-correlation of residuals to determine lead-lag structures) with instruments of the more recent stochastic time series analysis (correlogram analysis and spectrum analysis) principally in order

- to work out long-term growth patterns of general economic significance in different sectors (trend analysis),
- to date the principal business cycles within the economy as a whole,
- to analyse the number and average length of the cycles of single economic variables,
- to examine the economic interrelations between different variables or sectors, while taking account of delayed reactions,
- to draw conclusions about the economic effects of change in the socio-economic structure.

These issues indicate a shift to business-cycle analysis within German economic history. In the 1960's there still existed a wide-spread position of economists and historians, which meant that the classical business cycles had become obsolete. The come-back of business cycles in the 1970's — a surprising experience after two decades of "growthmanship" — may have fostered some kind of renaissance of the history of business cycle in Germany, where this issue traditionally was carried on by scholars like Hans Rosenberg, Josef Schumpeter, Arthur Spiethoff, Eugen Varga, Ernst Wagemann and others.

It is Spree's intention to refer to this tradition in providing a quantitative framework for a history of business cycles and long-term growth in the nineteenth century.

Central to the concept of the business cycle is the supposition that there are structural groupings within economic life, which condition a certain periodicity of growth cycles. The existence of such cycles of approximately constant length and unchanging amplitude is not, however, evident in the 18 indicators examined, neither in the original values, nor in the deviations from the trend. With the instruments of classical time series analysis, it is possible to adduce only a growing steadiness of the cyclical picture since the late 1870's. With the help of spectral analysis, the authors, however, are able to demonstrate that 13 of the 18 indicators present typical Juglar cycles lasting between 7.2 and 9.1 years. Only average population growth, coal demand and bankruptcy are not characterized by Juglar cycles. In 11 cases Kuznets cycles lasting between 16 and 25 years simultaneously overlay these cycles of moderate length, so that the course of each indicator seems to be the result of boost and brake effects from different cyclical patterns emerging simultaneously. Kitchin cycles, finally are present in 8 of the indicators analysed. It remains, however, uncertain how far the types of fluctuation revealed by the analysis only of spectra should be attributed to the boost effect of the filter which was used.

On the other hand, the results of Spree's study can neither be seen as a confirmation of Kondratieff's "long-wave" hypothesis of economic growth nor be used to prove the hypothesis, frequently upheld since Spiethoff (1923), of 4 so-called "Wechsellagen" (long cycles) in the course of the period from 1822 to 1913. Whereas the "Wechsellagen" are to be differentiated by the predominance in them of years either of stagnation and depression or of expansion, according to Spree's study the upswing years predominate in all long cycles (1822-42, 1843-73, 1874-94, 1895-1913) examined. Concerning Kondratieff's "long-waves" there is no trace of trend fluctuations in precisely those economic phenomena and their indicators which should provide basic support for the present elements of a theory of Kondratieff cycles.

Another aim of the study is to demonstrate the interrelations between variables and sectors important for business cycles and growth: Do business cycles influence population growth? Are there connections between agricultural cycles and those of the overall economy? Do factors of the cyclical development determine the consumer goods industries? Is there mutual relationship between monetary cycles and cycles of heavy industry, and so on? Broadly speaking the results of Spree's and Tybus's calculations say that the assumed connection were evident only in very few cases during the whole period under examination. The number of indicators or the method applied prove to be insufficient to allow us to test central hypotheses concerning the history of industrialization. Other problems, like socio-economic pre-conditions or ancil-

lary phenomena or effects of the growth processes examined or the various aspects of their institutionalization were not taken into consideration deliberately, because the study is strictly limited to a quantitative analysis.

Spree, however, pays great attention to choosing optimal trends for the indicators by alternative fitting of exponentials and logisistics and also of polynominals from the first to the tenth degree. He believes that polynominals prove to be more suitable throughout and that they make it possible to take into account breaks in structure of the long term growth patterns during the period 1820 to 1913. It is sure that this method does not have very high analytical value. The trend polynominals applied give nothing but an optical help interpreting long-term growth patterns. Nevertheless, Michael Tybus, who applies a more sophisticated apparatus of statistical methods to the same time series, comes to very similar results.

In general, the authors fulfil their aims of providing a framework of figures and of criteria which provide a basis upon which more exact quantitative and qualitative detailed studies can be made.

WERNER ABELSHAUSER
Ruhr - Universität, Bochum