
REVIEWS OF BOOKS

PH. S. BENJAMIN, *The Philadelphia Quakers in the Industrial Age 1865-1920*, Philadelphia: Temple University Press, 1976, ix, 301.

Those who found cities are fated to live in them and be transformed by them. The members of the Society of Friends who came to the rich American valley of the Delaware in the seventeenth century come with godly intentions and a simple, benign life style. The growth of America thrust them into crosscurrents of change and took from them the assurance thrust them into crosscurrents of change, took from them the assurance of power in the city they founded and surrounded them with a hectic complexity that left them in a social enclave of increasing isolation-nostalgic, uncertain and defensive.

Philip Benjamin's study of Quakers living amid the smoky din of nineteenth century industrialism is a welcome illumination of the social history of a religious group overtaken by the furies and strains of modernization. That peaceful lifestyle of Quakerism produced in the time of William Penn by radical religious simplification and a characteristic English yearning for domestic tranquillity was assaulted by all of the turbulence of American experience. The mercantile propriety of Quaker businessmen was overborne by the vast schemes and aggressive vitality of industrial enterprise. The modest order of family, meeting house and customary association was assailed by the pressures, temptations and surging problems of other cultural groups as the heterogeneous city grew.

Using the interpretation of American life of Robert Wiebe that sees society as a collection of cultural islands confronting and changing one another, Benjamin details the challenges to the simple creed and social codes of Philadelphia's Friends. Demographically the Quakers declined in the city, sustaining themselves with immigration from rural Quaker meetings. The author's use of the records of the meeting houses to show this change, and to show the occupational, economic and educational status of members of the Society of Friends is admirably executed. His exploration of the rich archives of Quaker life is most impressive throughout. The chief influences on the Quaker community, however, were not the internal dynamics of its own population, but the steady torrents of innovation, expansion, immigration and cultural competition of the burgeoning city.

The Quaker meetings in seeking to perpetuate a distinctive tradition and ideology were frequently reduced to patient impotence and ambivalence by the erratic tides of urban development. Familiar effects of social frustration and ambiguity resulted. Factionalism, petty disputes, schism and eroding confidence took their toll. Higher education, at first opposed as worldly and threatening, finally began to point the way toward a new definition of responsibilities and a new interpretation of the religious tradition as the twentieth century dawned. Haverford College, Swarthmore College and Bryn Mawr College all played a role.

The new synthesis of Quakerism coincided with the reforming spirit of Progressivism. Benjamin presents very interesting material on Quaker attitudes in the nineteenth century toward race, women and immigrant problems. His conclusions are unedifying. The reform impulses of Progressivism, however, suited the middle class, educated, urban posture of Quaker leadership. Like much of the Progressive effort for housing and political reform the Quaker contribution remained one of benevolence toward an immigrant and working class that was alien and culturally distant. The author says that the Quakers of this reform period themselves remained in a secure position economically and socially. A fundamental conservatism contained their endeavors within traditional philanthropic molds preventing any broad indictment of American institutions.

This book is a keenly instructive analysis of a religious culture amid tides of urban change. Its criticisms of Quaker life are sharply drawn, though specialists will question the basis of some of the critique. The study is a tribute to the talented research and rigorous objectivity of Philip Benjamin, and a tribute to the persistence of religious values in tension with the disorderly change and brutal exactions of modern city life.

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- L. R. BERLANSTEIN, *The Barristers of Toulouse in the Eighteenth Century (1740-1793)*, Baltimore, The Johns Hopkins University Press, 1975, pp. XII-210.
- J. R. CENSER, *Prelude to Power. The Parisian Radical Press, 1789-1791*, Baltimore, The Johns Hopkins University Press, 1976, pp. XIV - 186.

Despite the immense volume of published work on the era of the French Revolution, the literature is constantly refreshed by new conceptions, the identification of new evidence and the application of new methods. The two books reviewed here exemplify the persistent fruitful American interest in fresh approaches to the social and political history of France.

Berlanstein's short but thorough work on the *avocats* (translated as barristers) of Toulouse in the XVIIIth century has an interest that transcends the history of a particular profession in a particular town. The Order of Avocats occupied the heights of the legal system of the old regime just below the *parlementaires* — the magistrates of the sovereign courts. Most of the Toulousan *avocats* were attached to the Parlement of Toulouse and even more than their fellow-citizens of that "legal city" passed their lives under the influence of the prestigious court.

The legal functions of the *avocat* were not those of the journeyman lawyer, but of the expert in disputed points at law. Successful performance of these functions was rewarded with prestige, social authority and considerable wealth. It was possible for a brilliant *avocat* of obscure (if respectable) origins to rise to the top of the profession through talent alone. However, only a small minority was able to advance along this narrow avenue or even to make any mark at all in their chosen profession. Indeed, a considerable proportion of those of who had been received into the Order of Avocats never practised law. The attractions of a profession in which a considerable proportion of the practitioners did not expect to make a living provides evidence for Berlanstein's emphasis on the profession of *avocat* as a social status rather than a function.

According to Berlanstein the place of the *avocats* in Mousnier's "society of orders" was of greater historical significance than was their membership in legally defined estate or their place in an economically based social class. Their corporate attitudes, values, patterns of consumption, marriages, and even physical residence tell us more about them and are more predictive of their political behaviour, than are the technical demands of their profession, their success in attaining ennoblement, or their relative wealth.

They lived in a manner appropriate to a small stratum of the urban elite: cultured, unostentatious, solidly based in property holdings, prudent in investments, conservative in their political instincts. Perhaps not completely conservative — Berlanstein shows that the *avocats* of Toulouse re-

sponded to the turbulent currents of the pre-revolutionary period and the first years of the revolution, with an openness to reform and the criticism of the established order. They remained loyal, however, to the legal and social aristocracy of the Parlement whom they confused with defenders of national liberties as well as corporate interests.

Like many other social groups, the Order of Avocats split along political lines during the revolution. Berlanstein shows that these divisions occurred independent of relative wealth, success in the profession, or even of intensity of pre-revolutionary reformism and concludes that such evidence supports the interpretation that the antecedents of the revolution were political rather than social. However, his own research demonstrates that the avocats as a group occupied a far more conservative position on the revolutionary spectrum than did those lawyers (such as the procurers) lower down on the occupational ladder. Censer's book on the radical press reveals that the *avocats* were from the beginning a suspect group to the revolutionary journalists. In fact, the eventual movement of the *avocats* into the ranks of the counter-revolution is a not surprising consequence of their social situation before the revolution.

Prelude to Power is a systematic examination of the Parisian radical press, 1789-1791, and a contribution to the understanding of the attitudes of the revolutionary Left in the first two years of the revolution. Censer selected the newspapers according to his working definition of radicalism, which entailed support of those political activists associated with the Cordelier District, and the Cordelier Club, notably in their profound commitment to popular sovereignty. Out of the multitude of ephemeral journals spawned by the revolution, Censer selected six, ranging from the moderate *Mercure National* to Marat's firebreathing *Ami du Peuple*, that met his criteria of "radical" and survived for at least one year in the period May 1789 to October 1791.

After a brief useful survey of the background of the journalists and of the complex and precarious life histories of the journals Censer proceeds to a thorough examination of the shared ideology of these fiercely independent organs of radical opinion. The chapter, entitled "The Ideology of Popular Sovereignty", identifies the fundamental commitment of the radical press but suggests a certain circularity since Censer has already defined radical with reference to the commitment to popular sovereignty. This commitment was expressed in the celebration of mass political participation, in a paranoid suspicion of established, pre and post-revolutionary, institutions, and in the polarization of the French population into two tendentious categories, the *peuple* and the *aristocratie*. The *peuple*, however vaguely defined, were uniformly virtuous, pro-revolutionary, and guilty only of excessive tolerance for their implacable enemies. The *aristocratie*, somewhat more precisely defined with reference to the pre-revolutionary elites, were

uniformly committed to a conspiracy to reverse the gains of the revolution and reimpose the old regime.

These ideological premises determined the journalistic practice of all of the radical newspapers and determined their treatment of the great figures of the revolution, heroes or villains as their behaviour exposed their real relation to the sovereignty of the people. Such figures as the king or Lafayette, were highly praised in the early days of the revolution and then more and more suspect until they were endlessly pilloried for real and imaginary political sins.

Censer contributes a quantitative dimension with an analysis of the coverage of the various events that attracted the attention of the newspapers. He does this not merely to provide evidence of their political line, but to support an interpretation of the relation between political attitudes and revolutionary events: the radicals "did not shift their opinions of men and institutions because of a sequence of the key events of 1791", but rather in response to "relatively obscure occurrences in 1790". This was less a response to particular political actions than a consequence of attitudes they brought with into the revolution. Above all, the resentments and frustrations of their "grub street" antecedents predisposed them to distrust all constituted bodies.

This interpretation, derived from the absence of correlation between major revolutionary events and changes in the journals' political line seems rather arbitrary to me. Revolutions usually polarize. Allies on the eve become bitter enemies. The struggle to redefine the locus of authority radicalized some moderates and forced others into the ranks of the counter-revolution. The radical journalists did accept the king and the monarchy at the beginning of the revolution. The king's behaviour did have something to do with their subsequent shift in sympathies. If so, events whether salient or cumulative, do account for something, and not every commitment can be explained in terms of personal antecedents.

Apparently Censer did sample the centrist press, which he contrasts with the radical journals, but he did not undertake the sort of systematic investigation of the antecedents and the journalism of such "centrist" editors as Brissot that he applied to his selection of radicals. This somewhat weakens the force of his comparisons.

Both Berlanstein and Censer have supported their analyses with the statistical manipulation of rather small quantities of data that do not always convince this reader of the validity of their more ambitious generalizations. However, their meticulous examination of limited and clearly defined topics provides fresh perspectives on the broader issues of revolutionary historiography.

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- C. CARRIÈRE, M. COURDURIÉ, M. GUTSATZ, S. SQUARZONI, *Banque et capitalisme commercial. La lettre de change au XVIII^e siècle*, (Institut Historique de Provence, 1976), in 8°, pp. 222 (with a preface by J. Rueff).

The Roux papers in the Archive of the Marseilles Chamber of Commerce are now becoming more widely known, thanks particularly to the thesis by C. Carrière *Négociants Marseillais au XVIII^e siècle. Contribution à l'étude des économies maritimes* (Marseille, 1973). The collection forms part of the papers of one of the largest commercial houses in the city, and it provides an immense range of material and choice for the economic historian. The collection has now been drawn on again — and not by chance, since it contains some 10,000 bills of exchange — to produce a highly technical and intense study on a topic whose importance, if it is not immediately recognized, will rapidly be so on reading this study. Other sources have been consulted as well, including the account books of some large companies (Bruny), of some more modest, and of some very small companies (such as the grain merchant Barthélémy Boyer), especially for the first study in the volume which is entitled "Discounting". This involves a reappraisal of Raymond de Roover's now classic claim that there were no discount operations in France before the late XVIIIth century, which he believed to be due to the opposition shown by a Catholic environment to all forms of credit, in contrast with what had been occurring in England since the late XVIIth century. Charles Carrière first discusses the instructions found in merchants' manuals, and provides ample evidence to prove that bank discounting was fully established in French business practice even before 1700, and also that it was not reserved to a small elite but formed part of the day-to-day business of all types of commercial operators. In doing so he also does justice to the hitherto grossly overstated confessional explanations in passing, without remarking, as we would suggest, that commerce is itself a religion and — who knows — perhaps the most ecumenical...

In the second study, on "Circulation", Marcel Courdurié has grouped together the letters of exchange from the Roux collection for two sample years: 1729 and 1789. He analyses their features at the time of emission, their cause and duration, and he provides some conclusions on what one might call the different viscosities of the locations of payment—slow in Lyons and quick in Paris. He then turns to the enthralling process of toboggan effects which were created as the bills slid from town to town, sometimes acquiring on their way as many as six endorsements. The process is well illustrated by the diagrams (see especially the striking map of the route followed by bills drawn on Marseille in Palermo, which

returned via Naples, Trieste, Barcelona and Paris, or sometimes St. Quentin, a town of some importance in the cloth trade). Reflecting back on a discussion during the 1975 Prato Study Week, I should also add that on every occasion they discharged the inscribed sum and showed themselves to be outstanding instruments of monetary circulation, making the need to transport specie quite unnecessary, as witness the bill issued 12th October 1679 by Jacques Frémont of Amsterdam for Launay & Robert in Paris which passed through seven different hands.

The volume then presses on with a study on "Cost Estimating" by M. Gusatz and R. Squarzoni. The two authors have studied the exchange rates which were transmitted to Jean-Baptiste Roux by his correspondents in Cadiz in 1729. The rates show a wide range of variation, and this is studied against the movement of the Cadiz economy, which was punctuated by the arrival of galleons and mercury transports in February and March, followed by a freeze on the treasure until the amount of the indult had been settled, then the preparation of the fleet which was to sail on August 10th, the release of the American goods and the start of loading the future American galleons. The variation in the exchange rates was at its greatest in Genoa, and the variation from one correspondent to another followed a fixed hierarchy. A number of explanations for this are suggested, and I would guess the list is not meant to be exclusive. I am not fully persuaded, anyway, that it is men rather than events which fix prices and exchange rates; at least there must be some sort of sharing. But I do not intend to carp at a study undertaken in such a careful and accurate manner.

R. Squarzoni is not content, however, with a single trial of strength, and we now come to the study on "Speculation" which deals with arbitrage, that delicate operation by which the merchant bankers, after weighing the advantages and disadvantages of the available financial circuits, are able to profit from the variations in exchange rates in different places by choosing the most appropriate route. "Speculative arbitrage is based on the primary circulation of bills of exchange, on the drawings and remittances between merchants and bankers, on the flows of settlements and return arbitrage; it is a superstructure of the system of payments, of the monetary and banking edifice of the *Ancien Regime*". R. Squarzoni shows clearly what effective arbitrage was based on: a corpus of sound information, a real ability to calculate — which was in truth already greatly extended and standardized — an open eye, and speed in making decisions. He shows that the profits from these operations was in individual cases slight — from 1.5% to 3% — but that they formed a snowball on account of their short-term nature (about 3 months), so that by the end of the year they had accumulated an honourable place between 6% and 12%. R. Squarzoni

finally draws attention to a neglected feature of banking in the XVIIIth century, the creation of means of payment. This is particularly evident in arbitrage operations since the drawer issues a bill which is designed to extinguish itself when the exchange is completed, but which in the meantime playing its part in circulation, specifically for the purposes and within the framework of the speculation which he is undertaking.

Summary. Charles Carrière returns to write what many would judge to be an apologia or a panegyric on the book's subject, the bills of exchange. But in fact his essay is more rich and complex. He returns to the age of the letters of exchange, shows the flexibility of the technique employed, and attempts to assess their general importance in national (discussing, for example, the predominance of Paris) and international trade and finance. With an expert's touch he defines the divisions between commerce and agiotage, and discusses the relationship between the bills of exchange and precious metals, the part played by the Banks of Amsterdam and England, and the claimed inferiority of French financial operations in this period. This will be something to stir the specialists — who will inevitably disagree — and will be an eye opener for all.

Charles Carrière is an author who uses the personal pronoun, which is something that historians generally frown upon, preferring an impersonal presentation. But we should appreciate what lies behind this use of the first person, and in this case it is a sign of honesty. It is used here by someone who has acquired a sound grounding and confidence in interpreting documentary material, someone who also finds himself advancing into unexplored territory, knowing that his views and arguments differ from those of his predecessors, that there are problems which require further discussion, and that, until further discoveries are made, a degree of prudence is necessary. In such a situation I believe that the use of the first person is anything but a cause for censure. It is certainly no less worthy than the false impartiality of presentation behind which an author attempts to efface himself to create an impression — often quite against the dictates of his own conscience — that an issue is perfectly objective and definitive. The dangers of this are only too apparent today in the clichés to which so many historians resort, and it is this which not only justifies, but indeed cries out for, a return of the first person style, for this by itself immediately throws an issue open for debate. Charles Carrière has achieved no small success in convincing his fellow contributors — especially R. Squarzoni, who rather chastely uses “we” — to adopt the same style, which we should acknowledge as the sign of a spirit of inquiry and modesty.

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C. D. GOLDIN, *Urban Slavery in the American South, 1820-1860: A Quantitative History*, Chicago: University of Chicago Press, 1976, pp. xvi & 168, bibliography and index, 34 tables, 5 figures.

The potential of the "new economic" history to illuminate the past is here admirably illustrated. The past in question is that of urban slavery in the antebellum Southern United States. The widely known fact is that the extent of urban slavery — which never embraced more than one tenth of all slaves — declined after 1820.

This decline has prompted a variety of conjectures. In *Slavery and the Domestic Slave Trade* (1836), E. A. Andrews attributed the decline to the crystallization of Southern attitudes on the proper place of slaves in the Southern hierarchy: permanently inferior and naturally subservient to whites, these slaves needed constant direction by their masters; the greater freedom of town life deprived slaves of their masters' benevolent instructions, and perhaps gave slaves delusions of competency. In *the Slave Power* (1862), J. E. Cairnes attributed the decline of urban slavery to precaution against slave insurrection: free-moving urban slaves were an intolerable threat in vulnerable cities, but restricted urban slaves were by and large unprofitable. These alternative foundations, that urban slavery shirked masters' proper responsibilities to their slaves and that urban slavery exposed urban whites to intolerable threats, support historians' subsequent explanations of the decline of urban slavery. L. C. Gray in his *History of Agriculture in the Southern United States to 1860* (1958) concluded that urban slavery was "a serious nuisance and source of [slaves'] demoralization" (p. 513). And in *Slavery in the Cities* (1964), R. Wade likewise argued that the necessary freedoms of urban slaves transformed the master-slave relationship into an employer-employee one and thereby threatened the crucial Southern distinction between black and white. In sum, the traditional story is that urban slavery threatened the psychological or physical well-being of whites. In response, regulations on urban slaves were imposed and tightened uniformly, with the result that urban slavery declined throughout the South. The implication — drawn by Genovese, for example — is that if the profitability of cotton ever declined, then the slave South would stagnate.

Goldin turns this story on its head. First off, she looks at the data. The traditional story implies a uniform decline in urban slavery. In fact, the employment of slaves in different cities grew at different rates, sometimes positive and sometimes negative (p. 58). Furthermore, although older females (domestics) increasingly predominated in the urban slave force, their predominance varied across cities. Thus, the facts suggest that the decline of urban slavery did not stem solely from a pervasive and uniform crystallization of attitudes, but was importantly affected by the particular circumstances of individual cities.

What, then, can cut through this tangle of data and explain the course of urban slavery in the antebellum South? Goldin uses economists' favorite scissors, those of demand and supply. With their help, she successfully exposes the logic of urban slavery. The blade of demand, of course, relates the quantity of slave services demanded to the price of slave services, the price of alternative providers of like services (white workers and free blacks in Southern cities, for example), the value of slave services (measured by the size and income of the free population for cities and by the value of cotton for rural areas), the costs of regulation, and taste. The blade of supply relates the quantity of slave services demanded to the exogenous growth of the total slave population * and the value of slaves in alternative employments * (*asterisked determinants of demand and supply are explicitly considered by Goldin*).

Goldin's findings are indeed surprising. Taking Southern cities as a whole she finds that the demand for slaves in urban areas shifted *outward* in every decade. This outward shift was 91 percent in the peak decade, 1830-40, was 5 percent in the most slack decade, 1840-50, and varied considerably decade to decade (p. 104). Since this shift term embraces changes in tastes (crystallization of attitudes) and changes in regulatory costs, the conclusion is clear: the traditional story is wrong, wrong for every decade. Urban slavery did decline, of course; the quantity of slaves employed in cities fell 3 percent in 1840-50 and 24 percent in 1850-60. But the source of the decline was the rising price of slave labour (up 83 percent in 1850-60, for example) and the greater substitutability of free labour for slave labour in urban employments compared to rural employments. Thus, a boom in Southern agriculture pulled slaves out of cities. One confirmation of her finding is that the elasticity of demand for slaves in border cities (where free labourers were most readily available) was double that for Old South cities, and eight times that for New South cities (where free labourers were least readily available). Another confirmation is the pattern of slaves' exodus from cities: those least suited for fieldwork, skilled males and aged females, were left behind.

Goldin's findings bring us face to face with the reality of American urban slavery. That urban slavery disturbed many white Southerners is not disputed. That white urban workers sought to block competition from slaves is likewise admitted. That either the disquietened or the disadvantaged managed to throttle urban slavery is proved wrong. That cities and slavery were incompatible is conclusively denied.

For the importance of its conclusions and the simple elegance of its logic, it is a book recommended to all — and especially to those mistaken historians of American urban slavery yet living.

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A. HIRSCHMANN, *The Passions and the Interests: Political Arguments for Capitalism before Its Triumph*, Princeton, N.J.: Princeton University Press, 1977, pp. x + 153.

Having achieved international repute as a theorist and practitioner of development economics, Professor Hirschmann has been seeking new fields to conquer on the "fuzzy borderlands" between economics and the other social sciences. His *Exit, Voice, and Loyalty* was about rival methods of security efficiency and arresting decay in social institutions, economic and otherwise. His present monograph is admirably described by its own subtitle, "Political Arguments for Capitalism before Its Triumph".

Let us accept Max Weber's argument that Calvinistic Protestantism in particular, or perhaps a more general breakdown in ecclesiastical authority following the Reformation and religious wars, has induced or permitted to particular groups and individuals the roles of *wirtschaftende Menschen* as aids or manifestations of salvation. Hirschmann then raises the next question: Why was the larger society impressed so favourably by their example? Having asked this question, he seeks and finds his answer in intellectual history. Medieval and pre-Medieval Christian theology and ethics had condemned alike the passions for glory, for lucre, and for concupiscence. Feudal thinking had subsequently legitimized the first of these passions, but two main thrusts of late-Medieval and Renaissance social thought are interpreted by Hirschmann as: (1) Relying on the passions to neutralize each other in some sense, and (2) Treating the longer-term "interests" of rulers and of subjects alike as rational modifiers of the shorter-term passions. (Such treatment did not always imply praise, as witness Machiavelli's bad press for advising princes to follow their interests for localized wealth and power rather than their passions for glory and world empire). As time passed, to follow private economic interests and to lead a mercantile or bourgeois life came to be viewed as rational and calm (*doux*) preventives of warfare and simultaneously as restraints on governmental tyranny — even guarantors of justice if not precisely of liberty as we now define it. (Why should the sovereign kill the geese who lay his golden eggs?). In some cases, such views led to advocacy of *laissez faire* on political along with the standard economic grounds. In others, including the French Physiocrats, the implication was "legal despotism" or what we might today call "fine tuning".

Advocacy of an economic road to a political near-Utopia reached its XVIIIth century apogee in the neglected economic writings of Montesquieu in France and the better-known *Political Economy* of Sir James Steuart in Britain. The Scottish school, including David Hume and Adam Smith, seems not to have taken to this brand of political economism, perhaps because the Scots knew mercantile life too well and also, suggests

Hirschmann, because of affinity to the "noble savagery" of the Highlands. But the Montesquieu-Steuart doctrine has enjoyed belated revivals in such phrases as "the business of America is business".

This reviewer finds two main problems with the Hirschmann analysis — which is of course far more detailed, specific, and erudite than this condensed and distorted account. First, I am impressed by contemporary accounts of an "American Celebration" (1946-66?) — which I completely failed to experience — with the difficulty of defining the *Zeitgeist* of any period or country quite so readily as Hirschmann does — despite his wealth of citations and *explications de textes*. Second, I am sufficiently infected with the Marxian virus of economic interpretation to wonder whether these abstract ideas of apparently obscure theorists could really have had quite so much real-world influence as Hirschmann (and Lord Keynes, in the *General Theory*) assumes that they did. The political case for capitalism may have had more to do with the economic case *against* dynastic and religious warfare, or with the success of commercial cities in simultaneously fighting off robber-barons, robber-bishops, and robber-kings, eschewing Roman or Venetian passions for *imperium*, and developing substantial material prosperity, than with the musings of social philosophers.

Hirschmann and I also find ourselves on opposite sides of a long-standing controversy connected more intimately with the question of "capitalism and freedom" than with the argument of this particular volume. The controversy: How is a "freedom" related to the power to exercise it effectively? Can I be said to possess, e.g., free speech without the economic power to convince others that I am right, or even to resist economic reprisals by bosses, associates, or customers who are sure I am wrong? To me, bare legal immunity from prosecution suffices; Hirschmann appears from parenthetical comments to hold the opposite (majority) view. The political case for capitalism is easier to accept under my interpretation than under his.

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TH. KESSNER, *The Golden Door: Italian and Jewish Immigrant Mobility in New York City 1880-1915*, New York, Oxford University Press, 1977, pp. 224.

Thomas Kessner's *The Golden Door: Italian and Jewish Immigrant Mobility in New York, 1880-1915* is an admirable and daring work. As presented, it is an Italian and Jewish success story (although the Jews were considerably more successful than the Italians), and the setting is New York City, the "Mt. Everest of urban history". Using quantitative

materials incorporating federal and state census data and the Standard International Occupational Prestige Scale, the author argues that, in the 35-year period under consideration, Italians and Jews, the two largest immigrant groups to settle in New York City, were able to work their way up from the bottom of the city's social order to a degree hitherto unappreciated — primarily because New York City was a dynamic metropolis, constantly adding new work to old. This city provided expanding economic opportunities which matched the qualifications of the immigrant Italians and Jews and, consequently, enabled them to move upward, each, however, along its own trajectory — the Jews via the garment industry and retail trade, the Italians via construction, the “benchmark” of the Italian occupational experience in New York City even into the second generation.

Although *The Golden Door* is a major contribution to the New Urban history, the book does not confine itself simply to statistics and quantitative analyses. The author realizes that quantitative techniques have limits in explaining the story of immigrant social mobility. Statistics can tell us that the Jews and Italians experienced “x” amount of upward mobility in the period 1880-1915, and that the Jews were more upwardly mobile than the Italians, but such techniques cannot tell us why. For this one must look at other considerations such as the cultural or ethnic variables unique to each group. As the author rightly explains, “... ethnic predilections — previous experience, attitudes toward education, ambition for status and security — played far more important roles than the structural “pull effect” of the New York economy” (p. 67). Indeed, Professor Kessner has succeeded in blending the best of all possible worlds; he has merged quantitative data with literary and sociological sources to produce a synthetic work of high quality.

The Golden Door, however, is not without its weaknesses. The book, especially in its treatment of Italians, tends to be culture-bound and America-centred; situations and actions are interpreted through the prism of (Anglo) American cultural values, with little consideration for other cultural perspectives. Success, for example, which is ostensibly the issue at the heart of the book, is defined in American terms and is interpreted to mean upward occupational prestige or mobility usually accompanied by higher levels of education. The Italians, though, viewed “success” a bit differently. Indeed, recent findings of the National Opinion Research Center show that, even into the third generation, Italian (and Polish) Americans continue to be more likely than others to place greater emphasis on higher and secure incomes than on jobs which carry greater prestige but less income. In other words, they continue to operate in terms of the values of their ethnic “subcultures” rather than those of the larger society. In this sense, too, assimilation and “Americanization” have not proceeded quite as far as Professor Kessner assumes; hidden dimensions or non-material

forms of culture, including different values, continue to persist.

Furthermore, underlying the book's analysis is the assumption that immigrants came to America with the intention of becoming assimilated or "Americanized". They didn't. Unlike the migration of eastern European Jews, the southern Italian migration consisted mostly (75-80%) of men in the prime years of their lives; and 66-70% of the migration returned to Italy. The author mentions this, but fails to comprehend its true significance. The Italians, unlike the Jews, were not true immigrants, but migrant labourers, the vast majority of whom never had any intention of staying in America. They came only to earn money and then to return to Italy to buy land, reduce family debts or set themselves up in business; the Italian left for America only so he could return to Italy a better man. Contrary to what the author states, the Italians did not see themselves as "wretched, wasted, and uprooted bodies in search of new lives and broader horizons" in America (p. 4). (This smacks too much of the Handlinesque image of the *Uprooted*, an image long ago abandoned as false by scholars of Italian immigration). This may have been true of the Jews who were, in fact, uprooted, but it does not apply to Italians. The Italians had definite places to which to return and definite intentions to do so; and, as the author correctly points out, most of them did choose to return. The Italian migration needs to be viewed in its entirety and from the European as well as the American vantage point. Surely, the majority who returned tell us as much (if not more) about the nature of Italian immigration as the third or less who remained.

The author, however, argues that the Italians who returned to Italy were the least successful of the migration. He bases this assumption on the finding that 96% of those who returned to Italy did so via third class (rather than first or second class) and must have been unsuccessful in America. This interpretation is based on an American, not an Italian, definition of success, and totally misinterprets Italian motivations and goals. The idea was to save, not spend, money. One impressed the family and fellow villagers back home, not by returning first class, but by returning with as much money as possible. (Why waste money on an ostentatious passage, when the money saved could go toward the purchase of a farm or business?). Again, the author fails to comprehend that the Italians were migrants who came in search of work and money; they were not true immigrants.

Moreover, current research on Italian immigrants and their repatriation will not support the bold assertion that only the unsuccessful returned. Research on another migrant group, the Poles, also shows that the successful as well as the unsuccessful were equally inclined to return. Greater familiarity with Italian or Polish experiences would show that other factors, such as missing one's European draft call or the outbreak of World War One, were usually more important in encouraging one to stay than were considerations of upward social mobility. Finally, the author makes no

mention of the vast amounts of money sent home monthly, even weekly, by "unsuccessful" Italians who eventually returned to Italy.

If the author cannot understand why Italians avoided farms in America, it is also because he fails to appreciate that Italians were migrant-labourers; the land they coveted was in Italy, not America. Moreover, failure to understand Italian social structure prevents the author from realizing that it was easier for these people to recreate their network systems on the streets of urban America than on her isolated, detached farms — a form of land tenure alien to traditional Italian culture.

If the author cannot understand Italian reluctance to join labour unions, it is, again, because he forgets that Italians were migrants, not immigrants; their aim was to earn money, and strikes and union dues prevented or retarded this aim. Only when Italians changed their status from temporary to permanent did they become prime recruits for labour unions.

Finally, if Italians were "unable to transcend their lack of urban and industrial skills" and "at first took unskilled jobs..." (p. 70), it was often a matter of choice. The unskilled Italian construction worker of former "farmhand" was potentially a mason, carpenter, tailor, cook, shoemaker or baker, having performed many or all of these on occasion in his native village. He chose not to perform these occupations in America because he wanted quick, substantial cash — the very thing he had come for. Unskilled forms of construction activity, of limited duration and initially secured through a *padrone* (an important phenomenon in Italian culture which the author ignores), fitted his needs perfectly and hence attracted him. In short, the distinction between migrant-worker and permanent immigrant is an important one; the differing psychologies of the two groups affected motivations and hence patterns of behaviour, including work choices and "success" in America.

Despite, however, the author's difficulty in understanding Italian motivations and culture, *The Golden Door* remains a book of high quality and a major contribution to both quantitative and comparative ethnic history. It is a pioneering study, and, as such, its strengths as well as its weaknesses should serve as guides for future research.

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ST. KOBLIK (ed.), *Sweden's Development from Poverty to Affluence 1750-1970*, translated by Joanne Johnson in association with the University of Minnesota Press and Steven Koblik. Minneapolis: University of Minnesota Press, 1975.

-Thirteen essays by noted Swedish social scientists (nine historians, two political scientists, one economist, and one sociologist) dealing with a

variety of subjects on the last two hundred years of Swedish history constitute *Sweden's Development from Poverty to Affluence 1750-1970*, edited by Steven Koblik, professor of history at Pomona College, California. The editor points out in his extensive introduction that the purpose of the work is "to provide the English-speaking reader with a series of essays by leading Swedish scholars who can give us dependable and up-to-date information about the growth of modern Sweden" (page 4) as well as to familiarize the reader with current Swedish historiography. This second aim could have been better served had a brief biographical sketch of each contributing writer been included.

Sweden has always attracted much attention by its presumed unique political, economic, and social systems, yet some of these "sacred cows" are challenged. The concluding essay by Kurt Samuelson, for example, traces the philosophical supports of the Swedish welfare system with the conclusion, as underscored by Koblik, that the underlying foundations are really not so different from other West European welfare systems. Marquis Childs' thesis that Sweden always has been in the forefront of the peaceful "middle way" between business and labour is questioned by the editor, Berndt Schiller states that the "middle way" was not present in the "years of crisis, 1906-1914"; rather there were intense conflicts (ie, the General Strike of 1909, probably the longest anywhere in the world). Carl-Göran Andrae also pursues the theme of conflict in the Swedish body politic in his essay "The Swedish Labor Movement and the 1917-1918 Revolution". Karl Molin notes that during World War II, despite the national coalition, internal politics rather than being eliminated were further heightened.

Other thought-provoking themes include an interpretation by Nils Stjernquist of the 1809 Constitution (Sweden has the second oldest constitution in the world, only predated by the United States) as quite conservative in character (actually a series of compromises); the Swedish-Norwegian union from 1814 to 1905 was doomed because there was little held in common by the two nations (Jörgen Weibull); the transformation of Swedish agriculture took place before the beginning (1870) of the Swedish industrial revolution and "for a capital-poor country, such as Sweden was, this was crucial, because investments were largely concentrated in different sectors during different periods" (page 105, Lennart Jörberg contrasts this experience with contemporary underdeveloped nations); and the three major popular movements — religious revivalism, prohibition, and labor — between 1900 and 1920, due to the conservative nature of Swedish society, were forced to find common ground in order to achieve their ends in a nonrevolutionary manner (Sven Lundkvist). The fact that the Social Democratic Party became isolated from the nonindustrialized sectors of Swedish society again was a theme discussed by Sven Anders Söderpalm in

his essay, "The Crisis Agreement and the Social Democratic Road to Power".

Koblik introduces each essay with a succinct summarization of the period of history being reviewed. Not only does this aid the reader in acquiring quick background material, but also it helps to tie together the varied essay subjects. According to the editor in his introduction, each author was free to define his subject in his own way. Göran B. Nilsson's essay, "Swedish Liberalism at Mid-Nineteenth Century", attempts to define this term, and to analyze the methodological problems in studying intellectual history. The approach is quite different from the other more factual essays, and perhaps does not directly relate to the book's title, *Sweden's Development from Poverty to Affluence*. Jörberg was the only author to use footnotes.

This collection of essays indeed serves a useful purpose in further bringing to light certain key topics in modern Swedish history, and to suggest new directions for scholarship. It greatly supplements the general Swedish histories by such scholars as Ingvar Andersson, Eli Heckscher, and Stewart Oakley.

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F. MACHLUP (ed.), *Essays on Hayek*, New York, New York University Press, 1976, pp. xxiv + 182.

This is a book in honour and in praise of Friedrich A. Hayek, economist, legal philosopher and scholar at large, the co-recipient (with Gunnar Myrdal) of the 1974 Nobel Prize in Economic Science.

The editor makes clear that the volume is not a *festschrift* (since it contains relatively few essays, mostly about Hayek, rather than on some self-chosen topics from a larger group of celebrants). Neither is it a volume of conference proceedings, because the published papers represent merely the principal contributions delivered at a special regional meeting of the Mont Pelerin Society, in August 1975, but none of the comments and discussions. Milton Friedman, in his short Foreword, bemoans this limitation as necessary but unfortunate, and as a real loss to the reader. To convey at least some idea of the "intellectual feast" that took place in Hillsdale, Michigan, he reproduces the entire programme with the full cast of participants.

On the other hand, the book contains two post-conference additions: a comprehensive bibliography of Hayek's scholarly publications prepared by Fritz Machlup, and an appreciation "Friedrich A. Hayek: Nobel Prize-winner" by Arthur Shenfield.

The Mont Pelerin Society takes its name from the site in Switzerland where it was founded, at Hayek's instigation, in 1947 by a small group of selected, like-minded scholars concerned with and about a free society. It has grown since and spread in membership (currently 350 from 33 countries) but has chosen to remain a closed forum purely for scholarly intercourse. Judging from the contributions to the present volume, at least some members share not only a common dedication to individual freedom and an aversion against government interventionism, but a fierce loyalty to Hayek, the first president and subsequently life-long honorary president of the Society. Thus, in referring to the Nobel award to Hayek, neither William F. Buckley nor Arthur Shenfield was able to forego a sneer at Myrdal, presumably as one unworthy of sharing this honour with him.

The editor's own paper, "Hayek's Contribution to Economics", shows Fritz Machlup at his usual standard of perfection: comprehensive, yet concise and clear in exposition, and meticulous in every detail. He not only explicates Hayek's own work, mainly his contributions to technical economics (1928-1941), but places his ideas in perspective, relating them to those of earlier economists (e.g. Knut Wicksell) and contemporary or later scholars (notably J. M. Keynes). His up-to-date structured bibliography (through January 1976) is no small achievement in itself.

Similarly clear-cut is Gottfried Dietze's paper on Hayek's legal philosophy. Presumably, his characterization of Hayek as "a man of measure" (p. 145) — probably from the German word *Masz* but somewhat equivocal in English — refers to Hayek's dedication to restraint in his ideas on the rule of law.

In seeking to establish whether there was an anti-capitalist mentality among historians, and intellectuals generally, notably those of a liberal outlook, R. M. Hartwell looks back (p. 73 pp.) to a 1951 discussion held by the Society and later edited by Hayek; and William Buckley takes as his starting point (p. 95) Hayek's famous fighting credo, *The Road to Serfdom* (1944). But while Hartwell elaborates on some of Hayek's recurrent themes, such as unplanned (good) versus planned (bad) societal development, in Buckley's treatment the Hayek juncture seems purely incidental. Rather he remains true to form, his form: a surfeit of sophistication and plethora of polish and pompousness frequently without any discernible relevance.

Arthur Shenfield, the other militant in this group, does deal with Hayek's fundamental thesis, that "human society is the product of human action but not of human design", (p. 65) and with several related main themes in Hayek's philosophical edifice. E.g. (Shenfield): "but as a society is not a system of quantities but a system of relationships, its essentials cannot be measured" (p. 67). "Of course it is not to be denied that the

whole of a society is more than what the sum of its individuals would be if they had no contact with each other. Such a sum would not be a society at all" (p. 69). Yet he deprecates any holistic approach to society as being based on the erroneous presupposition "that the wholes are more than the sum of their parts" (*Ibid.*).

However, the reader's puzzlement at this and other basic contradictions in the position of Hayek and his sympathizers neither starts with Shenfield nor does it end there. The very lead article, e.g., by George C. Roche III, President of Hillsdale College, where the conference took place, pleads in resounding terms for a "heartfelt moral consciousness" (p. 5), since "no society can function effectively for long without a deeply felt consensus..." (*Ibid.*). And Hayek himself, in numerous passages, has spoken out against atomistic individualism and purely self-regarding motivations. Are "spontaneous order" and the model of the market the necessary and sufficient answers? And is the "libertarian" spirit likely to be the only or even the best guide to all human conduct, especially if, as Hartwell points out, the « "invisible hand" is not always beneficent » (p. 93)?

With such wonderments and doubts, one turns expectantly to Shirley Robin Letwin's concluding essay on "The Achievement of Friedrich A. Hayek". And a very special essay it is — albeit more on Hayek the man rather than his achievements. The author was a graduate student of Hayek's at the University of Chicago. From her deft and perceptive description based on years of close-range observation of her former teacher, Hayek emerges as a humane, erudite, sensitive and complex person of many diverse, yet converging interests; a scholars' scholar. Not being given to ascribing results to single causes, anti-reductionist, anti-utopian, committed to over-arching values and convinced of their crucial role in preserving community, one who « has taken great pains to dispose of the fantasy of "laissez-faire" conjured up by that "invisible hand" » (p. 162), Hayek appears to have every possible insight into man's past. Yet he permits his name to be widely associated with simplistic notions and slogans. And he virtually discounts man's every effort of consciously shaping his collective future.

While Hayek's achievements in technical economics are acknowledged (it is they which yielded him the Nobel Prize), and with due respect to Hayek the gentleman and scholar, one cannot help wonder whether all his writings published in the 23 years since *The Road to Serfdom* will prove any more helpful to mankind in finding (if not charting) its course past the multiple perils of our space-ship-Earth predicament than did that book in coping with the "Revolution of Rising Expectations" after World War II.

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M. P. LESNIKOV, *Die Handelsbücher des hansischen Kaufmanns Veckinchusen* [Forschungen zur mittelalterlichen Geschichte. Begründet durch Heinrich Sproemberg (†), Herausgegeben von G. Heitz, E. Müller-Mertens, B. Töpfer und E. Werner, Band 19]. Berlin, Akademie-Verlag 1973, pp. XL + 560.

Hildebrand Veckinchusen is the most famous of the German merchants of the Hanseatic area around 1400 — not only as a result of his life and economic activity, but because of the fate of his ledgers and their interest for historians. Born in Westfalia around 1370, he lived at Bruges in the years 1395 to 1398. Towards the end of 1398 he went to Riga where he married the daughter of the merchant Engelbrecht Witte. In the autumn of 1399 he moved to Lübeck. Two years later, 1401, he returned to Bruges where he remained for 24 years, the three last of which he spent in prison because of debts. This may have been the main reason for his return to Lübeck, where he must have soon died.

His ledgers and other business papers were deposited in the town archives of Reval (Tallinn). How they came there is not known, but since the 1870s they have attracted the attention of historians. In 1874 E. Papst published a part of the papers in the form of "regests". In 1789 Wilhelm Stieda, professor at the university of Dorpat (Tartu), began to make use of the material, but unfortunately he never completed the edition of all the papers he had envisaged. He only published two separate studies, one on Hanseatic-Venetian trade in the XVth century (1894), the other on the correspondence of Veckinchusen (1921).

After Stieda, who died in 1933, Fritz Rörig and his student Claus Nordmann began to prepare a new edition which was interrupted however by the war and by the death of Nordman (cf. *Hansische Geschichtsblätter* 67/68, 1942/43, pp. 21-24). Nordmann, however, worked from photocopies which had been made from the originals and were kept in the Archives of Lübeck, and so had no access to the originals at Reval. There were evacuated during the war by the Germans and were finally lodged at Göttingen in the "*Staatliches Archivalager*" — with two exceptions: Af1 and Af6. After 1940 Michail Pavlovich Lesnikov, professor at the Moscow Polytechnic of Trade, began to copy the ledgers and finally started to prepare an edition of the first volume, which is the subject of this review. It will be greatly appreciated that a Russian colleague has dedicated years to copying the ledgers and related papers and to understanding their contents. The value of his research is the higher because German was not his mother language and, beyond that, the German of Hildebrand Veckinchusen was not XXth century German but the "*Niederdeutsch*" of the late Middle Ages with its difficult Gothic script. One should also mention the names of the late Professors Heinrich Sproemberg and Paul Johansen who encouraged the author to go on and helped him, and the "*Forschungen*

zur mittelalterlichen Geschichte", founded by Sproemberg, and now under the editorship of his students, which provided a forum for the fruit of Lesnikov's work.

Before this volume came out, Lesnikov already published several studies in which he made use of the Veckinchusen papers. In 1958 he published an article on the Livonian merchants and their trade with Flanders in the early XVth century (*Zeitschrift für Geschichtswissenschaft* 6, 1958, 285-303).

In 1961 a study on the Hanseatic fur trade in the same period followed in the *Festschrift* for Sproemberg. Finally Lesnikov prepared a paper on the various Veckinchusen societies for the International Congress of Economic History at Munich in 1965 (cf. *Troisième Conférence Internationale d'Histoire Economique*, Munich 1965, Paris - La Haye 1974, pp. 37-47). Here he shows that within the gamut of the societies or companies established by Veckinchusen we meet all the forms which are known at that period, and especially the "sendeve", albeit that the name "sendeve" does not appear in the papers. Beyond that, the travelling merchant, the system of commission as well as the "wederlegginge" given to a "Geselle" meet besides each other.

The principal company in the whole complex was the "selschap" which Hildebrand Veckinchusen and his brother Sievert at Lübeck had established in 1401 as a loose union between the two merchants. It well fits the concept of "Fernhandelsgesellschaft auf Gegenseitigkeit" a term which derives from Gunnar Mickwitz' studies on the Reval companies in the XVIth century. During the years 1403 to 1406 the "selschap" made contracts with associates at Danzing, Riga and Dorpat which were separately accounted, so what one *Ferngesellschaft* entered the accounts of another as a partner. The whole was a loose system of a parent and daughter companies, a form which was not unusual in the North. But the Venetian company of 1407 (*Venedische Gesellschaft*) was different. It was the largest company in the North known at that time, and had been established to trade between Lübeck-Brügge and Venice and between Cologne and Venice, and the main participants lived at Cologne and Lübeck. An important figure in the organization was Peter Karbow at Venice. Lesnikov saw this as a rough imitation of the *Ferngesellschaft*. Here Lesnikov may be corrected, but the structure of the Venetian company makes a better sense when seen within the tradition of the Mediterranean commercial companies.

The Venetian company is not mentioned in the volume under review. The reason is that only the texts of two books *Af1* and *Af6* are published here and they do not mention the Venetian company. Unfortunately, the author has held his analysis of the text in reserve for the publication of the second volume.

Here in the "Introduction the author only gives a survey on the

general importance of the papers in terms which go far beyond the specific case of the Veckinchusen enterprise. In fact the rich details give a most detailed inside view of the reality of a merchant's activity ranging across the wide hanseatic horizon from Novgorod to Bruges and the salt ports of the Atlantic coast, with many incidental references to trade with England, Scandinavia, Cologne and Venice. He also briefly describes the whole set of 13 books one of which for the time being is lost (Af 10 - Af 3). They form two main groups; the one is "memoranda" or "memoriale" with entries which are written with no general order, whereas the other one consists of account books written in good order. Here Lesnikov discusses the differences between his method and that of Nordmann, and argues that his method of presenting the sources in the form in which they were left by Veckinchusen is the right one, and especially because the whole collection only amounts to some 700 folios.

In a second chapter the author discusses the dates of the fairs mentioned in the books. In an introduction to the edition of the two books *Af 1* and *Af 6*, Lesnikov gives a short description of the editing principles he followed. *Af 1*, which consists of 144 folios called "*Schuldbuch*", contains entries from 1399 to 1406. The other book, *Af 6*, consists of 200 folios and has entries which range between 1400 and 1420.

Pages XXXIII and XXXIV contain the trade marks of merchants mentioned in the books, and show how each merchant used his own trade mark, although there existed no company trade marks. Finally a good index of persons, places and topics, as well as an index of money and coins mentioned in the two books, help the reader to use the text. Lesnikov intends to publish the rest of the books in a second volume. Let us hope that he uses the opportunity to add in that second volume an analysis of the entire contents based on a study of the various aspects which are covered. He has the best knowledge of the whole "collection" and is the only one who can give us such analysis and general interpretation.

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F. MELIS, *Mercaderes italianos en España, siglos XIV-XVI* (investigaciones sobre su correspondencia y su contabilidad), prólogo de F. Ruiz Martín, Universidad de Sevilla, 1976 (Anales de la Universidad Hispalense, serie: Ciencias económicas y empresariales 1) XXXI, 202 p., 1 map, facs., tables.

All those who are interested in the economic history of the Renaissance period will be grateful to the University of Seville for having published

this volume, which contains the papers of the late Florentine scholar Federigo Melis, on the growth of Spanish trade and Hispano-Italian relations in the late Middle Ages. Some of these papers have already been published in various periodicals and in proceedings of congresses which are not readily available to many scholars, and one has not yet been published at all. All are based on the unique Datini archives which comprise 23,000 letters, 5000 other documents and 110 account-books referring to Spain.

Although these papers deal with many subjects, the author time and again draws attention to major results of many years of painstaking research in the rich archives of Francesco Datini. The Italian merchant of the later fourteenth century distinguished himself from traders in preceding periods by a more systematic exploration of markets and trading conditions in general. According to Melis, this great effort was the "*causa causarum*" of economic progress in that period and generally speaking of the ascendancy of the Italians (p. 187). The Italian merchants of the Renaissance period, those of Tuscany first, had their news services, and they were always well informed about changes in marketing conditions. Their commercial methods were different from those of earlier ages, and were more simple and more efficient. Instead of the *commenda* or the marine loan, they used the *accommandit* agency, and instead of applying to notaries for every contract, they made the agreements directly with their partners. The use of these new methods was taken over by the Catalans and made it possible for example for the Catalan cloth industry to compete on international markets with the products of other countries (p. 190). At the end of the fourteenth century even the structure of the trading companies changed: instead of big centralised companies with branches in different towns, the Florentines, always in the lead, founded independent companies which were united by a single chairman or major share-holder. Consequently, the insolvency of one company did not entail the bankruptcy of the whole concern. Another change in commercial methods was the greater differentiation of freight charges. The author considered that this was a really revolutionary step with far reaching consequences for shipping, trade and consumption. It made possible the development of seaborne trade for cheap goods (p. 145, 193) and was later also extended to river shipping (p. 170).

Melis was rightly very much concerned with the history of shipping and believed that the "opening of the Straits of Gibraltar", meaning the regular seaborne trade between the Mediterranean and the Atlantic regions, was a great achievement and a very important step in the development of world trade. The regular shipping through the Straits, from the end of the fourteenth century, brought about more intense traffic on the Continent, and first of all in Spain. Melis emphasized in this context the great role of Basque shipping. The Basque ships were the first to engage in long

distance transport in the Atlantic, and were mostly employed by the Florentines, Siennese, Lucchese and the traders of Lombardy in Bruges (p. 188). This supposition should lead to a fresh appraisal of the role of the Venetians, who from 1314 onwards had a regular galley service to Flanders. Melis maintained in fact that the role of the Spanish fleets in the development of shipping in the period has been underestimated (p. 194). For in the fourteenth century, when Basque and Catalan shipping was no more developed, the Castilian fleets played a great role. The Basques were also the first among the Atlantic seamen to sail into the Mediterranean, at least from the 1340s. At the end of the fourteenth century Portuguese, English and even German ships (p. 195) were to be found in the Mediterranean and voyages from Alexandria to Flanders had become almost regular (p. 163, 193). In the fifteenth century owing to the intense activities of Florentine traders Lisbon became a focus of international seaborne trade, and was certainly the most important port in the Atlantic coasts of Europe (p. 172).

This is the background against which the commercial relations between Spain and Italy in the later Middle Ages should be seen. Melis outlined the development of both Italian trade in Spain and of Spanish trade in Italy. The Genoese and the Pisans were the first foreign traders to settle in Catalonia, Valencia and on the Balearic Islands. From 1260 onwards they are to be found in Andalusia (p. 187). In the fourteenth century Italian traders of other towns (inland towns) also engaged in trade with Spain and founded trading companies there. In this connection Melis stresses once more that the Florentines played a leading role. Whereas Bruges had been the seat of rich and enterprising Florentine firms from the beginning of the fourteenth century, they also later began to trade in Normandy and in Spain. In the late fifteenth century they carried on very successful trade in Seville, and in the first half of the sixteenth century Lisbon was their headquarters in the Atlantic.

As a consequence of these intense commercial activities Spanish wool dominated the Italian markets until the middle of the fifteenth century. Spanish wool had been imported to Italy previously, but this trade became really voluminous only at the end of the fourteenth century, from 1375-80 (p. 145). In his address at the first Conference of the Datini Institute (in 1970), which is reprinted in this volume, the author draws a vivid picture of the export of Spanish wool from the Maestrazgo and neighbouring regions to Italy. In the Datini documents he found no less than 147 wool producing areas where the agents of the Italian firms bought wool. Tortosa was a port from which wool was shipped to Provence and Italy. The wool of Castile was exported from Valencia (and even from Cartagena). The wool ships sailed from town to town along the "Levantine" coast of Spain, from Valencia to Barcelona, loading raw material everywhere for the

flourishing cloth industries of their own countries. Languedoc imported wool from Maiorca, but also from other regions. Melis classified the wool of various regions; that of Minorca was the best and had double the value of Maestrazgo wool (p. 149). The leading wool port in Italy was Porto Pisano, from which the towns of Tuscany and Lombardy were supplied. The role of Genoa and Venice in this trade was more limited.

The author also deals extensively with the export of other commodities from Spain. His paper on Malaga in the economy of the fourteenth and the fifteenth centuries is a real masterpiece of mature scholarship and brings home to the reader the great qualities of the late scholar; it is an excellent example of his historical method - the reconstruction of trade routes and the development of industrial production through the systematic analysis and interpretation of commercial documents. By analysing a letter from Tuccio di Genaro, formerly an agent of Francesco Datini and later of the Florentine firm of Ricciardo degli Alberti, Melis sheds light on the role of the still Moslem Andalusian port in international trade in the early fifteenth century. He cites many hundreds of Datini letters to explore the export of Spanish fruit, sugar and silk, and shows that Spanish products were marketed all over the Western basin of the Mediterranean, whereas the Adriatic area and its hinterland were supplied by Venice with Levantine products. But Malaga served also as an important port for the export of Flemish and English cloth into the Mediterranean world (p. 49 f., 54).¹

The greatness of Melis lay in the rare combination of an ability to grasp the bold lines of historical development with a knowledge of detail. Another feature of the papers, which are printed in this volume, is their originality. Only a glance is needed to show that the author seldom relied on the research of other scholars, and his conclusions are drawn from documents he had studied himself.

Scholars who have worked on the economic history of other periods will be interested by the great historical continuity evident in certain branches of Spanish exports. Some commodities which the Datini documents show to have been staple Spanish exports at the end of the fourteenth century had the same role much earlier, when a great part of the

¹ When discussing the alum trade the author (p. 50), relies on the paper of M.L. HEERS, "Les Génois et le commerce de l'alum à la fin du moyen âge", *Revue d'histoire économique et sociale* 32 (1954), p. 31 ff., and claims that alum was shipped to Southern Spain mainly by the Genoese. This statement should be modified. Egyptian alum was in that period still exported by the Venetians, a fact which has been overlooked and even flatly denied by scholars not familiar with Venetian sources, see CL. CAHEN, "L'alun avant Phocée", in the same *Revue* 41 (1963), p. 444 ff. For the export of alum from Egypt by the Venetians see *ASV Senato, Misti* 26, f. 89a (1352) 43, f. 65a f. (1395), 130b f. (1396) 45, f. 18a f. (1400), 46, f. 32b (1402).

Iberian peninsula was under the rule of the Moslems. Andalusian figs were famous in the days of the Omayyads of Cordova, as is shown by the accounts of Arabic geographers.² The same is true of Spanish leather³ and silk was exported from Spain according to Ibn Khurdādhbeh in the Ninth Century and according to Ibn Hauḳal in the Tenth Century.⁴ Many Judaeo-Arabic *geniza* documents testify to the continuation of this export trade in the eleventh and twelfth centuries.⁵

The economic history of the Near East helps modify some conclusions drawn from the Datini papers. The upward trend in the export of Malaga sugar, for instance, was probably closely connected with the downward trend in the production (and the export) of Levantine sugar. The data given by Melis (p. 32) show that Levantine sugar, and mainly the middle price sugar, disappeared from Western markets around 1400. It was no longer to be found there, because many sugar factories in Egypt and in Syria had been closed when production costs, due to labour shortages, became too high.⁶ Venetian sources also show that even Venice imported considerable quantities of Malaga sugar.⁷ Consequently the differences in the prices of the various kinds of sugar (p. 31) does not necessarily indicate differences in quality, but also may reflect production costs. When discussing the international trade of Southern Spain, Melis mentions (p. 11) ships sailing from Seville to Jaffa. Although Ramla was visited by European traders in the fifteenth century, Jaffa (its port) served almost exclusively as the harbour where the pilgrims went ashore.⁸

Exclusive concentration on the archives of a great commercial firm naturally has its pitfalls. Melis concludes that Spanish goods were exported to Southern Italy and Sicily mainly by the Genoese (p. 197), but a great number of notarial deeds in the archives of Palermo show clearly that this

² See MELIS, p. 37, 185 and cf. AL-MUKADDASĪ, p. 235 and E. LÉVI-PROVENÇAL, *Histoire de l'Espagne musulmane* III (Paris 1967), p. 283.

³ See MELIS, p. 44, 196 and cf. LÉVI-PROVENÇAL, *op. cit.*, p. 307.

⁴ See MELIS, p. 19 ff. and cf. IBN KHURDĀDHBEH, p. 153 and IBN HAUḲAL, éd. Kramers, p. 114.

⁵ See S. D. GOITEIN, *A Mediterranean society* I (Philadelphia 1967), pp. 102, 222; N. A. SYLLMANN, "The eleventh century merchant house of Ibn 'Awkal", *JESHO* 16 (1973), p. 73 ff.

⁶ I deal extensively with this in my paper "Levantine sugar industry in the later Middle Ages", to be published in the *Proceedings of the Conference on the economic history of the Near East*, Princeton 1974.

⁷ *Misti* 46, f. 90a (1403). So the demarcation line between two areas in the Mediterranean, one supplied with Spanish products, the other by Levantine, was not clear cut.

⁸ I also doubt if Calcutta (p. 172) is rightly interpreted. In that period the European and Near Eastern merchants went to Calicut, on the Malabar Coast, a great emporium of the pepper trade.

trade was carried on mainly by Catalans.⁹ Also what the author has to say about the continuous export of cloth from Brussels, Malines and Lille to the Mediterranean are (p. 53), should be modified, because Venetian documents shew that the cloth of Brussels and Lille disappeared from Levantine markets in the later fourteenth century.

These observations, which certainly are not intended to detract from the outstanding value of the author's achievements, should indicate the nature of the task which economic historians of the Mediterranean world must now undertake, namely the combination of the information found in the Datini archives with those contained in the Venetian and Oriental sources.¹⁰

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L. PANITCH, *Social Democracy and Industrial Militancy: The Labour Party, the Trade Unions and Incomes Policy, 1945-1974*, Cambridge: Cambridge University Press, 1976, pp. 311.

Incomes policy, the relationship between government and the trade unions, and quasi-corporatism constitute the most significant group of questions in contemporary British politics. This important and well written book by Leo Panitch treats these subjects in a lucid and provocative manner. Along with the recent writing of others such as Lewis Minkin, the book also provides a sophisticated contrast to a considerable body of superficial and sometimes journalistic and polemical work by political scientists and economists who identify trade union behaviour as the source of most of Britain's political and economic ills. Far from accepting such a characterization, with its attendant picture of pusillanimous Labour governments catering to inflationary union demands and disruptive practices, Panitch

⁹ *Archivio di Stato Palermo*, Notai 79, Enrico Di Citella, f. 142b ff., 147a f., 151a, 152a, 159a; 798, Antonino Aprea, f. 14b; 823, Nicolo Aprea, f. 14a; 843, Giacomo Comite, sub February 24, 1429, November 9 and 22, 1429; 844, idem, sub September 28, 1434; MSS. 34, Conti di Nicolo Castagna, Tesorero del Regno 1397-99, f. 117a; Gravezze 352, f. 139b.

¹⁰ On Venetian trade in Malaga see e.g. *ASV Senato, Misti* 45, f. 1a (of March 4, 1400, decision to send an ambassador to the Moslem king of Granada, because of the great volume of Venetian trade in Malaga), 7a (of March 26, 1400, order to the Venetian consul in Malaga to collect a tax of 1/2% from the Venetian traders in this town) 46, f. 11a (decision, in 1402, to suppress the consulate in Malaga), 14b (the decision was annulled).

provides a careful scholarly treatment of the complex inter-relationship between government and the trade unions since World War Two.

What makes this book of particular interest is the combination of both diligent historical research on the background and evolution of these interactions and the development of several controversial theses about contemporary British political economy. The first of these theses is that "the Labour Party has performed the functions of an integrative party in the British political system" (p. 235) and that the party's frame of reference has not primarily been focused on the class solidarity, economic interests or political power of British workers. This analysis of Labour in power as an integrative and accommodationist party following policies within a conservative paradigm offers a means for the author to account for the inability or unwillingness of Labour governments to follow particularly socialist policies when in power.

It is this theme which provides the basis for Panitch's treatment of incomes policy and which enables him to develop a number of important points. He argues that a particularly heavy governmental economic policy emphasis has fallen upon wage restraint because of the undercapitalization and inefficiency of large sections of industry, a relatively slow economic growth rate, and a marked dependence on external trade, all of which made the UK susceptible to pricing itself out of international markets and to balance of payments deficits (pp. 238.239). But the roots of a continuing government-trade union tension over incomes policy lie in the fact that wage restraint in a class-divided society such as that of Britain has decidedly inequalitarian implications. Indeed, Panitch identifies this important theme emergent as early as 1941 (p. 7). What is particularly important about his emphasis is that it addresses a crucial distributive dimension (i.e., the question of who gets what). This question has largely been ignored in recent discussions of incomes policy as they fail to take into account an important reason for recurring trade union discontent. Panitch also effectively conveys the remarkable degree to which the TUC proved accommodating toward Labour government efforts at incomes policy, despite the neglect of government efforts at redistribution:

Labour's incomes policy of 1964-70, like the wages policy of 1948-50 before it, was not designed to facilitate the planning and redistribution of incomes as part of a larger design to transcend the existing economic system... It was concerned rather to further the integration of the trade unions into the existing political and economic system by obtaining from the trade unions a more explicit acceptance of their responsibilities to society... (p. 241).

Ironically, the relatively orthodox economic policies followed by these governments were not so much forced upon them by the IMF but represented a basic underlying consensus shared by leading governmental economic

policy makers and businessmen on the proper course of action (p. 243).

These considerations lead Panitch to criticize quasi-corporatism and the analysis of the new group politics by Samuel H. Beer. Panitch argues that government consultation with unions has often been on a *post facto* basis and he asserts that "the pivot of power in British society continued to rest with Britain's capitalist class" (p. 243). He also argues that corporatism in Britain faces a difficult dilemma: in the name of social harmony, it suggests policies (e.g., agreement on wage restraint) which can be achieved only through coercive legislation which in turn would abrogate the very liberal democracy to which its advocates subscribe.

This argument is an interesting one, but it tends to overstate the coerciveness of Labour's abortive "In Place of Strife" proposals of 1969 and of the 1970-74 Conservative Government's Industrial Relations Act, the provisions of both of which were far from draconian.

This book is at its best in providing a sophisticated analysis of Labour government economic orthodoxy, TUC accommodation to and relationships with the Labour leadership, the impact of distributional questions, and the serious limitations inherent in successive British incomes policies. These assure Panitch's work a place among the most significant recent books on contemporary British politics. With these points in mind, however, the most debatable aspect of *Social Democracy and Industrial Militancy* is the alternative which, in the last few pages of his book, Panitch poses to the gravely flawed economic policies of Labour governments.

In criticizing "the grotesque distortion of Labour's social reform policies", Panitch makes a number of references, implicit or explicit, to alternatives. These include references to Labour as "a working class party committed to extending public ownership and control and to raising working class living standards" (p. 247), a criticism of Labour's tendency "to reinforce the working class's conception of itself as a subordinate class rather than a hegemonic one" (p. 254), and a mention of "a commitment to bring the working class to power" (p. 257). He also notes the failure of the Labour Left in changing the Labour Party and attributes this to its acceptance of the doctrine of "Parliamentarism" and its refusal to come to terms with the Party's "class harmony orientation".

These points have a certain quality of being delivered *ex cathedra*. More important, they shed little light on the policy choices which even a more explicitly leftist government could make in coping with Britain's difficult economic problems. In short, the author has good reason to pose the question of alternative policies, especially in terms of how a more fundamentally reformist or socialist government might govern in these circumstances, but his suggestions are both unconvincing and undeveloped. In particular, they do not convince this reviewer that there are not inter-

vening alternatives between a seriously flawed orthodox fiscal policy on the one hand and a vague invocation of working class hegemony on the other hand. Perhaps this suggests the need for another book. If Professor Panitch chooses to undertake such a task, the precedent of the work under review here suggests that it is likely to be both important and stimulating.

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M. ROMANI, *Storia economica d'Italia nel secolo XIX (1815-1914)* con una scelta di testi e documenti. Part II: *L'Economia del nuovo regno sino alla grande crisi agraria (1860-82)*, Milano, Giuffrè, 1976, pp. 1,200.

The study of the contemporary economic development of Italy has been greatly advanced over the last decade by a number of often excellent contributions which have succeeded in filling out our understanding of the economy in the years after Italian unification. But despite this effort and activity there still remain, as has recently been observed,¹ far too many gaps and unsettled problems, too many important issues which have scarcely been touched — some of them of crucial importance — with the result that it is still difficult to present a full reconstruction of the economic situation and development of united Italy. One of the reasons for this must lie in the absence of any general study which would be suitable to place on undergraduate reading lists and would also go beyond the still valuable and essential work of Luzzatto,² in so far as an up-dated bibliography and the whole discussion of economic growth is concerned.

This was the task which Romani, before his untimely death, had set himself. On the basis of a set of well developed methodological assumptions, he proposed to write an economic history of Italy in the XIXth century which was designed with teaching needs specifically in mind. The first part, on the economy before unification, was published in 1970,³ and the volume under review constitutes the second part. The present volume was intended to cover the period up to 1888-94, but in fact now concludes

¹ G. MORI, "Appunti e Spunti per una riconsiderazione della storiografia economica sull'Italia postunitaria", *Rassegna Economica del Banco di Napoli* 1977, n. 1, p. 25 ff.

For a comprehensive review of the literature up to 1965, see: L. DE ROSA, "Vent'anni di storiografia economica italiana (1945-65)", in *Ricerche Storiche ed Economiche in Memoria di Corrado Barbagallo*, Vol. 1, Napoli 1970.

² G. LUZZATTO, *L'economia italiana dal 1861 al 1914*, Vol. 1, 1861-1894. (Banca Commerciale Italiana, Milan 1963). The latest edition is in the Einaudi "Reprints" series (1974).

³ M. ROMANI, *Storia economica...* Introduction & Part I, 2nd ed., Milan 1970.

in the early '80s, the period of the onset of the agrarian crisis. The two chapters which make up the volume were designed as part of a broader study. There are some problems which appear to be overlooked — the "Southern Problem" is hardly referred to, for example — and it is clear that the author planned to deal with such issues in the part that was never completed.

Romani's method provides an interesting response to some of the historiographical gaps which we mentioned. Following a basic description of the political and economic situation of the period, the author provides a massive appendix which includes a chronological representation of the principal political and economic events, statistical materials, bibliographical references, together with a huge collection of extracts from official documents (laws, acts of parliament, ministerial reports, etc) and contemporary writings. The careful selection of these documents helps point to some of the fundamental problems of Italian economic history in the period. To give an example, the parliamentary discussion on reform of the customs tariffs, the government reports on standardization of the taxes on land and income, speeches made at the industrial exhibitions, etc, are amongst the extracts published in the volume.

The value of such a volume for teaching purposes is clear, and the composition of the study means that topics can be discussed with direct reference to original documentation. It is unfortunate that the very high cost of the book will in some sense limit its usefulness in this respect. The author's general interpretation of the situation of the Italian economy does not in general differ greatly from that of Luzzatto, and the bibliographical section contains very wide references which as well as informing the text indicate recent research contributions and hypotheses. Clearly this is a book of great value and one can only regret that the author was unable to carry his discussion through until the eve of the First World War as he had intended.

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