REVIEWS OF BOOKS

PH. S. FONER - R. L. LEWIS (eds.), *The Black Worker, A Documentary History from Colonial Times to the Present*, (2 vols), Philadelphia, Pa., Temple University Press, 1978.

Philip S. Foner's and Ronald L. Lewis's *Black Worker* makes an important addition to the growing body of published documentary material on blacks in the United States. Focusing exclusively on blacks as workers, the collection illuminates themes vital to understanding the black experience as a whole.

Volume one intentionally ignores documents relating to agricultural slavery, focusing instead on southern industrial slavery and northern, free black life. With that caveat, it illustrates the variety of black artisan trades and the difficulties blacks faced in following them. Some of the more interesting material examines the conditions among the numerically large, but little studied black seamen. Other material depicts living and working conditions among blacks in such northern cities as Philadelphia, New York, Cincinnati, and Boston. Volume one provides evidence not only of remarkable white solidarity with northern blacks but also of a recurring pattern of white violence that culminated in the 1863 New York Draft Riots. The last quarter of volume one examines the postbellum era when former slaves left their bondage only to face proscriptions ranging from legislative Black Codes to systematic violence. It also depicts the continuing difficulties northern blacks faced in practising their trades, focusing especially on the case of Frederick Douglass's son, Lewis H., who was excluded from the Columbia (D.C.) Typographical Union.

One of the more important sections of Volume one documents the theme of inter-racial labor solidarity, which the most enlightened of the white labor

newspapers enunciated. Volume one concludes with documents illustrating organized efforts among black workers to confront problems facing them (especially in the South) and to form an alliance with the northern, white labor movement.

Many of the most critical documents of the organized black labor movement appear in Volume two. It provides the entire proceedings of the December 1869 organizational meeting of the Colored National Labor Union (CNLU), and those of the next annual meeting in January 1871. It salvages from obscurity in local newspapers the proceedings of important state-level conventions of black workers in Georgia, South Carolina, Virginia, and Alabama. It also publishes excerpts from the proceedings of similar meetings in Texas, New York, and Pennsylvania held during the 1869-1872 life span of the CNLU. Similarly, a section devoted to labor militancy during Radical Reconstruction offers tantalizing items from local newspapers that depict black labor organizations and their frequently militant strike actions, especially in Louisiana, South Carolina, Texas, and Virginia. It includes important documents on the continuing efforts of black and white workers to break down the boundary of color that divided them. Volume two concludes with material on the Black Exodus to Kansas from the lower Mississippi Valley shortly after the end of Radical Reconstruction.

Taken by itself, Volume two constitutes a major contribution to understanding post-bellum labor relations. Whereas Volume one covers a much longer time period and a greater diffusion of topics, Volume two focuses on one decade and centers around the organized labor movement and concerted labor actions. The documents in Volume two illuminate the ideology of the black and white labor movements and point out the similarities and differences between the two. For example, most white labor leaders considered the interests of labor and capital antagonistic, while the black leaders hoped to harmonize interests they perceived as essentially identical. Similarly, whites preferred to stay aloof from both major political parties, while blacks preferred to remain steadfastly loyal to the Republican party, their party of freedom and equal rights. The documents also reflect concerns black workers shared with whites, for example, land ownership, cooperative enterprises, strike actions, education, socialism, and greenbackism. Finally, documents from the racially progressive *Workingman's Advocate* and the Boston *Daily Evening Voice* offer a perspective on the black labor experience that ties it into the national labor experience of the 1870s.

For the most part, the documents reflect the research interests over the years of the senior editor, Philip S. Foner. Readers acquainted with his published work, from his *History of the Labor Movement in the United States* (4 vols., N.Y., 1947-1965) to his recent *Organized Labor and the Black Worker* (N.Y., 1974), as well as numerous articles, will find much familiar material in the *Documentary. History.* Nevertheless, there is much new material, especially in Volume two.

Historians more interested in the "social history" than the "organizational history" of black labor will neglect the volumes only at their peril. Both offer a goldmine of information on virtually every aspect of the black labor experience.

The editors rely on nineteenth-century newspapers for the bulk of the published documents. Such a strategy entails both strengths and weaknesses. On the one hand, the black, labor, and abolitionist press provides information unavailable in any other source. Similarly, local newspapers offer the only source, however biased at times, of much local activity of black workers. On the other hand, too heavy a reliance on newspapers considerably narrowed the editors' pool of publishable documents. For example, the entire period before 1830, that is, before the appearance of the black and abolitionist press, receives scanty attention and the few documents published are inconsequential. Furthermore, excessive newspaper reliance dictated that other available sources remained virtually untapped. For example, published laws could have added to the otherwise thorough consideration of legal proscriptions against free black workers. As it stands, the editors reproduce none of the post-bellum Black Codes mentioned, and all their documents on proscriptive legislation come from newspaper sources. Overall, the weaknesses of newspaper reliance work to the disadvantage of Volume one; the strengths work to the advantage of Volume two.

Minor problems in annotating the documents occur, yet such problems are unavoidable in a collection of this nature. In most respects, the excellent index in each volume compensates for annotation flaws. In some instances, the editors' decision to include certain documents is open to question, for example, in volume two, where they use fifty-seven pages to publish testimony into Ku Klux Klan violence in South Carolina. The importance of violence in the lives of black workers is undeniable, but the editors could have edited the testimony so as to publish only the most relevant passages.

The qualification expressed above must not detract from the overall significance of the volumes. They stand as a tribute both to the years of patient, thorough, and often unrewarded labor taken to compile the documents and to the central importance of the black worker in the overall black experience.

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D. LEVINE, *Family Formation in an Age of Nascent Capitalism*. Studies in Social Discontinuity. New York, San Francisco, London: Academic Press, 1977, pp. xiv, 194.

This book presents the results of an intensive study of family and demographic patterns in four English villages between 1600 and 1850. Far from being just a demographic study, however, it thoroughly commits itself to demonstra-

ting that major economic and social transformations taking place in certain regions of England before the Industrial Revolution and its early stages were at the root of contrasts and changes in family and demographic patterns. These social and economic transformations are summarized by Levine as "nascent capitalism" or "proletarianization". One manifestation of capitalism was the growth of market-oriented rural industries, or protoindustrialization. Two villages serve as case studies of this phenomenon. Shepshed, Leicestershire, was "the most intensely industrialized village in Leicestershire (p. 4). Its population quadrupled between 1750 and 1850, even though the last 25 years are described as "industrial involution, the last phase of protoindustrialization" (p. 46). Then rural industry entered into a crisis under the impact of factory competition, but the population continued to grow in spite of declining wages and living standards. Colyton is the village of Devon that had been studied in E.A. Wrigley's pathbreaking articles.[1] Levine returns to Colyton on order to show that Wrigley's findings on the demographic slowdown of the period 1650-1720 can be explained by "deindustrialization", that is, the decline of rural industry. Thus, Colyton is used by Levine as an indirect, negative test case. Wrigley, however, has now published an article that casts a serious doubt on the extent of the decline of rural industry in Colyton.[2]

The rise of rural industry, nonetheless, is not the only manifestation of "nascent capitalism". The second facet of this phenomenon is the proletarianization of agriculture, that is, the decline of the peasantry and the emergence of large commercial farms using a wage labor force. Two villages are used for observation. Bottesford, only 15 miles away from Shepshed, was a "close" parish tightly controlled by its aristocratic landlords, the dukes of Rutland, who could watch it from their hilltop family seat at Belvoir Castle. Their policy was to keep industry out for fear of attracting impoverished migrants. The other village, Terling, Essex, was also tightly controlled by powerful interests acting through the settlement laws and the parochial machinery of poor relief.

In terms of methodology, Levine's study is sophisticated, imaginative, even path-breaking. As was just mentioned above, Levine picked his villages not just because demographic information happened to have been available for them, but because their socio-economic characteristics qualified them as representatives of major types of communities and offered an opportunity to establish correlations between such characteristics and family and demographic patterns. Secondly, Levine's study employs the family reconstitution method,

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[1] E.A. Wrigley, "Family Limitation in Pre-Industrial England," *Economic History Review*, Second Series, Vol. 19, No. 1 (April 1966), pp. 82-110; "Mortality in Pre-Industrial England. The Example of Colyton, Devon over Three Centuries," *Daedalus*, 97 (Spring, 1968), pp. 546-80.

[2] E.A. Wrigley, "The Changing Occupational Structure of Colyton over Two Centuries," *Local Population Studies*, No. 18 (1977), pp. 9-21.

which is the most accurate method of calculating the major demographic rates and tracing their evolution from cohort to cohort.

The family reconstitution method has its pitfalls too, but Levine, in an appendix to his book, shows by matching information from the parish registers with information culled from the 1851 census, that no significant bias could be attributed to his demographic sources and methods.

Thirdly, Levine has been able to compare the demographic characteristics of different social groups defined by occupation, that is, to compare the demography of the framework knitters with that of the farmers, laborers, or other artisans. Furthermore, he has used information from the 1851 census to conduct a separate study of household and family composition, thus combining, or at least juxtaposing in one book demographic with family history, another breakthrough. Finally, from the results of his reconstitution work, that is, from the demographic characteristics for successive cohorts, Levine has attempted to measure the rate of growth of the hypothetical stable populations that would possess such characteristics. In other words, he has tried to synthesize his findings on nuptiality, fertility, and mortality and assess what their combinations would lead up to in terms of population growth. Then, in an effort to impute the relative contributions of these three variables to changes in growth rates, Levine, in a sort of exercise in counterfactual historical demography, has measured what the stable population growth rates might have been under a variety of alternative assumptions. Unfortunately, the calculations are only explained in one specific example (pp.33-76) which is enough for readers to realize that some strong assumptions had to be made in order to make these estimates, but not enough for readers to verify all the steps for the rest of the calculations.

What emerges from this ingenious and innovative demographic analysis is some major themes. Repeatedly the book makes the point that the diversity of demographic and family behavior that is observed over time and across villages is all a logical or rational consequence of social and economic circumstances. There is no room here for attitudes or mentalities. Their role is explicitly denied. Changes in the rural economy, whether the growth of cottage industry (Shepshed, XVIIIth century), its decline (Colyton, XVIIth century), or its involution (Shepshed, XIXth century); or the growth of pasture farming (Bottesford, XVIIIth century) and of dairy farming (Bottsford, XIXth century) completely account, according to the book, for all the changes and variations in demography and the family. In effect, demography and the family adjusted themselves to the modifications in the optimal labor/land ratio these economic changes imposed.

Secondly, the rise of rural industry is relegated in this book to the rank of being one of two major manifestations of "proletarianization". The latter phenomenon is the one that undermined the traditional balance between population and economy in a peasant society. Protoindustrialization could be determining in some cases but in other regions the same disequilibrating role

could just as well be played by the rise of commercial agriculture using a wage labor force. A third theme that emerges from the book is that demographic variables adjusted themselves to economic determinants, but both ultimately were controlled by the autonomous and determining influence of property and power. Levine is neither a demographic nor an economic determinist. He sees economic-demographic interactions shaped by the power of local authorities and property owners over the populace. Although in Colyton, XVIIth century deindustrialization is portrayed as the result of economic factors, it was the rulers of Bottesford who both kept population down and industry out. When in the XIXth century Bottesford suddenly started to grow, Levine attributes it to the rulers temporarily releasing their grip. If both industry and population grew in Shepshed, it was because its property owners allowed in-migration and land fragmentation. In Terling, "the parish officers were empowered to strike their own balance between, on the one hand, a large supply (of labor) and high poor rates and, on the other, a restricted supply and low poor rates". (p. 121) Levine's portrayal of interactions between population and economy thus fairly coincides with that presented by Mills, Dupâquier, and Berkner.[3] A fourth theme emerging from the book is that the ecological balance that was reached in different circumstances was attained through the interaction of nuptiality, fertility, migration, and household composition. Mortality, on the other hand, played a role that was quantitatively minor in reaching the mutual adjustment.

I found one major methodological flaw in this otherwise sophisticated work. Levine's book would have been much more readable and convincing if it had started with the explicit statement of a hypothesis and then proceeded with the presentation of findings, some supporting the hypothesis, others contraverting it. Instead, Levine presents a wide variety of heterogenous facts and findings and insists on explaining everyone of them, no matter how convoluted the *ad hoc* argument might have to be in order to save the phenomenon. Levine never admits to surprise; even the most unexpected trend is presented as the logical, even unavoidable result of some socio-economic factor. Thus Levine found in Shepshed a temporary reversion in the falling trend of the age

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[3] DENNIS R. MILLS, ed., *English Rural Communities. The Impact of a Specialized Economy* (London: Macmillan, 1973); JACQUES DUPÂQUIER, "Réflexions d'un historien sur les problèmes de la répartition géographique du peuplement et ses variations," *Bulletin du Centre d'histoire économique et sociale de la région lyonnaise*, no. 2 (1973); ID., "La France de Louis XIV était-elle surpeuplée? Réflexions d'un historien sur la répartition géographique du peuplement," *Annales de démographie historique 1974* (Paris: Mouton, 1974), pp. 31–43. LUTZ BERKNER, "Family, Social Structure and Rural Industry: A Comparative Study of the Waldviertel and the Pays de Caux in the XVIIIth Century." Unpublished dissertation, Harvard University, 1973; F. MENDELS, "Aux origines de la proto-industrialization," *Bulletin de centre d'histoire économique et sociale de la région lyonnaise*, no. 2 (1978), pp. 1–25.

of marriage in the 1770s. He explains that " of course (this) occurred during a time when framework knitting industry was in a depression with its best foreign market embargoed ". (p. 63) Yet, when the same industry was hit again by the early XIXth century crisis, this time Levine found that the age of marriage did not rise. He explains that " because both man and wife reached their maximum earning capacity at an early age, it was unlikely that a reduction in income would call forth a significant rise in their age at marriage (p. 66). Now in Colyton, when the cottage industry fell into a depression in the XVIIth century, women did marry later, but not men. Why? At this point Levine appeals to a new model of marriage, where female marriage was apparently geared to short-run income prospects, but male marriage to mortality. Following Goran Ohlin, Levine argues that male marriage depended on inheritance, hence on paternal mortality.[4] As mortality rose in Colyton, so male marriages could be kept precocious, in spite of unfavorable income prospects. (p. 72) But why not invoke the same explanation for Shepshed a few pages earlier, since its mortality was also rising over time? In Shepshed, as mentioned above, it was the schedule of maximum earning capacity that seemed determining.

Even though his book as a whole suffers from over-explanation, it remains nevertheless that Levine's contribution to the field is a very important one. There have been many family reconstitution studies before, but none has so well succeeded in presenting the interplay of demographic, social, and economic factors. The best demographic studies have tended to be weak in their economic history, and the best economic-demographic studies have tended to be weak on the side of demographic technique. It is hard to think of any book that has better blended historical demography and economic history.

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A. MACFARLANE, *The Origins of English Individualism: The Family, Property and Social Transition*, New York, Cambridge University Press, 1978, pp. XV, 216

It was with a real sense of excitement that I took up this book. Here was one of the most productive young scholars of English history of the past decade. As he informs us in opening remarks, his efforts have been buttressed by such academic greats as the social anthropologist Jack Goody and that world-renowned research centre, the Cambridge Group for the History of Population and Social Structure. My excitement was heightened as he told us in the Intro-

[4] GORAN OHLIN, "Mortality and Growth in Pre-Industrial Populations," *Population Studies*, Vol. 14 (1961), pp. 190-197.

duction that Englishmen were to regain their proper heritage of distinction from Europeans (pp. 5-6).

In Chapter One, "The Nature of a Peasant Society", Alan Macfarlane sketches briefly the development of the meaning of the non-English peasant to be found in anthropological and historical literature. I was momentarily confused with the statement (p. 18): "This century (XIIIth) and the medieval period in general in England has been dominated by the work of scholars of East European origin, particularly E. A. Kosminsky, Sir Paul Vinogradoff and M. M. Postan. It is clear from their writings that they were consciously comparing medieval England with traditional Russia". However, when I observed that the non-English peasant he was specifying had such qualities as common ownership (p. 20), no cash markets (p. 22), immobility (p. 24) and no women rights (p. 27) my enthusiasm returned. This was not medieval England.

Indeed, with Chapter Two, "When England Ceased to be a Peasant Society: Marx, Weber and the Historians" I could scare forbear to cheer as our brave exponent fingered our whole inherited intellectual superstructure from the Whig Interpretation (exemplified by Macaulay) to Karl Marx as villains of the case. Explosion of this cultural myth is long overdue. The degree by which the myth has permeated and been accepted uncritically by intellectuals is confirmed in a statement of a leading American sociologist, R.A. Nisbet (*The Quest for Community*, p. 79):

"So too have such scholars as von Gierke, Duguit, Maitland, Tawney, the Hammonds, and many others called attention to the contrast that exists between contemporary society, organized increasingly in impersonal terms and resting on the legally separate individual, and an earlier form of society characterized by the primacy of custom and community. On the basis of this perceived contrast unnumerable specific studies of law, education, kinship, town, and religion have rested.

So far as Western society is concerned, the frame of reference for all these contrasts is the transition from mediaeval to modern Europe. It is the social structure of the Middle Ages, real or imagined, that has provided a common point of departure for interpretations as different as those of the socialist Marx and the conservative Maine. It must be our point of departure also".

But, alas, alack and woe! Our brave knight charged the battlements with such unchecked ardour that he not only dislodged the villains but himself fell into the moat on the other side. In current vernacular, what we mean to say is that Alan Macfarlane has attempted to displace one polemic by another. Or, to return to the metaphor again, we poor foot soldiers of the profession of working historians have been left behind. The polemicist may cut down the enemy standard with one sweep of his sword. For the research historian, on the other hand, the careful reconstruction of evidence is literally a battle of inches. Alan Macfarlane has so lost contact with the troops that he fails to realize that the latter have not only been moving along the same line of attack as his own but have

actually been laying the groundwork for his assault. To some extent, Macfarlane may simply be excused his ignorance of such matters since, to judge by his publications, he is not a medievalist. Unfortunately, however, the main fault lies with his methodology. Only a polemicist could pick and choose arguments so carefully with such total disregard for the contributions of scholars of medieval England in this century. Owing to the limits of space, I will only sketch out the author's unfamiliarity with the field, the weaknesses in his proposed methodology and the essentially circular nature of his approach.

Long before the question of individualism could be raised, it was necessary to break the static mould into which historians had cast the medieval economy. Macfarlane does not seem to be aware that such has been the contribution of M. M. Postan as the latter introduced the reality of change along with concomitant questions about population, prices, productivity and so on. In similar fashion, Macfarlane appers to be totally insensitive to the fact that R. H. Hilton has virtually revolutionized our approach to the English rural economy by a score of studies illuminating the possibilities of untapped original sources. However, as I write this, it seems rather ridiculous to be offering an apology for the work of Postan and Hilton. More to the point is Macfarlane's selective approach and I can best illustrate this from his misrepresentation of my own work. The whole purpose of my study, *Tenure and Mobility*, was to elevate the villager to a stature worthy of treatment by the historian — as I, and my former students, have so often expressed the point — to remove the faceless image. In order to do so I employed the obvious dichotomy of the title to signify that the English villein had a *de facto* title to land but, at the same, had considerable opportunities for mobility. To construct my emphasis upon tenure into a system, in total disassociation from mobility as Macfarlane does in several chapters, is a deliberate fabrication. My former students and I have in fact been consistently criticized for not imprisoning the English villager anew into one demographic or social system or another. However, I have persisted in my naive humanist emphasis to the extent for example, of giving the title " Individuals " to the last chapter of the volume *Warboys!*

Alan Macfarlane pretends to have a most unusual gift among historians, that is, the gift of prophecy. On p.62 we find the statement: " the weight of proof for the thesis we will be pursuing will have to come from future work ". Certainly, the skimpy data he puts forward in chapters Three and Four place the burden of proof on the future. It is also less than helpful that he has referred so frequently to unpublished work or comments by the Cambridge Group, since that body has already suffered from its image as an ' in group '. More especially has Macfarlane exploited the confidences of his colleague, Richard Smith. Again and again, one finds carefully worded statements of the latter cited as pivotal evidence. For example, on p. 149, after a sentence beginning " All the evidence indicates... " there follows " Richard Smith has found the presence of servants " on a scale which, if it becomes possible to estimate it

firmly, might well be similar to that which characterized England two or three hundred years later ". It is a pity to have a promising young scholar's work presented in such a fashion that, by the end of the volume, one begins to expect references to " poor Richard's almanack ".

Alan Macfarlane is able to employ early modern evidence and to pick and choose among the findings of medieval scholars because he fails to give weight to the rôle of custom. Inevitably, medieval man was as much an individual as modern if the historians of common law are taken as the main authority. None of us has done much to assist in the study of medieval English customs. The Past and Present publication, (1976), *Family and Inheritance*, may be taken as the first concerted attack on the problem of custom. But, even if Macfarlane's approach should prove to be useful, what is he proving? The concept of individualism is part of that same ideological philosophy of the XIXth century that Macfarlane throws out early in the volume! What he is attempting to prove is a non-question; it is an obsolete intellectual formulation. One fondly hopes that medieval English villagers were individuals but what individualism could mean in pre-industrial society is a non-question to this reader.

This is a bold and courageous piece of iconoclasm and should indeed be read by those requiring awakening from dogmatic slumbers of the XIXth century. So long, of course, as the reader keeps in mind that individualism, too, is a XIXth century dogma!

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D.E. Martin - D. Rubinstein (eds.), *Ideology and the Labour Movement: Essays Presented to John Saville*, London, Croom Helm, and Totowa, Rowman & Littlefield, New Jersey, 1979, 276 pp.

The essays collected here in homage to John Saville and his pioneering work in British economic and labor history argue the case for what might be called British exceptionalism. Unlike continental European workers' movements but more like those in the United States, the British " movements " described and analyzed in this collection of nine substantive essays (plus an editors' introduction, a biographical sketch of Saville, and a bibliography of his writings) were fragmented and limited in goals. Ordinary workers tended to dwell in the here and now, focused their energies on bread and beef (seldom even roses), and acted suspiciously toward middle-class reformers, intellectuals, and socialist politicians. Trade unions, in turn, stood halfway between the narrow wants and aspirations of the working class and the more ambitious objectives of Socialist and Labour parties. Finally, the movement's political expression, which came to be the British Labour Party, more so than the Inde-

pendent Labour Party, invariably held back from challenging the most basic elements of the capitalist and parliamentary systems. Hence, in the word coined by John Saville in one of his own earlier essays, all sectors of the British workers' movement, in the end, practised " labourism " rather than socialism.

All these themes are made quite explicit in the editors' introduction, where Martin and Rubinstein explain why it was natural for British " working-class people to work within the political system rather than outside it " (p. 10), why they followed Keir Hardie in believing that " the ballot is much more effective than the barricade ". (p. 11) The editors are also quite correct in stressing that all the contributors to their *festschrift* expose " the continuous growth of working-class power within a strictly limited ideological framework ". (p. 12)

Actually, the essays form a rather mixed bag. Ralph Miliband's biographical sketch provides an insightful description of John Saville's personal intellectual and political journey to the left as well as a dissection of the travails of British communism since the 1930s. Victor Kiernan analyses how the more literate members of nineteenth-century British society perceived working people and also how *few* people developed a socialist consciousness (p. 59). In a variation on his now famous essay on the language of class, Asa Briggs probes " The Language of ' Mass ' and ' Masses ' in Nineteenth-Century England ". Alan J. Lee explains the proclivity to vote Tory among at least one-third of British workers between 1880-1918, largely in terms of traditionalism, religion, and economism. Iris Minor uses the matrimonial law reform movement of the same era to disclose the gulf between middle-class reformers and working-class women. Through a microscopic examination of members of the Parliamentary Labour Party in the 1906 session, David Martin concludes that Labour MPs exemplified " labourism " and trade-union consciousness, and that " the Labour Party was neither a torrent nor a battering ram; it was closer to being a respectful but compelling petition " (p. 143). In still other essays, Frank Matthews dissects the intellectual origins of guild socialism and its limitations, while Sidney Pollard exposes British Labour's failure of will on the question of bank nationalization. And Pollard, too, echoes John Saville's indictment of Labourites who " proceed to administer a capitalist society, which they have previously denounced, in as efficient way as possible ". (p. 185). Dame Margaret Cole follows with a long, largely personal recollection of why trade unionism and the Labour Party scarcely changed during the crisis of the inter-war years. Finally, David Rubinstein, in the closing essay which examines the post-World War II Labour left, yet again underscores why the Labour government of 1945-50 produced the welfare state, not a socialist society.

Despite the essays' exposure, individually and collectively, of British labour's chequered and restricted history, the editors, and apparently the bulk of the essayists also, remain optimists and believers. Committed to a Marxist approach to history, society, and politics, they believe that the last two hundred years of British history has been the story " of *reaction* by other social classes to mo-

vements of the working class". And they remain convinced "that it is the working class and its industrial and political institutions which have been the motive force of modern history" (p. 13)

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A. E. MUSSON, *The Growth of British Industry*, New York, Holmes and Meier, 1978, pp. 396.

This volume is a highly useful survey of British industrial activity from 1500 to 1939. Most modern discussions of British industry focus on particular epochs and often examine only the performance of a few leading or lagging sectors. All sectors of British industry are covered in this volume and the gain from this approach is substantial. First, the forces shaping the appearance of technical innovation are much more sharply defined when the performance of technically moribund sectors is being simultaneously analysed. Second, this approach clearly illustrates an essential feature of modern economic growth, the unevenness of technical progress across sectors and time. Based on his own monographic research Musson excells when he analyses the interrelatedness of technical progress in coal, iron, steam and mechanical engineering during the Industrial Revolution. He also does valuable service in exploring the technically quiescent consumer goods industries of the same period. The growth of income and population during this era augmented the demand for final consumer goods and this translated into a substantial increase in the employment of handicraft workers. The numbers tell us that most British workers in 1850 at the end of the Industrial Revolution era did not work with steam-powered machinery but only a thorough survey of British industry gives this point substance.

Musson divides the volume into four sections covering the periods, 1500-1700, 1700-1850, 1850-1914 and 1914-1939. In each section discussion covers the principal hypotheses of industrial change, general developments in industrial organization and, of course, description and analysis of any industrial sector employing more than a few thousand hands. Economic theory informs Musson's organization and text but there is a minimum of what might be called microeconomic fine tuning. The text is thus accessible to general historians without sacrifice to the needs of undergraduate specialists or advanced scholars interested in a thoughtful presentation of the state of British industrial history. Still, there are some issues raised by the technical literature which might not have been ignored. For example, Musson tends to treat the appearance of major process innovations as synonymous with large productivity discontinuities and substantial additions to social product. First, in many studies it has been found that a considerable portion of productivity increase is related to within-

process innovation, partly based on learning-by-doing. Second, Fishlow, Fogel, and Hawke have certainly raised the possibility that big process innovations may not augment social product to the degree suggested by the specialization in a particular product of production method which follows the innovation's diffusion.

On the whole Musson takes an optimistic view of the performance of British industry. The context for this optimism is sometimes uncertain, however. There is little comparison of British industrial output and productivity growth rates with other nations? In the 1870-1913 period when these growth rates clearly slowed by historical standards Musson's optimism focuses on the large, absolute jumps in output. Clearly, British industry survived and contributed to the nation's record of economic growth from 1500 to 1939. The issue is the conditions and characteristics of that survival. International comparisons, for example, suggest that Britain always travelled somewhat slowly and during certain extended periods, quite slowly. Perhaps there were peculiar British constraints, natural or social, which were important, thus justifiyng Musson's optimistic view. My dissent is not that Musson ignores evidence; indeed, someone with a less sanguine opinion of British industrial performance could easily build a good case from Musson's materials. However, to make his case, Musson needs a bit more analysis and argument.

Musson offers a number of interesting and often compelling suggestions relative to the existing literature First, he gives more credit to pre-1700 and Continental influences on the technical change of the industrial Revolution than previous writers. Second, with some well-placed indignation, he argues that British mechanical engineering has been slighted in the early history of standardized, interchangeable parts and mass production methods, usually termed, the 'American' system. Unfortunately, Musson does not pursue the point although I suspect his answer would be that the U. S. had a large domestic market for mass production methods after the the mid-XIXth century.

Confronted by the falling growth rates of aggregate industrial productivity during the 1850-1914 period Musson takes what might be described as a structural view. The XIXth century staples like textiles, iron coal and steam engine machinery achieved some technical progress and, according to Musson, fought the U.S. and Germany well in third markets. However, since it is generally true that productivity advance is most rapid in the initial decades of a major process innovation, Musson argues that one should not expect major contributions to total industrial productivity indices from these still quite profitable and large sectors. Where were the new industries with new processes and high profits and wages to attract capital and labor thus lifting total productivity performance? Absence of U. K. tariffs and poor scientific and technical schools absorb most of Musson's explanation, particularly in fields affected by chemical and electrical processes. Poor entrepreneurship might have had some small effect on the period's performance but Musson finds no widespread failure of leadership.

This view is not original to Musson but his survey of all industrial sectors and many firm histories gives his judgement considerable weight.

In sum, if somewhat extravagant in tone on occasion, the intelligence and lucidity of the text should commend this voulme to a wide readership.

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G. PESCOSOLIDO, *Terra e Nobiltà. I Borghese*. Rome, Jouvence, 1979, pp. 372.

The careers and lives of the great Roman aristocratic families have attracted a wide and varied range of studies which have demonstrated their economic strength, their social position and behaviour, and their role in the social and economic life of the capital of Catholicism. Although the present study also makes an important contribution to the history of one of the leading princely families, it is based on a very different type of approach. Using the exceptionally valuable archival sources provided by the *Archivio Borghese*, the *Archivio del Capitolo di S. Pietro in Vaticano*, and the *Archivio di Stato di Roma*, the author sets out to reconstruct the economic basis of the Borghese family, and by demonstrating the links between the vast landed patrimony which they possessed and the agrarian economy which dominated the Lazio region he has succeeded in pin-pointing structures and processes of development which are relevant to a much wider range of social and economic problems.

The study pays much more attention to the analysis of the ways in which the family's vast possessions in Lazio evolved in the XVI th and XVIIth centuries than to their political and social fortunes. The discussion of the family's agrarian rents in the XIXth century is also of particular importance in the context of the debate on the development of Italy's agricultural production in the first twenty years after Unification, and the conclusions show that the progress of the agricultural economy in Lazio in the first eighty years of the century was much more positive than has often been assumed.

The study consists of six chapters. In the first, the author describes the formation of the family patrimony from its origins to the point of its greatest extension, locating the formation of its essential nucleus in the early XVIIth century. The main period of the expansion of the patrimony was the pontificate of Paolo V Borghese, and the acquisitions made between 1607 and 1637 definitively consolidated the landed wealth of the Borghese family. By the end of the XVIIIth century the Borghese estates were the largest in Lazio, at the beginning of the XIXth century they covered well over 52,000 hectares and continued to grow, largely as the result of advantageous marriage alliances, until the 1880s, when their sheer size became an anachronism in the context

of the economic and social development of the new Kingdom of Italy. From then on a new tendency of seeking investments in sectors other than land began to take over, and this was paralleled by a tendency for the amount of land concentrated in the ownership of individual members of the family to decline.

Before moving on to examine the ways in which these vast estates were managed, the author devotes the second chapter of the book to a detailed description of the composition of the patrimony. The Borghese lands were both allodial and feudal, and were scattered throughout the Agro Romano. They provided an accurate reflection of the general conditions of agriculture in the Lazio region, determined by the almost total lack of inhabitants, the absence of irrigation, and the consequent sub-division of the great estates and the predominance of extensive forms of cultivation. But the author shows that there were significant differences between the economic structures of the private and the feudal estates, and while the level of production on the latter tended to be extremely low the continuation of tithes, quit-rents and common rights into the XIXth century throughout Lazio and the Roman countryside acted as a major obstacle to development.

Making full allowance for all the problems involved in reaching any definitive figure, Pescosolido attempts to calculate the quantitative value of the common rights exercised on the Borghese lands. Given the vast range of different types of impositions and obligations in existence this is no easy task, but Pescosolido shows that common rights were exercised on the staggeringly high proportion of 40% of the Borghese estates. That this was the position in the 1820s' after the abolition of feudalism, shows clearly that the process of simplifying traditional commmon rights had still not progressed far in Lazio at that time.

The third chapter is devoted to an analysis of the administration of the estates and the types of accounting employed. This reveals that the greater part of the family's landed rents came from large-scale leases. A detailed examination of the correspondence between the owners and their tenants in the XVIIIth and XIXth centuries reveal a number of changes which, although not of a radical nature, are significant, the author argues, for understanding the ways in which the Borghese responded to various shifts and developments in the social and economic life of the region. Comparing the situation in 1763, when 93% of the 50,276 hectares owned by the Borghese princes in Lazio were under lease, and that in 1805 when the percentage of land leased out had fallen to 74%, Pescosolido shows that on the privately owned estates there had been an enormous increase in the concentration of land in the hands of a small number of lessor (the total number of lease-contracting enterprises had fallen from 30 in 1763 to only 19), whereas there was a growing tendency for the feudal estates to be farmed directly on the family's behalf. But the contrast was more apparent than real, and rather than indicating any real development in the system of managing the feudal possessions it should be seen as the momentary product of

particular political and administrative circumstances which were soon to change. The final years of the XVIIIth century and the early years of the XIXth were times of particular crisis in the Papal States, and it was almost certainly in an effort to overcome immediate difficulties that the Borghese family decided to adopt a more intensive and direct form of management of those sections of their property where the exaction of revenues was the most complex.

There was a further inversion of these tendencies in the 1860s and 1870s, this time in the opposite direction and leading to a much greater fragmentation of management than had been the case in 1763. This new reversal of the tendency for leases to become increasingly concentrated affected both the vast leases which had brought a number of estates under the control of a single lessor, and also the estates themselves which now tended to be divided amongst a number of smaller lease-holders. The division of the estates in this way and the introduction of a greater variety of forms of farming was a result, the author argues, both of the general advances taking place in the agricultural structure of the region and also of the enormous demographic expansion which had begun to affect the Papal States as well as the rest of the peninsula.

Going beyond the specific comparison, Pescosolido concludes that between 1763 and 1810 the increase in profits from land throughout the Agro Romano and the surrounding areas had been very modest (between 10%—20%) in relation to the contemporary increases in the market prices of agricultural products, especially those for cereals which in response to growing demand and commerce had risen throughout Europe from the 1760s onwards and reached extremely high levels by the end of the century and throughout the Napoleonic Wars. Until the Restoration after 1815, when they began to be identified as the principal cause of the backwardness and underdevelopment of the region, it had been impossible for the owners of the vast latifundist estates, who were costantly challenged by a new agrarian bourgeoisie eager to gain access to the land themselves, to obtain for themselves a larger share of agricultural profits in the rents they received . Analysing the ways in which the value-added in agriculture was distributed among the three factors of production, the author concludes that ' it seems certain that in these difficult and uncertain years the lion's share went to the suppliers of capital.' The greatest profits went to those bogey-men of contemporary publicists, the *mercanti di campagna* — the capitalist lessors of the noble estates. The nature of their participation in the political events of the period provides the clearest evidence of their advantageous economic position in these years.

Throughout the early part of the XIXth century, landed incomes grew more slowly and remained divorced from the market price of agricultural products, being still more closely related to levels of production and the class relations which determined the division of the economic surplus generated within agriculture itself. But the 1840s saw the beginnings of a new phase of expansion. From the early 1840s land rents began to rise markedly and conti-

nuously, and followed an unbroken upward trend until the 1880s. Between 1838 and 1858 the unit gross lease value on the Borghese estates rose from 16.43 to 18.43 lire per hectare, taking the overall index to 126 — this meant an increase of 20 points in twenty years, compared with only 6-7 points over the previous twenty-eight years. In 1880 the unit lease value reached 32. 77 lire per hectare, and the index had shot up by a further 72 points, but the most rapid advance had been made between 1858 and 1866, after which there had been a static period until the index began to move up again at the end of the 1870s to reach 213.

This increase in income cannot be explained solely in terms of the rising market value of agricultural products, but was largely the result of the introduction of more varied types of farming, and statistical evidence points to the rapid expansion in the production of cereals, olives and livestock throughout the whole region. The Borghese lease contracts also reveal that in the second half of the XIXth century the area under cultivation was extended and that farming was becoming more intensive in both quantitative and technical terms.

By the beginning of the 1880s, therefore, Lazio's agricultural economy had experienced for some thirty years a process of change and development, and even though this had not as yet fully obliterated all the more traditional structures and levels of production, it had paved the way for a more radical transformation which would enable the region to break out definitively from the traditional structure of production and social relations in the countryside. In the final chapter the author explores the investment policies, the expenditure and the growing debts of the Borghese family, and reveals some of the crucial factors which were ultimately to plunge the patrimony into crisis. In addition both to a very costly life style and to the legislation introduced by the new state which damaged the interests of the great landowners much more directly than historians have been prepared to acknowledge, there were the consequences of the family's land purchases. Further acquisitions of land had not led to any negative consequences in the 1840s, since the parallel rise in land rents produced increasing rent income, but by the late 1880s they were proving disastrous as rents fell back in the face of growing international competition, especially from American grain. Within a very short period the family's budget swung heavily from surplus to deficit, and although Prince Paolo tried to find a solution by way of investment in agricultural improvements and increasingly by investments in non-agricultural sectors, between 1887 and 1929 virtually all the family's rural and urban properties had to be mortgaged. The slow but steady decline of the family's economic fortunes became irreversible in the economic and social circumstances of the new state, and it was a fate shared by the noble landowners of Lazio as a whole, despite the fact that they had not been slow to see that times were changing.

Pescosolido's book has an importance which takes it well beyond the fortunes of a single family. It is a major contribution to the agrarian and social history

not just of Italy but of Europe as a whole, since it reveals clearly the slow, painful and often contradictory nature of the patterns of agricultural development in Italy when compared with those of other countries.

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N. ROSENBERG - W.G. VINCENTI, *The Britannia Bridge: The Generation and Diffusion of Technological Knowledge*, Cambridge, Mass. and London: The MIT Press, 1978.

The authors, respectively an economic historian and an engineer, both members of the interdisciplinary Program on Values, Technology, and Society at Stanford University give us, in this small book, the fruit of their collaborative inquiry into a single technological event which they see as paradigmatic of the process of (as the subtitle puts it)the generation and diffusion of technological knowledge in the course of industrialization. The book is the tenth in a series on technology and culture published jointly by the Society for the History of Technology and the MIT Press.

The Britannia Bridge was built in the late 1840's (it was completed in 1850) as part of the London-to-Dublin train connection. On the east side of St. George's Channel in the Irish Sea, the railroad terminated at the port city of Holyhead in Anglesey County, Wales, at about the latitude of Dublin and Liverpool. This route necessitated spanning a distance of approximately 450 feet over the Menai Straits (separating Anglesey and Caernarvon Counties) by a bridge strong enough to support its own weight plus that of a loaded train in motion. The site chosen for the crossing was over Britannia Rock in the middle of the straits so that a centre support could be placed at that point (hence the name of the bridge).

A combination of the great distance to be spanned, the necessary rigidity and load-carrying capacity, and the insistence of the British Admiralty that the Menai Straits remain navigable by the Navy's sailing vessels ruled out recourse to either of the then conventional methods of building long bridges: the suspension method would not have provided the necessary rigidity and the use of cast-iron arches would not have allowed the necessary headroom.

The result was a major and at the time unprecedented technological challenge. New materials had to be worked and tested, unusual configurations considered, novel construction techniques devised; new knowledge had literally to be created for the occasion, for many of the problems encountered were as yet not provided for in contemporary engineering theory.

> It was under the circumstances of these unusual constraints that Stephenson [Robert Stephenson, engineer in charge of building the Britannia Bridge, and son of the famed railroad engineer, George Stephenson] conceived of a tubular bridge, to be constructed out of riveted wrought-iron plates, and large enough to allow

> trains to pass through its interior... Such a structure, however, was totally unlike anything that had been previously attempted. Wrought iron had never before been used on so large a scale. The novelty of both the materials and the design was so great that there was no reservoir of reliable knowledge or experience upon which to draw in determining feasibility and, above all, safety (pp. 6–7).

The first chapter of this book ("Generation of Knowledge") describes how Robert Stephenson and his principal associate in the venture, William Fairbairn (a prominent ship builder, iron fabricator, and engineer of the time), overcame the challenges thus posed and opened the Britannia Railway Bridge to traffic in March and October 1850 (for each of the two tubes respectively). It is a delightful tale, clearly written, with enough technical detail to induce an appreciation of the problems encountered and the solutions forged, but not so much as to daunt the inexpert. There are photographs of the bridge (from the 1860's and 1870's) and there are diagrams, some contemporary and some new, that, given reasonable attention, help make intelligible how the engineering principles and problems entailed by the project led eventually to development of a novel cellular design for the top flange of the rectangular tube that provided the needed resistance against buckling, thus solving the problem.

The hitting upon the cellular design in effect marked the invention of the thin-walled, hollow, ribbed, load-bearing beam, and its usefulness far outstripped that of the bridge for which it was designed. The tubular bridge

> was soon displaced for long-span railway bridges by far more satisfactory types constructed along different principles... Nevertheless the brief experience with tubular bridges was of far-reaching importance because an extremely useful learning experience took place in the process of design and in dealing with the numerous technical problems that such bridges posed (p. 67).

The second chapter of the book ("Diffusion of Knowledge") indicates the extent and importance of that learning experience to the industrialization of the west in the second half of the nineteenth century. The engineering knowledge gained in the Britannia Bridge project found direct applicability in the construction, not only of more long bridges (including Stephenson's Victoria Bridge over the St. Lawrence River at Montreal), but also of buildings (allowing for larger open spaces and more light than hitherto), cranes (with curved projecting arms that allowed great weight to be lifted the full height of the crane without coming into contact with the then traditional straight-line jib), machine tools (now provided with hollow box frames that used less material without sacrifice of strength and rigidity, as in "the familiar example of the goose-quill, in which the maximum of strength and stiffness is combined with the minimum consumption of material" «p. 94, n. 131»), and iron ships ("an iron ship could be regarded for all practical purposes as

an oceangoing tubular beam" (p. 52). More generally, the hollow beams designed for the Britannia Bridge "were the forerunners of the box girders of welded steel or concrete widely used today" (p. 47) and, "«as» a thin-walled structure in which the walls act as primary load-resisting elements..., the Britannia Bridge was, in fact, an early example of the kind of structure essential to the modern-day airplane and submarine" (p. 84, n. 20). (The notes in the back of the book contain a wealth of information and explanation that illuminate the main story and its history.)

The authors draw a number of morals from their tale. Arguing that "technological change can most fruitfully be examined as a problem-solving activity," thus eschewing a tendency to obscure its nature by treating it "in vague, general, or purely abstract terms" (p. 2), they note that technological changes often come as a response, not only to technical problems, but to socially imposed constraints as well, as in the Admiralty's insistence that the navigability of the Menai Straits not be compromised. The other side of this same coin, according to the authors, shows the social benefits that derive from new technical knowledge:

> These benefits are, conceptually, measured by how much sooner the useful knowledge was made available than would have been the case had the bridge never been built. We do not presume to offer an estimate of the economic value of these highly diffuse benefits, but a compelling case could readily be made that the benefits attributable to the earlier availability and the more rapid diffusion of the new engineering knowledge were large indeed (pp. 61-62).

If such a compelling case were indeed to be made, not only for the Britannia Bridge, but also for a significant additional number of historical instances, knowledge of the interaction of technological and social processes could be greatly enriched.

The authors note, also (all this in the last of their three chapters, "Conclusion"), the interdependence among different technologies that is illustrated by the history of the Britannia Bridge. The bridge was destroyed by fire in 1970 and was replaced by an arch bridge, the same kind of arch bridge that had been earlier proscribed by the Admiralty. The Menai Straits remain navigable, thanks to new propulsion technologies that eliminate tall masts as an inhibiting factor. One would think that deliberate and systematic exploration of such technological interdependence might enhance techniques of what currently goes under the name of technology assessment.

The authors question the legitimacy of the "widespread tendency to regard technology as involving essentially the application of knowledge derived from science" (p. 71). Much of the new knowledge required to bridge the Menai Straits was empirical knowledge hammered out on the spot by trial-and-error experimental methods; theoretical knowledge that could have guided the engineering effort was insufficient for many purposes.

> For example, Hodgkinson's compression tests of plates bypassed a lack of theoretical knowledge in order to establish the buckling properties of thin plates, a problem that was first studied theoretically by Bryan only forty years later (p. 72).

But if there can be technology without science, economics is an ever-present factor in technological change. In the case of the Britannia Bridge economy "meant mainly that the cross-sectional shape of the tube and the disposition of material should be chosen so as to keep the total amount of material — and hence the weight and cost of the tube — as low as possible" (p. 13). And more generally, as noted above, economics provides a principal measure of the social benefits of technological change.

The authors address explicitly an important implication of their study for the writing of technological history. Most such writing, they say, assumes that the significant technologies of the past (and, therefore, the only ones worth writing about) are those which evolved into the significant and visible technologies of today. But, "«if we examine technological history by focusing not on the major long-term success stories but upon the historical incidents that resulted in significant accretions to the stock of human knowledge, our perspective changes drastically" (p. 69). Thus, tubular railway bridges did not enjoy a long history, but the knowledge generated in their construction influenced and enriched much subsequent technological development, and such knowledge should command the attention of historians at least as much as surviving hardware does. The point may have a further implication for those who study the interactions of technological change and social change. The relationship is rarely direct, and the task of inquiry is to identify and explain the role of mediating factors in the interaction. The cumulation of technological knowledge irrespective of the viability of particular technologies might well prove a significant factor.

The book does raise some questions and puzzlements, though they are minor blemishes on an otherwise fine miniature canvas (74 pages for the main text). Looking at a map, one wonders why Caernarvon in Caernarvon County was not chosen over Holyhead in Anglesey as the port city for the London-to Dublin train connection; no bridge over the Menai Straits would then seem to have been necessary. Was the possibility raised in Parliament during the authorization debates?

Puzzling, also, is the authors' attempt to distinguish between theoretical knowledge and "engineering knowledge". We are told that "«design engineers... often select a particular configuration because it lends itself readily to thought and analysis" (p. 10) and that "by visualizing the upper flange (of the rectangular tube) as a set of columns, Stephenson, Fairbairn, and Hodgkinson were now on conceptually familiar ground" (p. 25). Yet, later, we are cautioned that "engineering knowledge takes various forms, and we must continually remind ourselves that knowledge does not necessarily require or

even imply theory " (p. 60). If this point is more than another instance of a defensiveness of engineer confronting scientist that occasionally creeps out of these pages (cf. the full paragraph on p. 72), then, surely, the role of theory in knowledge is a subject complex enough to warrant more than the passing remarks given here.

Unnecessary confusion could have been avoided at a couple of points by clearer formulation. Thus, figures 6c and 6d illustrate, respectively, a " «strut in pure compression " and a " «bar in pure tension ", and we are told, on page 21, that " «a beam subject to bending... can fail... either through failure of the bottom flange as a bar in tension or through failure of the top flange as a strut in compression ". The point is confused, however, when earlier (p. 19), " bar " and " strut " appear to be used synonymously: " The top flange is thus in a sense equivalent to an isolated bar equivalent to an isolated bar (or strut) in pure compression... " Elucidation of the precise meanings of the terms used would have helped.

Similarly, a reader might be somewhat confused, at least initially, by reading that cast iron " is very strong in compression but weak in tension " whereas wrought iron " is strong in tension as well as in compression " (pp. 81-82, n.5) and then reading Stephenson to the effect that " the power of wrought iron to resist compression was much less that its power to resist tension, being exactly the reverse of that which holds with cast iron... " (p. 87, n. 47). If there is no real inconsistency here, more careful exposition could have avoided an apparent one.

There is, finally, occasional recourse to phrases indicative of what J.H. Randall, Jr. once dubbed a " must-have-been " theory of history: I.K. Brunel's thinking about thin-walled construction " must have been influenced ... " (p. 48); the design of the ship Great Eastern " doubtless also incorporated... " (p. 55); both Brunel and Russell " would certainly have been well acquainted with... " (p. 56); the knowledge generated during the bridge project " was doubtless widely employed... " (p. 58). Surely historians above all must be chary of the danger of assuming that events " must have been " patterned in a certain way when there is insufficient evidence to show how they were in fact patterned.

When all is said, however, the story of the Britannia Bridge remains a nice story well told.

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