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## REVIEWS OF BOOKS

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R. CHURCH, *The History of the British Coal Industry, Volume 3, 1830-1913: Victorian Pre-Eminence*; Oxford, Clarendon Press, 1986, pp. XX + 831;

B.R. MITCHELL, *Economic Development of the British Coal Industry 1800-1914*; Cambridge, Cambridge University Press, 1984, pp. XV + 381.

Pity the poor reviewer! 1212 pages on the British coal industry in the nineteenth century: it will be a devoted scholar indeed who will pay out the £85 asked for them, though the specialists may want Church on their shelves if only for reference. Unhappily it is not in the same league as the late Michael Flinn's book in the same series, dealing with the eighteenth century coal industry with its elegant prose and deep unselfconscious scholarship. Mitchell's is the more immediately enjoyable book, largely because he does not have anything like the same volume of pure information to offer, though one becomes uneasy at the extent to which Church's more intensive work has shown that important statistical series used by Mitchell need to be modified. Long passages of Church are almost unreadable: fact is piled on fact and no felicity of expression, never mind any element of levity, is allowed to dispel the solemnity of the academic occasion. Worse still, the views of scholar after scholar are paraded out to make sure the reader knows they have not been missed. All the analyses dreamed up by the "new" economic historians are applied, whether or not they are really relevant or the material adequate for the strains imposed upon it — as it rarely is. Total factor productivity is measured in all its glory, though virtually no use is made of it — possibly fortunately. Church does seem more interested in showing that he knows about certain techniques than in what one might learn from them. For instance, though he spends a great deal of time in elaborate calculations of the capital/output ratio, he does not ask the same sort of sharp questions about the results that we get from Mitchell — above all how did they compare with the rest of the economy? In fact Mitchell shows that although the ratio rose quite sharply between 1880 and 1913, the figure for the latter year, 1:1, is very low compared with Feinstein's calculation for the whole economy of 3.8:1. Also from 1854 to 1913 in coal mining it had risen by 54% compared with an economy-wide fall of 24%.

Even so, for all its failings, Church has produced a very impressive book that all scholars will use with profit and with gratitude. Mitchell's book is clearer and livelier but is much more limited in scope. One has to say frankly that it simply does not work to pull an old Ph.D off the shelf and try to bring it up to date with additions and amendments here and there. Cairncross and Pollard both tried to do it and both failed. The contrast between Mitchell and Church is most marked when they tackle the crucial question of productivity. Mitchell couches his argument in general terms: falling output per man year and per manshift in most coalfields from the mid 1880s is attributed to shorter hours worked and to the ageing of mines to which, unlike earlier years of the century, technology did not respond quickly enough. In part, at least, this was due to the inelasticity of demand for coal so that prices could be raised in line with rising costs. It also arose, so Mitchell argues, from the relative elasticity of supply of labour, for though miners' wages were rising relative to other groups, they were still low enough to make new techniques such as mechanical cutting and face conveyors unviable in the majority of circumstances. So physical productivity fell but revenue productivity continued to rise — from £ 23 per man over 1880/98 to £ 117 over 1899/1913. Church is considerably more sophisticated and far more persuasive. Recalculation of the OMY figures gives a much lower figure for the third quarter of the nineteenth century than those hitherto accepted and a less precipitate fall thereafter. In any case, although OMY and OMS both fell from the 1880s onwards as a result mainly of shorter hours and possible more absenteeism as wages rose, output per man hour did not fall nearly so sharply. That inevitably casts doubt on any arguments related to age of pits, shortcomings of new technology or sheer recalcitrance of labour. Real working costs rose by approximately a half from 1890 to 1913 due to a fall in hours worked and rising wages but technology and improved methods to some degree checked the trend. Even so, both writers agree that there were very considerable limitations to the profitability of installing new machine mining methods before 1914. The technology was not very effective in thin and hard seams and frequently uncompetitive in thick seams where the hewing costs were low. In an excellent chapter on technology and working conditions, Church suggests that three quarters of the coal mined in 1913 was cut in conditions unsuitable for mechanisation for geological reasons and he also suggests that the unavailability of electricity in the mines, frequently because of the small size of the individual pit, hindered technological progress. Both Church and Mitchell agree that very little criticism should be directed towards the mine operators.

If Church wins on productivity, Mitchell is more impressive in analysing the course of wage movements. As regards the supply of labour, Church challenges Cairncross's estimate that from 1841 to 1911 only one sixth of the rise in colliery population came from net migration; he raises the proportion to about one third, though it still remains true that the natural increase of population in mining areas was very high. Interestingly enough Church also shows that although mining was a dangerous and unhealthy trade, at the turn of the century

mortality from all causes was below the national average and only one third of that of general labourers. Of course the age structure was heavily skewed towards the 20-44 age group and possibly miners were a "tough group". Church excels in a detailed study of the supply of capital, pointing out that banks rarely offered anything in the form of long-term loans and that the long period of gestation meant that during the initial phases profits were simply not there for ploughing back. Consequently private wealth or the supply of funds from other coal/iron companies were particularly important. Both agree that over the long term the return to capital was lower than that for industry in general. Why, therefore, was investment seemingly so attractive? Partly, Mitchell argues because those well informed, such as mining engineers, had a reasonable chance of making above average gains and partly because of the ever present speculative element which was fed by the very high profits available in boom periods.

Church, of course, writes on an altogether broader canvas. On the one hand, there is a detailed, careful study of hours worked, on the other a graphic account of child labour. The description of the social provisions made by the owners — schools, libraries, housing, churches, brass bands is accompanied by the very persuasive argument that all this failed to make miners more submissive if that was the intention — and it appears that in most cases it was. He reminds us, in this connection, that the number of villages and towns where mining was the only, or even the main, form of activity was not all that large — not, I think a very telling point because wherever they were, the miners have always considered themselves and been considered by others as a highly independent group. Professor Church obviously does not come from West Yorkshire; my wife does! Both give a lot of space to wage bargaining but both make it a very dull business, all too reminiscent of those vast, boring tomes on the history of the mining unions in different areas. Church does make the important point that the whole environment after the mid 80s was conducive to increases in real wages, given the long secular increase of demand and of prices, with severe labour shortages in the upswings when profits were potentially at their highest and a thriving trade union organisation able to capitalise on these potentialities.

Where both fall short is that they make no attempt to place the industry in its international setting. Church concludes his book by arguing in the manner which became fashionable two decades ago, that the industry did not let the country down but did as well as could be expected in all the circumstances. But it is not clear how one can argue this in a purely national setting. It has been suggested more recently that the traditional British industries did remarkably well to 1914 to remain competitive in the face of low wage level competition from others — in this case in Europe. How much lower were wage levels on the Continent; and how much lower was productivity and what were the trends? Were attitudes to mechanisation more forward looking? These questions have to be answered before a valid judgement of performance can be made.

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P. GREGORY, *Russian National Income, 1885-1913*, Cambridge, Cambridge University Press, 1982, Pp. XII-359.

The prospective reader should not be misled into thinking that the scope of this book is limited to its title. Rather, Professor Gregory has provided us with a detailed exegesis of the methodology of constructing retrospective national income accounts, the pitfalls which await those who interpret the data, a detailed description of the sources and methods used to derive the constituent series making up the accounts, and finally some interpretation of what conclusions may be reached from his work. This is a fine piece of scholarship.

In assessing the value of this book a natural starting place is why should this topic receive so much attention? I believe there are three reasons for the extensive work done in this area. First, the Russian Empire became the first «laboratory» experiment to reveal the impact of dictatorial socialism upon the economic development of a country. For better or worse, the experience of the Russian Empire under the Czars and then under the Soviet regime was taken as the basis for a comparison of capitalism versus communism. In order to evaluate this change as a before/after experiment, it is necessary to ascertain just what the «before» happened to be.

Second, in order to understand fully the factors which led to the unrest in 1905 and the revolution in 1917 it foreshadowed, one must assess the economic progress and development of the Russian Empire under the Romanoffs over the half century before the revolution, and especially the period 1885 - 1913.

Third, due to its relative backwardness, the Russian Empire is a useful case study in development. The lateness of its development leads us to ask why this area was the laggard. In addition, because economic activity in the Russian Empire even before the revolution was likely more centrally controlled than was the case at a comparable stage in the development of most Western societies, its experience may contain lessons for currently developing countries operating under systems which are neither totally free-enterprise capitalism nor socialism.

What then does this book contribute to our understanding on each of these three points? Professor Gregory makes the case that the growth rate from 1885 to 1913 was 3.25% (1.7% per capita), figures which are higher than the estimates of Raymond Goldsmith. The most important factor which accounts for Gregory's higher rate of growth is his assessment that the agricultural sector exhibited a more rapidly growing output than it is usually given credit. The growth rate for national product placed the Russian Empire among the more rapidly growing economies over the period, and in per capita terms it was average.

Gregory estimates that in per capita terms 1928 income was only 80 - 85% of the prewar level. When we attempt to make comparisons with the Soviet period traps abound. The lack of data makes it very difficult to construct the correct national income accounts for the Soviet era. We should start at the Revolution; this starting date would be favourable to the Soviets since output would be temporarily depressed by the dislocations at the end of the war. When it comes

time to estimate national income during central planning our estimates must be qualified by both the questionable quality of the data and questionable quality of the goods. In the early stages of development output is relatively more concentrated in goods which are somewhat homogeneous, food stuffs, cloth, mine output, iron and steel, etc. The essence of economic growth is that it raises the standard of living for consumers, so ultimately we should focus upon the output level of consumer goods over time. The issue of quality becomes steadily more important over time as the desired pattern of expenditure shifts to services and higher quality consumables as consumer income rises. Ultimately to do a correct comparison of economic growth under alternative systems we must consider the market value of consumer goods being produced. It is not enough to compare the output of shoes or foodstuffs without a quality comparison. When an economy is closed such comparisons are practically impossible, but nonetheless must be considered. To be more precise, we must consider what valuation would be placed upon output in the event that trade were reasonably free. The Russian Empire went from reasonably free trade early in the twentieth century to near autarky under the Soviets, and this change may mask systematic biases in the intertemporal valuation of output. The computation of national income must not use the metal-eaters' accounting rules.

Recent events in China, the Soviet Union and elsewhere in the world suggest growing disenchantment with the Soviet model. Centrally planned command economies are effective tools to duplicate a structure of capital goods and production methods which are observed elsewhere. One of the disadvantages of being backward is that there exists a template for development; one only need follow the technological and organizational road mapped by others. This greatly facilitates the recovery from war or civil unrest. The hard part is evolving the economic structure to account for technological change and consumer preferences. Flexibility and dynamism seem lacking in central command economies, guaranteeing perpetual backwardness. One interpretation of the recent moves away from central control in China and the Soviet Union is that, to paraphrase Richard Nixon, «We are all capitalists now». If this is the case, Professor Gregory's book may be the last one written on pre-revolutionary national income in the Russian Empire.

Turning to lessons on economic development which can be learned from the Russian experience, Gregory argues that, based upon his estimates of a more rapid growth rate over the period 1885 - 1913, Russia conformed to the modern mould of economic growth during the last quarter-century of Czarist rule. His more sanguine appraisal of Russian agriculture also leads him to the conclusion that rapid industrialization was not forcibly extracted from the peasants in the sense that they were subjected to privation in order to channel resources to the industrial sector. This is an important conclusion with a bearing not only upon the industrialization process but also upon the economic condition of the peasants on the eve of the revolution. Gregory's work is important if only for this thesis. The one point upon which I would question Gregory is his assertion

that foreign capital played an important role in industrialization. I have argued that in assessing the importance of foreign capital one must not follow the flow of funds, but rather the flow of goods. By seeking to amass a stock of specie which would stabilize the ruble at a fixed rate with gold, Russia in effect exported goods and imported money. Gold being an unproductive asset in manufacturing, this served to retard growth. The import of capital goods instead of accumulating gold would have had a more salutary effect upon development. One cannot be too critical of the Russians for fixing upon gold and pegged exchange rates inasmuch as Great Britain made an even larger mistake of this sort after World War I. Gregory's Appendix Table M.1. shows the consistent pattern of trade surpluses to which I allude. We must remember that «capital» consists of goods used as factors of production, and these trade surpluses show that foreign capital was unimportant in enabling the Russian Empire to import more than it exported and so finance growth. It would likely be more accurate to point to the import of foreign entrepreneurial skill than of foreign capital as a constructive factor in economic development. This entanglement in the veil of money is the only flaw I could detect.

Professor Gregory is due a lot of credit for producing fine narrative detailing his most impressive research agenda. This is an important book and deserves a place on the shelf of every historian and student of Russia. Only an opening of the Soviet archives would produce a work which would eclipse this monograph.

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M.C. HOWELL, *Women, Production, and Patriarchy in Late Medieval Cities*, Chicago, The University of Chicago Press, 1986.

The major empirical focus of this book is upon the economic roles of women in the cities of Leiden and Cologne from about the fifteenth through the mid-sixteenth centuries. The author presents clearly and argues cogently for a major thesis and several corollary but important and intellectually provocative lesser theses. Specifically, Martha Howell contends that:

Women acquired high-status positions in market production in Leiden and Cologne because the family economy was directly involved in production for the market, and they later lost them because the kind of work that could confer high labor-status shifted from the family production unit to the individual. This shift occurred as further development of market production intensified changes in the political and economic organization of work (p. 161).

By «family production unit» Howell intends a family economy that «produced goods and services for sale» rather than one that laboured for mere subsistence or whose members only were wage earners (pp. 28-29). By «labour status» she means «the degree to which a person's role in economic production

grants access to resources of production and distribution». Those with high labour status «independently obtain their own raw materials and supplies ... and control the distribution of the products of their labor» (pp. 23-24).

Howell develops and supports this thesis with a nuanced and challenging presentation that relates empirical findings to major historiographic and theoretical issues. Throughout the book she makes one aware of the relations between her own methodologies and findings and those of other researchers.

«Women's Work in Medieval Cities of Northern Europe» (Part 1) sets for the study's theoretical framework and objectives, and presents Howell's principal hypotheses (p. 43). This section includes a valuable exposition of the differences between northern European and Italian families and household economies. Here, too, we learn that Howell will challenge the belief of «many Marxist and feminist historians that capitalism diminished women's roles in economic production by diminishing the family's role in market production» (p. 32).

«Empirical Studies: Leiden and Cologne» (Part 2) contains the bulk of the data and the analyses upon which Howell's conclusions rest. In each case she examines first the socioeconomic and political structures of the city and then «Women's work in ... Market Production». She has characterized the economic and social structure of Leiden as being not capitalistic, but rather as «small market production», a separate alternative both to medieval craft production and to capitalism (described on pp. 37-39). She found that in Leiden women in the middle stratum of society, the small merchants and producers, attained high-status positions even in the principal economic sector, the drapery or production of heavy English wool cloth. They only lost those positions when economic and political changes favoured the individual producer over the family production unit. Howell's analyses include comparisons between the work and roles of urban and suburban women and of married, widowed, and single women. She offers the intriguing possibility that frequently single women did low status or unskilled work «not because they were unmarried but simply because they were unhealthy, poor, unintelligent, untrained, very old or very young» (pp. 86-87). Those women who attained high-status positions often did so through their families, where they learned trades or were their husband's partners or heirs. Once, however, a trade attained political status, women's roles in it declined, because politics were a male preserve. Howell's study of the more complex economy of Cologne and of women's places in it is even more stimulating and nuanced. In the Rhineland city she found women's guilds, women who were small merchant-producers, and a «higher labor-status for Cologne's women than for Leiden's, (that) can be attributed to capitalism or to the economic and social climate which forested it» (p. 155).

Howell finds further support for her theses in her very short comparative studies of the diverse towns of Lier (near Antwerp), Douai, one of the early major Flemish wool producing centres, and Frankfurt am Main. The final chapter, «Women's Work and Social Change», (pp. 174-183) considers causes

for the different patterns of women's» work other than labour status and economic change. Howell examines in turn «cyclical and economic factors», «the traditional sexual division of labour» and «the connection between law and changes in women's labor status». In the closing pages of text Howell proposes that «Although women held high labor-status in the European market economy only for a short time ... they must have threatened the patriarchal character of Europe's sex-gender system during the period they held it» (p. 181). This threat, she suggests, impelled men to arrange for women's dismissal from trade (p. 183).

This is a tightly written, intellectually provocative book worthy of attentive reading. It is particularly distressing that the University of Chicago Press placed the notes, many of them discursive, at the end of the volume rather than at the bottom of the pages.

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P.E. MALCOLMSON. *English Laundresses: A Social History, 1850-1930*. University of Illinois Press, Chicago and Urbana, 1986. 163 pp. plus notes, bibliographic essay, and index.

In the Victorian imagination, the washerwoman was the antithesis of the Respectable Woman. She was half-clothed, foul-mouthed, bedraggled, hard-drinking, and fiercely independent. There was, Patricia Malcolmson concludes, much truth in this characterization. From Malcolmson's perspective, however, this is not a negative stereotype. In *English Laundresses: A Social History, 1850-1930*, Malcolmson has drawn a detailed and fascinating portrait of washerwomen and the industry in which they laboured. This study joins other recent publications by feminist historians that have illuminated the life and work of Victorian women.

The history of Victorian laundresses has none of the features that generally attract the attention of historians. Laundresses had neither a great leader nor a great strike. They were not important players in the great labour movement of late Victorian and Edwardian England. Their history has neither an obvious beginning nor a logical end. Yet Malcolmson has done an exemplary job of mining the sources and imposing an order on the data.

The number of professional laundry workers in England grew in the last half of the nineteenth century to serve the needs of urbanized Victorians, who no longer had the space or the inclination to do laundry in their homes. Working-class, as well as middle-class Victorians increasingly «sent out» the family wash. In 1901, the number of laundry workers peaked at 205,000.

Laundry work was important to a particular group of Victorian women. It was, along with charring, the only Victorian occupation that was dominated by married women and widows. The wages earned by female laundry workers were

often a critical portion of working-class family incomes, especially when the male wage-earner was seasonally unemployed. When the husband was unable or unwilling to work, the laundress was the sole support of her family. And, so critical was the occupation for widows that the *Pall Mall Gazette* noted in 1894 that «widows and washing, misery and mangles seem, somehow, indissolubly connected» (p. 19). The legendary independence of laundresses was captured in the cryptic comment of one: «A shilling you earn yourself is worth two given you by a man» (p. 43).

The coming of mechanized laundries in the early twentieth century reduced the positions available in the industry, but eased the work for those who filled them. By 1930, the laundry industry had been transformed into a safe, clean, well-regulated modern enterprise. In an interesting «Epilogue», Malcolmson notes that further mechanization changed laundering in an unusual way. The increasing availability of automatic washing machines and dryers eventually made commercial laundries obsolete. «Laundry work is a rare, possibly unique example of a traditional domestic task moving back to the home after its processes had become industrialized» (p. 158).

Malcolmson's best chapters deal with «Hand Laundry and the Family Economy», and «Mechanization and Social Change». Other chapters discuss the attempts to gain protection of the Factory Acts for the industry, and the struggle for unionization. The book also includes a useful bibliographic essay.

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D. REID, *The Miners of Decazeville. A Genealogy of Deindustrialization*. Cambridge, Mass.: Harvard University Press, 1985. pp. 333.

The rear dust jacket lists as books «of related interest» Scott's *Glassmakers of Carmaux*, the Tillys' *Rebellious Century* and Bezucha's *Lyon Uprising*. Although less original and path-breaking, the book inside the jacket stands comparison with these works as a major contribution to the new French labour history. At the core of the miners' story, as Reid tells it, is work process and the attempts of the company to seize control of it from the workers and manage labour from the top. It is this struggle over control of work itself that becomes the underlying cause of myriad forms of industrial and political conflict between workers and company.

The book is arranged essentially chronologically in eight chapters that stretch from the struggles of peasant miners of the eighteenth century, resisting the intrusion of large capitalist firms backed by mining concessions granted by the state, to the final struggle of the employees of a nationalized industry, protesting the state's decision to close their mine. Reid focuses on the nineteenth century; chapters two through six treat the years 1820 to 1914, the ground covered in his 1981 dissertation. But the decision to expand his coverage was

both wise and brave. Few studies of French history cross the archival and pedagogical boundary set by the Revolution and still fewer nineteenth century economic or social historians have shown much predilection for venturing very far into the uncharted waters of the twentieth century. In this book we have the rare satisfaction of feeling that we have begun at a real beginning and followed through to a real ending.

Each chapter typically begins with a description of economic conditions over a few decades. A few words about the French economy at large and a few more about the coal industry lead into a discussion of changing markets and competitive conditions imposed on a company mining a low-cost but inferior grade of coal in an isolated pocket of «an economically disadvantaged region», and this in turn leads finally to an analysis of the structure and strategy adopted by the company in the face of this shifting array of external opportunities and constraints. These slices of conventional economic and business history, focusing as they do on the travails of an atypical French mining company in an atypical French industry, are no substitute for the full-scale history of the French coal industry we still lack or the history of French business we are gradually piecing together. They are, however, well done and well worth the attention of economic and business historians.

Each chapter then moves on from the company's response to economic conditions to the workers at work. «The triumph of industrial capitalism» at Decazeville produced an apparent paradox, the hegemony of the work crew in the work place. Reid's account of the chief hewer's control of hiring, training, paying and assigning work roles to his crew members and his independent negotiation of piece rates with company management bears a superficial resemblance to many recent studies that have shown how early industrial workers had taken traditional patterns of work from their craft and shop origins with them into the factory. But, of course, the mine was not a factory and the miners were not former craftsmen. The organization of work at Decazeville was not so much an anachronistic holdover from a pre-capitalist, communal or corporative golden age of independent artisans as it was a new configuration imposed on company and worker alike by the conditions of large-scale capitalist mining in an age of rudimentary management. «In coal mines, technical constraints and the concessionary system created a situation in which relatively autonomous crews worked within a hierarchical administrative structure» (p. 24). Under pressure to cut overall production costs in the face of a tendency for real wages to rise while extraction conditions grew ever more difficult as the more accessible coal was mined out, successive managements moved step by step to limit the autonomy of the crew and replace the personal authority of its head with supervision by specialized employees backed by company authority. Reid's clear, sensitive and nuanced account of this complicated and difficult process is good labour history, no doubt, but it is also his most original contribution to economic and business history. Work is, or should be, a central and common concern of historians of technology, labour and the economy, and only coop-

erative effort involving all three disciplines will enable us to fully understand its history.

In the final sections of each chapter, Reid shows how the company's rational attempts to control work and the workers rational struggle to maintain the status quo — a «defensive» but not «traditional» posture — led to an ever changing and broadening succession of challenges and responses. Step by step, purely local strikes for control of the mine and local elections for control of the town gave way to industrial and political battles fought under the aegis of national ideologies and organizations and in the spotlight of national media. Reid has used family papers and other sources to sketch fine portraits of several individual company managers and union leaders. And he uses the marriage registers to provide quantitative data on geographic and social origins, occupational inheritance and «networks of sociability» that enrich his portrait of the miners in action.

We conclude each chapter with the realization that an organizational scheme has become a methodology and interpretation in which «work process» rather than «class struggle» is the mediating link between economy and politics. This rather than the «genealogy of deindustrialization» is Reid's theme. In fact, the perfunctory treatment of deindustrialization has the feel of a recent addition, a stretch for relevance, rather than an integral theme of the work as a whole. Much more research and hard thinking will be necessary before we can decide whether deindustrialization should join proto-industrialization as an important new category of historical analysis.

*The Miners of Decazeville* is a mature work in what is now a mature genre. In the inevitable comparison with Rolande Treppe's two dense volumes on *Les mineurs de Carmaux (1848-1914)*, Reid's slim volume both suffers and gains from its choice of coverage in breadth rather than depth, but his own depth of research cannot be faulted; he has used an immense variety of likely and not so likely sources. His acknowledgements to a dozen or more eminent friends and colleagues for reading drafts of his manuscript reveal the extent to which his book reflects current thinking in the now dominant school of labour history. Those of us who do not know Donald Reid and do not hold full membership in that school will be the losers if we do not also read his fine book.

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J. STUART WOOLF. *The Poor in Western Europe in the Eighteenth and Nineteenth Centuries*, London, Methuen, 1986.

Readers must be warned that the title of this book does not describe its contents accurately. Of the eight chapters (seven of which are revised versions of articles originally published elsewhere) only one deals with the poor outside

Italy and a second one describes the victims of poverty in an Italian region other than Tuscany during the Napoleonic era. The other six chapters focus on specific examples of aspects of poverty drawn from surveys and censuses of the poor ordered by the Napoleonic governing authorities during the early years of the nineteenth century. Woolf evaluates the reliability of the Napoleonic era statistics and then uses them to evaluate and analyse such subjects as vagrancy and begging; job skills in Florence; private and public charity; and the relationship between poverty and household structure (chs. 3-8). Woolf, moreover, discloses how in early nineteenth-century Tuscany poverty struck hardest at the youngest and oldest age cohorts, and how charitable institutions sought to teach most boys a useful craft, render girls more marriageable through the provision of dowries, and confine the indigent and usually infirm aged to hospitals. For anyone interested in the extent of poverty in early nineteenth century Tuscany, how public and private institutions responded to the poor, and how the poor dealt with their misery, this is a valuable book.

Woolf's first and most general chapter surveys the history of poverty and charity in the broadest fashion. Using older scholarly studies and the more recent cornucopia of publications concerning ordinary people and everyday life, he limns the changing aspects of poverty across time from the medieval era to the industrial age in Western Europe. This survey of the subject concludes that the state of the economy and the labour market determined the extent of poverty; that, almost from time immemorial, charitable benefactors distinguished between the deserving and the undeserving poor; that not until well into the nineteenth century did charity give rise to shame among its recipients; that the nineteenth century also saw the emergence of the friendly visitor programme everywhere among charitable *bourgeois* women in order to separate better the respectable from the disreputable poor; and that historically the methods devised to succour the poor customarily had the same objective: «to ensure deferential acceptance of the existing social order (p. 40)». All this should surprise few students of the subject.

Readers of Woolf's book, however, may be more surprised by some of his findings concerning the history of poverty in Italy. He leaves no doubt, for example, that he believes that poverty increased during the transition from medieval to modern society partly as a result of the decline of subsistence agriculture and partly as a consequence of excess population pressing on scarce resources (here temporary and permanent migration acted as safety valves for the poor). Woolf also believes that a discernible line separated the poor from the working class. Although he concedes that most of the poor most of the time slipped in and out of poverty, Woolf nevertheless asserts that the poor ordinarily fell victim to a «culture of poverty» in which they developed a deferential mentality that doomed them to a life of dependency. The working class, by contrast, acted for itself, forming organizations, striking, and acting politically. Yet Woolf does sometimes make the line between the poor and the working class more permeable, as, for example, when he links poverty to the life cycle of

the family. For, even among the families of stable workers, poverty invariably followed births too closely spaced, illness, and the approach of old age. One thing, however, remained constant, the philosophy and practice of poor relief. «The aim,» in Woolf's words, «was to provide that minimum of assistance which would keep the deserving poor from death by starvation or cold, while making conditions so uncomfortable for the able-bodied poor that they would be induced to work... But, whatever the intent, the structure of poor relief probably rarely proved adequate to meet the need (p. 111)».

This book, then, is one more example of that old truism that the more things change the more they remain the same. Whether in the medieval or the modern eras, in Western Europe or North America, the poor, according to Woolf, are always with us. And their «social betters» just as persistently separate them into deserving and undeserving categories and dole out public and private charity primarily to control the poor.

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